THE

AMERICAN NATURALIST,

An Illustrated Magazine

OF

NATURAL HISTORY.

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VOLUME XXX.

PHILADELPHIA, U. S. A.

THE EDWARDS & DOCKER CO.,

518 & 520 MINOR STREET,

1896.

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THE

AMERICAN NATURALIST

VOL. XXX. January, 1896. 349

ON HEREDITY AND REJUVENATION.[1]

BY CHARLES SEDGWICK MINOT.[2]

The subject of this article is presented under the following sections:

I. The Formative Force of Organisms.

II. The Conception of Death.

III. A Comparison of Larva and Embryo.

IV. Concluding Remarks.

The first section is not new, but a reproduction without change, of an article published in *Science*, July 3d, 1885. As this article has not become generally known, and yet is an essential link in the chain of reasoning, I venture to repeat it. Though written in 1885, I consider that to-day it is still sufficient to disprove Weismann's theory of germ plasm. Weismann has not considered this article, otherwise, from my point of view, he could not have maintained his theory.

[1] This article is translated from one which appeared in the Biologisches Centralblatt, Vol. XV, Page 571, August 1st, 1895. A few trifling changes have been made in the text. An abstract of the article was read before the American Association for the Advancement of Science, at its recent Springfield meeting.

[2] Professor in the Harvard Medical School.

The views which I then defended have been recently brought forward in almost parallel form, and without essential additions, by O. Hertwig (*Zeit-und Streitfragen der Biologie*, I, Heft, D. 32–53) as arguments against the views of Weismann.

The second section is also directed against Weismann, for it attempts to replace his conception of death by one more exact.

The third section is intended to make the significance of rejuvenation clear, and at the same time, by a comparison of larvæ and embryos, to demonstrate a law of heredity which has not been hitherto recognized.

The Formative Force of Organisms.

The assertion is safe, that the majority of biologists incline at present to explain the forming of an organism out of its germ upon mechanical principles. The prevalent conception is that the forces of the ovum are so disposed that the evolution of the adult organism is the mechanical result of the predetermined interplay of those forces. The object of the present article is to point out that this conception is inadequate, and must be at least supplemented, if not replaced, by another view, namely, that the formative force is a generally diffused tendency, so that all parts inherently tend to complete by their own growth and modification the whole organism—a fact which finds a legitimate hypothetical expression in Darwin's Doctrine of pangenesis. The nature of the view here advanced will become clearer upon consideration of the evidence upon which it is based, and which is adduced below. The evidence that the formative force is diffused through all parts falls under three heads: 1. The process of regeneration in unicellular and multicellular bionts; 2. The phenomena of of the duplication of parts; 3. All forms of organic reproduction. Let us briefly consider these categories.

1. *Regeneration.*—All living organisms have, to a greater or less degree, the ability to repair injuries; indeed, we must regard the power of regeneration as coextensive with life, but

the capacity varies enormously in the different species. In man the power is very small, though more extensive than is generally realized. Among Annelids are species, the individuals of which may be divided in two, and each piece can regenerate all that is needed to render it a complete worm. We sometimes see a small fragment of a plant, a single switch of a willow, for instance, regenerate an entire tree, roots, trunk, branches, leaves, flowers, and all. In the last instance a few cells possess a latent formative force, which we recognize by its effects, but cannot explain. We perceive, therefore, that each individual has, as it were, a scheme or plan of its organization to which it strives to conform. As long as it actually does so, the cells perform their routine functions; but when an injury destroys or removes some portion, then the remaining cells strive to conform again to the complete scheme, and to add the missing fragment. The act of regeneration of lost parts strikes the imagination almost as an intelligent pursuit by the tissues of an ideal purpose.

Our knowledge of the regeneration power has recently received important extensions through the noteworthy experiments of Nussbaum[3] and Gruber,[4] who have demonstrated, independently, the possibility of dividing unicellular animals so that each piece will regenerate the missing parts. In this manner the number of individuals can be artifically multiplied. For example: Nussbaum divided a well-isolated Oxytricha into two equal parts, either transversely or longitudinally, and found that the edges of the cut became soon surrounded with new cilia. Although some of the substance of the body, or even a nucleus, was lost through the operation yet, by the following day, the two parts converted themselves into complete animals with four nuclei and nucleoli (*Nebenkerne*) and the characteristic ciliary apparatus. "The head piece has formed a new hind end; the right half, a new left half." The

[3] M. Nussbaum, *Ueber spontane und kunstliche Zellteilung*, Sitzungsb. d. neiderrh. Ges. f. Nat. u. Heilkunde, Bonn, 15, Dez., 1884.

[4] A. Gruber, *Ueber kunstliche Teilung bei Infusorien*, Biol. Centralblatt, Bd. IV, No. 23, 717–722.

newformed duplicate Infusoria multiplied subsequently by spontaneous division. From one Oxytrachia cut in two, Nussbaum succeeded in raising ten normal animalcules, which subsequently all encysted. After an unequal division, the parts are both still capable of regeneration, but parts without a nucleus did not survive, which suggests that the formative energy is in some way bound up with the nucleus. But nucleate pieces may break down. Thus, all attempts at artificial multiplication of the multinucleate Opalina failed, although the division of Actinosphærium had been successfully made by Eichhorn as long ago as in the last century. Pelomyxa palustris has been successfully divided by Greef, and Myastrum radians by Haeckel.

Gruber (*l. c.*, p. 718) describes his experiments with Stentor: "If one divides a Stentor transversely through the middle, and isolates the two parts, one finds on the cut surface of the hind part, after about twelve hours, a complete peristomial field with the large cilia and buccal spiral newly formed. On the other hand, the piece on which the old mouth is situated has elongated itself backwards, and attached itself in the manner peculiar to these Infusoria. If one has made a longitudinal section, so that the peristom is cut in two, then the peristoms both complete themselves and the lateral wounds heal over. I have repeatedly separated, by transection, pieces considerably less than half of the original Stentor, and these have also regenerated themselves to complete animals." Gruber, too, observed that artificially divided Infusoria were capable of subsequent spontaneous multiplication. If the section is not very deep, there may arise double monsters; but here, just as in spontaneous divisions, as long as there remains an organic connecting band, the two parts act as one individual, showing that the nervous actions are not restricted to determined paths. Gruber also adds that two divided pieces may be reunited if brought together quickly enough. The observation thus briefly announced is of such extreme interest and importance that the publication of the full details of the experiment will be eagerly awaited. Gruber adds that at present we can-

not go much beyond the proof of existence, to a high degree, of the regenerative capacity in unicellular organisms, He also makes the significant observation that in the Protozoa, we have to do foremost with changes of function; in the Metazoa, with growth also.

2. *Duplication of parts.*—In these anomalies we find an organ which, although an extra member, yet still conforms to the type of the species. For example: a frog is found with three posterior limbs; dissection proves the third leg to agree anatomically with the typical organization of the frog's hind leg. In determining the importance to be attributed to this evidence, it should be remembered, on the one hand, that these instances are by no means unusual; on the other, that the agreement with the normal structure is not uniform.

3. *Asexual reproduction.*—When a species multiplies by fission of any kind, we must assume that each part, after division, possesses the formative tendency, since we see it build up what is necessary so complete the typical organization of the individual. Again: a bud of a hydroid or polyzoon, although comprising only a small part of the body, is equally endowed with this uncomprehended faculty. In pseudova we reach the extreme limit; in aphis, for example, the parent gives off a single cell, the capacity of which, to produce a perfect and complicated individual, fully equals the like capacity of a hydroid bud or of half a worm.

The evidence forces us to the conclusion that the formative force or cause is not merely the original disposition of the forces and substances of the ovum, but that *to each portion of the organism is given:* 1. *The pattern of the whole organism;* 2. *The partial or complete power to reproduce the pattern.* The italicized formula is, of course, a very crude scientific statement, but it is the best which has occurred to me. The formative force, then, is a diffused tendency. The very vagueness of the expression serves to emphasize our ignorance concerning the real nature of the force. In this connection, I venture to insist upon the fact that we know little or nothing concerning any of the fundamental properties of life, because I think the

lesson of our ignorance has not been learned by biologists. We encounter, not infrequently, the assertion that life is nothing but a series of physical phenomena; or, on the other hand, what is less fashionable science just now, that life is due to a special vital force. Such assertions are thoroughly unscientific; most of them are entirely, the remainder nearly worthless. Of what seems to me the prerequisites to be fulfilled before a general theory of life is advanced, I have written elsewhere.[5]

II. Conception of Death.

My thesis reads: There are two forms of death. These are *first*, the death of the single cells; *second*, the death of multicellular organisms. Death in the one case is not homologous with death in the other.

Weismann assumed the complete homology of the two forms of death. Without this assumption, his hypothesis of the immortality of unicellular organisms falls to the ground and with it falls the entire superstructure of his speculations upon germ plasm. Oscar Hertwig (*Zeit und Streitfragen, Heft* 1) has already expounded, very clearly, the dependence of the theory of germ plasm upon the hypothesis of unicellular immortality; it would, therefore, be superfluous to discuss it here.

The conception of the biological problem of death, to which I still hold, was formed several years before Weismann's first publication, which appeared in 1882, with the title, "*Ueber die Dauer des Lebens.*" He has further defended his view in his article, "*Ueber Leben und Tod*" (1884), and has steadfastly adhered to it since. In the years 1877–1879 I published my theoretical interpretation of the problem.[6] This interpretation became the starting point of elaborate special investigations, by which I endeavored to advance the solution of the problem and, in fact, observation and experiment have confirmed the

[5] C. S. Minot, *On the conditions to be filled by a theory of life,* Proc. Amer. Assoc. Adv. Sc., XXVIII, 411.

[6] Proc. Boston Soc. Nat. Hist., XIX, 167; XX, 190.

original thesis.[7] Moreover, in an especial short article I have directed attention to the fact that Weismann has not considered the essential issue of the problem. The difficulties pointed out still remain, and, according to my conviction, cannot be removed. Weismann passes these difficulties by and carries out his speculations without first securing a basis for them. His method is illustrated by the following quotation: "I have, perhaps, not to regret that I cannot here discuss the article referred to (Minot's Article in *Science*, Vol. IV, p. 398); nevertheless, almost all objections which are there made to my views are answered in the present paper." (Weismann, Zur Frage nach der Unsterblichkeit der Einzelligen, Biol. Centralbl., IV, 690, Nachschrift). I have studied the paper with conscientious care and cannot admit that the objections have been answered. On the contrary, I maintain now, as formerly, the judgment: "He misses the real problem." For this reason I hold it to be unnecessary to discuss the details of Weisman's exposition, because—if I am right—he has not considered the actual problem of death at all. "He misses the real problem." The following reasoning leads to this decision: Protozoa and Metazoa consist of successive generations of cells; in the former the cells separate; in the latter they remain united; the death of a Protozoa is the annihilation of a cell, but the death of a Metazoon is the dissolution of the union of cells. Such a dissolution is the result of time, that is to say, of the period necessary to the natural duration of life, and we call it, therefore, "*natural death.*" Moreover, we know that natural death is brought about by gradual changes in the cells until, at last, certain cells, which are essential to the preservation of the whole, cease their functions. Death, therefore, is a consequence of changes which progress slowly through successive generations of cells. These changes cause senescence, the end of which is given by death. If we wish to know whether death, in the sense of natural death, properly so called, occurs in Protozoa or not, we must first pos-

[7] Journal of Physiology, XII, and Proc. A. A. A. S., XXXIX, (1890).

sess some mark or sign, by which we can determine the occurrence or absence of senescence in unicellular organisms.

Around this point the whole discussion revolves. Certainly a simpler and more certain conclusion could hardly be drawn than that the death of a Metazoon is not identical, *i. e.*, homologous with the death of a single cell. Weismann tacitly assumed precisely this homology, and bases his whole argument on it. In all his writings upon this subject, he regards the death of a Protozoon as immediately comparable with the death of a Metazoon. If we seek from Weismann for the foundation of this view we shall have only our labor for our pains. Starting from this view Weismann comes to the strictly logical conclusion that the Protozoa are immortal. This is a paradox! In fact, if one compares death in the two cases, from Weismann's standpoint, then we must assume a difference in the causes of death, and conclude that the cause in the case of the Protozoa is external only, while in the Metazoa it is internal only, for, of course, we may leave out of account the accidental deaths of Metazoa. If we approach the problem from this side, we encounter the following principal question: Does death from inner causes occur in Protozoa? Weismann gives a negative answer to this question, with his assertion that unicellular organisms are immortal. The assertion remains, but the proof of the assertion is lacking. In order to justify the assertion, it must be demonstrated that there does not occur in Protozoa a true senescence, showing itself gradually through successive generations of cells. Has Weismann furnished this demonstration? Certainly not. He has, strictly speaking, not discussed the subject. It is clear that we must first determine whether natural death from senesence occurs in Protozoa or not, before we can pass to a scientific discussion of the asserted immortality of unicellular beings. The problem cannot be otherwise apprehended. Weismann has not thus conceived it, therefore the judgment stands against him: *he misses the real problem.*

Senesence has been hitherto little investigated; for many years I have been studying it experimentally and have tried

to determine its exact course. My paper, "Senesence and Rejuvenation," affords evidence of new facts proven by these experiments. I believe I have thus won the right to oppose my view to the pure speculations of Weismann.

(*To be continued.*)

LOST CHARACTERISTICS.

By Alpheus Hyatt.

Dr. Minot having noticed, in the translation of his article "On Heredity and Rejuvenation," an accidental omission of quotation of work done by paleontologists on the loss of characteristics in the development of animals, has most courteously asked me to follow his essay by an article dealing with this question. I gladly avail myself of this opportunity on account of the advantages offered where similar subjects can be consecutively treated from different points of view, and because Dr. Minot's article, on account of his great and deserved reputation in embryology, will reach the students of existing biological phenomena, and perhaps induce some of them to read a connected publication.

The loss of characteristics is not so readily observed by a student of the biology of existing animals or neobiologist, as by the paleobiologist or student of fossils, because the latter necessarily deals with series of forms often persisting through long periods of time, and is led, especially if he follow more recent methods of research, to study these in great detail. The observer of these remains is not, as is falsely imagined, limited to fragments, but can and does work out of the hard matrix the external skeletons or shells even of embryos, and can, in the corals, brachiopoda, mollusca, echinodermata and even in protozoa, follow the entire life history of these parts in the individual. He has also the further advantage of availing himself of the knowledge amassed by the neobiologist and neoembryologist, the works of Cope, Beecher, Schuchert, Gurley,

Jackson and others, written in the last thirty years in this country and in Europe. The new school of Paleobiology also insists upon the close study of series of forms and rejects the methods usually pursued by the neoembryologist, who, as a rule, selects his objects of study and pursues his comparisons upon the old basis of comparative anatomy and with but little regard to the serial connections of forms. The importance of studying the seriality in structure of the members of the same group, those gradations, which lead from one variety to another, one species to another, one genus to another, until they may end in highly differentiated and often degraded offshoots, with as strange and unique developments as they have adult characters, seems not, as yet, to have attracted the attention of the students of development among recent animals as it has that of paleobiologists. The prevalent modes of study of living types has consequently led to noticing the pheuomena of omission of hereditary characters only in an isolated way, and from the time of Balfour's "Comparative Embryology" these omissions occurring in the embryo have been named abbreviations, shortenings and omissions of development, and various attempts have been made to explain them upon more or less general grounds of inference. Prof. Cope and the writer and some other authors have been for a number of years publishing observations upon this class of phenomena under the title of the law of acceleration, asserting that in following out the history of series in time, or of existing series in structure, there was observable a constant tendency in the successive members (species, genera, etc.) of the same natural group to inherit the characters of their ancestors at earlier stages than those in which they appeared in these ancestors. That as a corollary of this tendency, the terminal forms eventually skipped or omitted certain ancestral characteristics, which were present in the young of the preceding or normal forms of the same series. and also in the adult stages of development of more remote ancestors of the same genetic stock or series. This law has since been independently rediscovered by several other naturalists, notably Würtemburger in Germany, and Buckman in England. The writer has lately christened this as the law of

Tachygenesis[1] in allusion to the general character of the phenomena.

In a late paper,[2] the writer reviewed Prof. Cope's and Haeckel's views of this law, and contrasted them with his own, and it seems advisable to give these remarks again in this connection.

Professor Cope has given the fullest explanation of this law, but has joined it with retardation. Thus, from his point of view, if I rightly understand him, inexact parallelism in development or failure to reproduce any hereditary characteristics is due to a tendency which appears in organisms and works in parallel lines with acceleration, the law being in his conception of a double nature. Thus he says, on page 142 of his "Origin of the Fittest," "The acceleration in the assumption of a character progressing more rapidly than the same in another character, must soon produce, in a type whose stages were once the exact parallel of a permanent lower form, the condition of inexact parallelism. As all the more comprehensive groups present this relation to each other, we are compelled to believe that acceleration has been the principle of their successive evolution during the long ages of geologic time. Each type has, however, its day of supremacy and perfection of organism, and a retrogression in these respects has succeeded. This has, no doubt, followed a law the reverse of acceleration, which has been called retardation. By the increasing slowness of the growth of the individuals of a genus, and later and later assumption of the characters of the latter, they would be successively lost. To what power shall we ascribe this acceleration by which the first beginnings of structure have accumulated to themselves through the long geologic ages, complication and power, till from the germ that was scarcely born into a sand lance, a human being climbed the complete scale, and stood easily the chief of the whole." And again, on page 182 of the same work: "Acceleration signifies addition to the number of those repetitions during the period

[1] "Phylogeny of an Acquired Characteristic." Proc. Am. Phil. Soc. Philadelphia, XXXII, No. 143.

[2] "Bioplastology and the Related Branches of Biologic Research." Proc. Bost. Soc. Nat. Hist., XXVI, p. 77–81.

preceding maturity, as compared with the preceding generation, and retardation signifies a reduction of the numbers of such repetitions during the same time." Thus, from Cope's point of view, tachygenesis is the law of progression, and retardation is the law of retrogression, and they are both essential parts of his law of acceleration and retardation.

Haeckel alludes in general terms to the law of abbreviated development in his " Morphologie der organismen," and in his "Anthropogenie," published in 1874, substantially agrees with Cope in his view of the law and uses the term " palingenesis " for the exact repetition of characteristics which occurs in the earlier and simpler forms of a phylum and " coenogenesis " for the abbreviated or highly accelerated cases of inexact parellism of the young of more complex forms with their ancestors. There is, however, an objection to this mode of using the last term which I mentioned also in writing the paper quoted.[3]

[3] During the writing of this paper I took from Cope the statement made above, although unable to find any verification of it in Haeckel's Anthropogenie (1st and 2d editions both dated 1874), but, since the above was in press, I obtained a copy of the 4th edition (1891) and the reading of this has caused me to entirely alter my opinion with regard to Haeckel's opinions. He certainly had at that time, 1891, what seems to me erroneous and inadequate view of the nature and action of the laws of tachygenesis and gave it too limited application. He also used the terms, palingenesis and cenogenesis differently from the way in which Cope and others have used them in this country.

Haeckel states (Anthropogenie, 4th edition, Leipzig, p. 9, 1891) that "Palingenetische Processe oder keimesgeschichtliche Wiederholungen nennen wir alle jene Erscheinungen in der individuellen Entwickelungsgeschichte, welche durch die conservative Vererbung getreu von Generation zu Generation übertragen worden sind und welche demnach einen unmittelbared Rückschluss auf entsprechende Vorgänge in der stammesgeschichte der entwickelten Vorfahren gestatten. Cenogenetische Processe hingegen oder keimesgeschichtllche *Störungen* nennen wir alle jene vorgänge in der keimesgeschichte, *whelche* nicht auf solche vererbung von uralten stammformen zurüchführbar, vielmehr *erst spater durch Anpassung der Keime oder der Jugendform* an bestimme Bedingungen der Keimesentwickelung hinzugekomen sind. Diese ontogenetischen Erscheinungen *sind fremde zuthaten* welche *durchaus keinen unmittelbaren Schluss anf entsprechende Vorgänge in der stammesgeschichte* der Ahnenreihe erlauben, vielmehr die Erkenntniss der letzteren geradezu fälschen und verdecken."

So far as one can get at Haeckel's oplnions from such expressions as the above it is obvious that he views shortened or abbreviated development in a very distinct light from that to which I am accustomed. He speaks of it as due to the introduction of " fremde zuthaten " as " Cenogenetische oder Störungsgeschichte "

and further to make his meaning clearer, on page 11 he divides cenogenetic phenomena into "Ortsverschiebungen oder Heterotopien," and, on page 12, "Zeitverschiebungen oder Heterochronien." Organs or parts may be developed heterotopically, that is, out of place or in a different part of the body from that in which they originated in the ancestors; or heterochronically, that is earlier in time during the life of the individual than that in which they originated, and he also speaks of the latter as "ontogenetische Acceleration," using exactly the adjective applied in this country many years beforehand, but that fact does not seem to have been considered worthy of his attention. Haeckel then proceeds to add: "Das umgekehrte gilt von der verspäteten Ausbildung des Darmcanals, der Leibeshöhle, der Geschlechtsorgane. Hier liegt offenbar eine Verzögerung oder Verspätung, eine *ontogenetische Retardation.*" This is probably what Cope alludes to in his quotation of Haeckel, and certainly this is a restatement of Cope's law of retardation with, however, the ommission of any reference to the original discoverer. It will be gathered from the text above that I view acceleration firstly, as a normal mode of action or tendency of heredity acting upon all characters that are genetic, or, in other words, derived from ancestral sources; secondly, that a ctetic, or, in other words, a newly acquired character must become genetic before it becomes subject to the law of tachygenesis. Haeckel has evidently confused ctetic characters like those of the so called ovum of Taenia, the Pluteus of Echinoderms and the grub, maggot, caterpillars of insects, which have caused the young to deviate more or less from the normal line of development, as determined by the more generalized development of allied types of the same divisions of the animal kingdom, with the normal characters that are inherited at an early stage in the ontogeny and considers them all as heterochronic. It is very obvious that they are quite distinct and that, while the ctetic characters may have been larval or even possibly embryonic in origin, and may not have affected perceptibly the adult stage at any time in the phylogeny of the group, they are, nevertheless, subject to the law of acceleration and do affect the earliest stages as has been shown in Hyatt's and Arm's book on Insecta. Such characteristics do, of course, contradict the record, if we consider that the record ought have been made by nature according to anthropomorphic standards, and in such misleading phraseology they are falsifications of the ontogenetic recapitulation of the phylogeny. In a proper nomenclature, framed with due regard to natural standards, such expressions are inadmissable. There is absolutely no evidence that characteristics repeated in the younger stages of successive species and types owe their likeness to ancestral characters to other causes than heredity. This likeness may be interfered with or temporarily destroyed by extraordinary changes of habit, as among the larvae of some insecta and the forms alluded to above, or among parasites in different degrees, but the obvious gradations of structures in many of these series show that hereditary tendencies are not easily changed in this way. There are comparatively very few forms having doubtful affinities even among the parasites. It is also evident that the novel larval characters originating in the young in their turn speedily become hereditary and are incorporated in the phylogeny and recapitulated in the ontogeny.

It may be seen from this that in dividing tachygenesis into palingenesis and cenogenesis the writer has followed Cope rather than Haeckel, and there is a seri-

Either through want of acquaintance with good examples of retardation or because of a different point of view, I have not been able to see any duplex action in the law of acceleration. To me it is the same law of quicker inheritance which is acting all the time in the phylum at the beginning, middle, and end of its history, as will be seen by the explanation given above. In Insecta[4] I have tried to apply it to the explanation of the peculiar larval forms of those animals which often present retrogression through suppression of ancestral characters in the young, although their adults are perfectly normal and perhaps progressive. Consequently, palingenesis and coengenesis are, from my point of view, simply different forms of tachygenesis, and there is no boundary or distinction between them. In other words, retardation or retrogression occurs because of the direct action of tachygenesis upon more suitable and more recently acquired characteristics which are driven back upon and may directly replace certain of the ancestral characters causing them to disappear from ontogenetic development.[5]

ous objection to the use of cenogenesis at all, since it is from *Κενός* meaning strange, and was first applied by Haeckel in such a way that both by his statements, and the derivation, it ought to be confined to types like larvae of the Echinodemata Insect, etc., and parasites in which acquired characters do interfere with the ontogenic recapitulation for a certain time. Normal types, in which tachygenesis occur in a marked way might be called tachygenetic. Palingenesis and palingenetic might be confined to generalized forms in which the ontogeny was a more or less prolonged recapitulation of the phylogeny, and coenogenesis would thus be properly confined to its original field wherever ctetic characters were introduced. This would avoid the need of using a new term.

[4] Guides for Science Teaching, Boston Soc. Nat. Hist., No. 8.

[5] Specialization by reduction of parts is evidently included under the head of retardation by Cope; thus in Origin of the Fittest, p. 353, he says that "change of structure during growth is accomplished either by addition of parts (acceleration) or by subtraction of parts (retardation)." So far as my experience goes in the major number of cases, the parts of characters that are undergoing reduction disappear according to the law of tachygenesis. They reappear in the ontogeny at earlier and earlier stages, or exhibit this tendency in the same way as characters of the progressive class, but their development is not so complete as in ancestral forms. In this sense they can be regarded as retarded or thrown back in their development. There is, however, another way of formulating the expression for this. Instead of regarding this disappearance by retrogressive gradations as due to a tendency opposed to acceleration, is it not a tendency of the same

The law of tachygenesis as defined by the writer acts upon all characteristics and tendencies alike, and is manifested in genetically connected phyla by an increasing tendency to con-concentrate the characteristics of lower, simpler, or earlier occurring, genetically connected forms in the younger stages of the higher, more complicated or more specialized, or more degraded, or later occurring forms of every grade, whether the characteristics arise in adults or in the younger stages of growth. Since my first publication in 1866, the law has become clearer to me, but I have made no fundamental change in the conception. The application of the law to degenerative characteristics appears to me to explain why there are degenerative forms in the phylum which are indicated by the senile stages of the individual.

The degenerative changes of the senile period may, and practically in all cases do, tend to the loss of characteristics of the adult period and consequently in extreme cases bring about not only the loss of a large proportion of progressive characteristics, but loss in actual bulk of the body as compared with adults, as has been stated above. This is usually regarded as due to the failure of the digestive organs or defective nutrition, and this may be true in many examples; but, on the other hand, it often begins in individuals long before there is any perceptible diminution in size, and may occur in dwarfs and in some degenerate species in the early stages, and finally in series of species according to the law of tachygenesis, so that

kind? That is to say, do not the parts and characters show a tendency to disappear earlier and earlier, and are they not, in most cases, at the time of disappearance, present also in earlier stages of growth than that in which they originated in ancestral forms?

Is not the case of the wisdom teeth exceptional? The frequently extremely late external appearance of these is not accompanied by a later origin of their rudiments in the jaw. Although they may not appear in many cases above the gum until a person is past fifty, is not this retardation in becoming externally visible due primarily to the fact that they are deficient in growth power (tending to disappear from disuse, etc.), and secondarily to their internal position. When they cease to be able to break through the gum, will they not still continue to develop at the same stage as the other teeth, and will not their rudiments be likely tobe present at this early stage long after they have ceased developing into perfect teeth?

one is led to believe that the tendency to the earlier inheritence of degenerative modifications producing retrogression is inheritable like the tendency to the earlier inheritance of additional or novel characteristics producing progression. Thus, this law applied to progressive or retrogressive groups explains the mode in which their progression or retrogression is accomplished so far as the action of the laws of genesiology (science of heredity) are concerned.

In the same essay on Bioplastology, the writer reviewed Dr. Minot's law of growth, and in this and in his Phylogeny, quoted above, used it to throw light upon one of the most difficult problems of evolution.

It is a general law of unique importance, as readily observable in the growth of skeletons and shells of all kinds, and therefore as obvious in fossils as in the famous guinea pigs studied by Dr. Minot. This law enabled the writer to get what seemed to him a clearer view of the action of tachygenesis. See Bioplastology (p. 76).

Minot's researches enable one to see clearly that the reduction of parts or characteristics which takes place through the action of the law known as the law of acceleration in development (often also descriptively mentioned as abbreviated or concentrated development) cannot be considered as due to growth.

" It seems probable from my own researches published in various communications, but more especially in the ' Genesis of the Arietidae,'[6] that the action in this case is *a mechanical replacement of the earlier and less useful ancestral characteristics and even parts by those that have arisen later in the history of the group.* We can fully understand the phenomena of acceleration in development only when we begin by assuming that the characteristics last introduced in the history of any type were more suitable to the new conditions of life on the horizon of occurrence of the species than those which characterized the same stock when living on preceding horizons or in less specialized habitats. These new characters would necessarily, on

[6] Smithsonian Contributions to Knowledge, v. 26, p. 40–48, 1889; also, Mus. Comp. Zool., v. 16, 1889.

account of their greater usefulness and superior adaptability, ultimately interfere with the development of the less useful ancestral stages and thus tend to replace them. The necessary corollary of this process would be tachygenesis or earlier appearrance of the ancestral stages in direct proportion to the number of new characteristics successively introduced into the direct line of modification during the evolution of a group.

If this be true, it can hardly be assumed that the loss of characteristics and parts taking place in this way is directly due to growth force. If growth has anything to do with these phenomena, it must act indirectly, and, as in the repetition of other similarities and parallelisms, under the controlling guiddance of heredity.

VARIATION AFTER BIRTH.

BY L. H. BAILEY.

At the present time, our attention is directed to differences or variations which are born with the individual. We are told that variation which is useful to the species is congenital, or born of the union—or the amalgamation in varying degrees—of parents which are unlike each other. From the variations which thus arise, natural selection chooses those which fit the conditions of life and destroys the remainder. That is, individuals are born unlike and unequal, and adaptation to environment is wholly the result of subsequent selection.

These are some of the practical conclusions of the NeoDarwinian philosophy. It seems to me that we are in danger of letting our speculations run away with us. Our philosophy should be tested now and then by direct observation and experiment, and thus be kept within the limits of probability. The writings of Darwin impress me in this quality more than in any other,—in the persistency and single-mindedness with which the author always goes to nature for his facts.

In this spirit, let us drop our speculations for a moment, and look at some of the commonest phenomena of plant life as they transpire all about us. We shall find that, for all we can see, most plants start equal, but eventually become unequal. It is undoubtedly true that every plant has individuality from the first, that is, that it differs in some minute degree from all other plants, the same as all animals possess differences of personality; but these inital individual differences are often entirely inadequate to account for the wide divergence which may occur between the members of any brood before they reach their maturity.

The greater number of plants, as I have said, start practically equal, but they soon become widely unlike. Now, everyone knows that these final unlikenesses are direct adaptations to the circumstances in which the plant lives. It is the effort to adapt itself to circumstances which gives rise to the variation. The whole structure of agriculture is built upon this fact. All the value of tillage, fertilizing and pruning lies in the modification which the plant is made to undergo. Observe, if you will, the wheat fields of any harvest time. Some fields are "uneven," as the farmers say; and you observe that this unevenness is plainly associated with the condition of the land. On dry knolls, the straw is short and the plant early; on moister and looser lands, the plant is tall, later, with long, well-filled heads; on very rich spots, the plants have had too much nitrogen and they grow too tall and "sappy," and the wheat "lodges" and does not fill. That is, the plants started equal, but they ended unequal. Another field of wheat may be very uniform throughout; it is said to be "a good stand," which only means, as you can observe for yourself, that the soil is uniform in quality and was equally well prepared in all parts. That is, the plants started equal, and they remained equal because the conditions were equal. Every crop that was ever grown in the soil enforces the same lessons. We know that variations in plants are very largely due to diverse conditions which arise after birth.

All these variations in land and other physical conditions are present in varying degrees in wild nature, and we know

that the same kind of adaptations to conditions are proceeding everywhere before our eyes. We cannot stroll afield without seeing it. Dandelions in the hollows, on the hillocks, in the roadside gravel, in the garden—they are all different dandelions, and we know that any one would have become the other if it had grown where the other does.

But aside from the differences arising directly from physical conditions of soil and temperature and moisture, and the like, there are differences in plants which are forced upon them by the struggle for life. We are apt to think that, as plants grow and crowd each other, the weaker ones die outright, because they were endowed with—that is, born with—different capabilities of withstanding the scuffle. As a matter of fact, however, the number of individuals in any area may remain the same or even increase, whilst, at the same time, every one of them is growing bigger. Early last summer I staked off an area of twenty inches square in a rich and weedy bit of land. When the first observations were made on the the 10th of July, the little plat had a population of 82 plants belonging to 10 species. Each plant was ambitious to fill the entire space, and yet it must compete with 81 other equally ambitious individuals. Yet, a month later, the number of plants had increased to 86, and late in September, when some of the plants had completed their growth and had died, there was still a population of 66. The censuses at the three dates were as follows:

	July 10.	Aug. 13.	Sept. 25.
Crab grass (Panicum sanguinale) .	22	20	15
Black Medick (Medicago lupulina) .	16	17	15
Purslane	14	15	12
White Clover	12	13	8
Red Clover	9	11	8
Red-root (Amarantus retroflexus) .	4	4	4
Ragweed (Ambrosia artemisæfolia) .	2	2	2
Pigeon-grass (Setaria glauca) . .	1	2	3
Pigweed (Chenopodium album) .	1	1	0
Shepherd's Purse	1	1	1
	82	86	66

What a happy family this was! In all this jostle up to the middle of August, during which every plant had increased its

bulk from two to twenty times, only the crab grass—apparently the most tenacious of them all—had fallen off; and yet the area seemed to be full in the beginning! How then, if all had grown bigger, could there have been an increase in numbers, or even a maintenance of the original population? In two ways: first, the plants were of widely different species of unlike habits, so that one plant could grow in a place where its neighbor could not. Whilst the pigweed was growing tall, the medick was creeping beneath it. This is the law of divergence of character, so well formulated by Darwin. It is a principle of wide application in agriculture. The farmer "seeds" his wheat-field to clover when it is so full of wheat that no more wheat can grow there, he grows pumpkins in a cornfield which is full of corn, and he grows docks and sticktights in the thickest orchards. Plants have no doubt adapted themselves directly, in the battle of life, to each other's company.

The second and chief reason for the maintenance of this dense population, was the fact that each plant grew to a different shape and stature, and each one acquired a different longevity; that is, they had varied, because they had to vary in order to live. So that, whilst all seemed to have an equal chance early in July, there were in August two great branching red-roots, one lusty ragweed and 83 other plants of various degrees of littleness. The third census, taken September 25th, is very interesting, because it shows that some of the plants of each of the dominant species had died or matured, whilst others were still growing. That is, the plants which were forced to remain small also matured early and thereby, by virtue of their smallness, they had lessened, by several days, the risk of living, and they had thus gained some advantage over their larger and stronger companions, which were still in danger of being killed by frost or accident. When winter finally set in, the little plat seemed to have been inhabited only by three big red-roots and two small ones and by one ragweed. The remains of these six plants stood stiff and assertive in the winds; but if one looked closer he saw the remains of many lesser plants, each "yielding seed after his kind," each one, no

doubt, having impressed something of its stature and form upon its seeds for resurrection of similar qualities in the following year. All this variation must have been the result of struggle for existence, for it is not conceivable that in less than two square feet of soil there could have been other conditions sufficiently diverse to have caused such marked unlikenesses; and I shall allow the plat to remain without defilement that I may observe the conflict in the years to come, and I shall also sow seeds from some of the unlike plants. From all these facts, I am bound to think that physical environment and struggle for life are both powerful causes of variation in plants which are born equal.

Still,the reader may say,like Weismann,that thesedifferences were potentially present in the germ, that there was an inherited tendency for the given red-root to grow three feet tall when 85 other plants were grown alongside of it in twenty inches square of soil. Then let us try plants which had no germ plasm, that is, cuttings from maiden wood. A lot of cuttings were taken from one petunia plant, and these cuttings were grown singly in pots in perfectly uniform prepared soil, the pots being completely glazed with shellac and the bottoms closed to prevent drainage. Then each pot was given a weighed amount of different chemical fertilizer and supplied with perfectly like weighed quantities of water. All weak or unhealthy plants were thrown out, and a most painstaking effort was made to select perfectly equal plants. But very soon they were unequal. Those fed liberally on potash were short, those given nitrogen were tall and lusty; and the variations in floriferousness and maturity were remarkable. The data of maturity and productiveness were as follows:

Phosphate of Potash.	Sulphate of Potash.	Phosphate of Soda.	Check	Phosphate of Ammonia.
68 days	99 days	65 days	67 days	104 days
23½ blooms	18 blooms	27½ blooms	26½ blooms	33 blooms

Here then, is a variation of 39 days, or over a month in the time of first bloom, and of an average of 15 flowers per plant in asexual plants from the same stock, all of which started equal and which were grown in perfectly uniform conditions, save the one element of food.

But these or similar variations in cuttings are the commonest experiences of gardeners. Whilst some philosophers are contending that all variation comes through sexual union, the gardener has proof day by day that it is not so. In fact, he does not stop to consider the difference between seedlings and sexless plants in his efforts to improve a type, for he knows by experience that he is able to modify his plants in an equal degree, whatever the origin of the plants may have been. Very many of our best domestic plants are selections from plants which are always grown from cuttings or other asexual parts. A fruitgrower asked me to inspect a new blackberry which he had raised. "What is its parentage?" I asked. "Simply a selection from an extra good plant of Snyder" he answered; that is, selection by means of suckers, not by seedlings. The variety was clearly distinct from Snyder, whereupon I named it for him. The Snyder plants were originally all equal, all divisions in fact, of one plant, but because of change of soil or some other condition, some of the plants varied, and one of them, at least, is now the parent of a new variety.

But even Mr. Weismann would agree to all this, only he would add that these variations are of no use to the next generation, because he assumes that they cannot be perpetuated. Now, there are several ways of looking at this Weismannian philosophy. In the first place, so far as plants are concerned in it, it is mere assumption, and, therefore, does not demand refutation. In the second place, there is abundant asexual variation in flowering plants, as we have seen; and most fungi, which have run into numberless forms, are sexless. In the third place, since all agree that plants are intimately adapted to the conditions in which they live, it is violence to suppose that the very adaptations which are directly produced by those conditions are without permanent effect. In the fourth place, we know as a matter of common knowledge and also of direct experiment, that acquired characters in plants often are perpetuated.

I cannot hope to prove to the Weismannians that acquired characters may be hereditary, for their definition of an acquired

character has a habit of retreating into the germ where neither they nor anyone else can find it. But this proposition is easy enough of proof, viz., plants which start to all appearances perfectly equal, may be greatly modified by the conditions in which they grow; the seedlings of these plants may show these new features in few or many generations. Most of the new varieties of garden plants, of which about a thousand are introduced in North America each year, come about in just this way. A simple experiment made in our greenhouses also shows the truth of my proposition. Peas were grown under known conditions from seeds in the same manner as the petunias were, which I have mentioned. The plants varied widely. Seeds of these plants were saved and all sown in one soil, and the characters, somewhat diminished, appeared in the offspring. Seeds were again taken, and in the third generation the acquired characters were still discernible. The full details of this and similar experiments are waiting for separate publication. The whole philosophy of "selecting the best" for seed, by means of which all domestic plants have been so greatly ameliorated, rests upon the hereditability of these characters which arise after birth; and if the gardener did not possess this power of causing like plants to vary and then of perpetuating more or less completely the characters which he secures, he would at once quit the business because there would no longer be any reward for his efforts. Of course, the NeoDarwinians can say, upon the one hand, that all the variations which the gardener secures and keeps were potentially present in the germ, but they cannot prove it, neither can they make any gardener believe it; or, on the other hand, they can say that the new characters have somehow impressed themselves upon the germ, a proposition to which the gardener will not object because he does not care about the form of words so long as he is not disputed in the facts. Weismann admits that "climatic and other external influences" are capable of affecting the germ, or of producing "permanent variations," after they have operated "uniformly for a long period," or for more than one generation. Every annual plant dies at the end of the season, therefore whatever effect the environment may

have had upon it is lost, unless the effect is preserved in the seed; and it does not matter how many generations have lived under the given uniform environment, for the plant starts all over again, *de novo*, each year. Therefore, the environment must affect the annual plant in some one generation or not at all. It seems to me to be mere sophistry to say that in plants which start anew from seeds each year, the effect of environment is not felt until after a lapse of several generations, for if that were so the plant would simply take up life at the same place every year. This philosophy is equivalent to saying that characters which are acquired in any one generation are not hereditary until they have been transmitted at least once!

My contention then, is this: plants may start equal, either from seeds or asexual parts, but may end unequal; these inequalities or unlikenesses are largely the direct result of the conditions in which the plants grow; these unlikenesses may be transmitted either by seeds or buds. Or, to take a shorter phrase, congenital variations in plants may have received their initial impulse either in the preceding generation or in the sexual compact from which the plants sprung.

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A COMPARATIVE STUDY OF THE POINT OF ACUTE VISION IN THE VERTEBRATES.[1]

BY J. R. SLONAKER,
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In this preliminary sketch of a comparative study of the eyes of vertebrates, with special reference to the *fovea centralis* or point of acute vision, I shall first give the processes and methods of preparation which I have used and results obtained, and, second, the position of the *area centralis* as indicated by the retinal arteries. The microscopic descriptions and the relation of the position and shape of the eye and arrangement of the retinal elements to the habits of the animal will follow in a later paper.

[1] I wish to thank Dr. C. F. Hodge for valuable assistance and for his method of injecting the eye-ball, thus preserving it for complete sections. I am also very much indebted to Clark University for valuable aid and for apparatus and materials to further this study.

For microscopical purposes and best results it is necessary to obtain the eye fresh, at least not later than an hour after death, and subject it to the action of certain hardening liquids which will permeate and preserve without causing the retina to swell and become wrinkled. With some animals it is quite easy to preserve the retina without its becoming wrinkled or floated off (fishes, amphibians, reptiles, and some mammals), while with others (most mammals and birds) it is a more difficult task.

In order to prevent this folding and floating off of the retina, the eye is injected under pressure and immersed at the same time in a bath of hardening fluid. It is carried thus on up through the different percentages of alcohol and imbedded in celloidin.

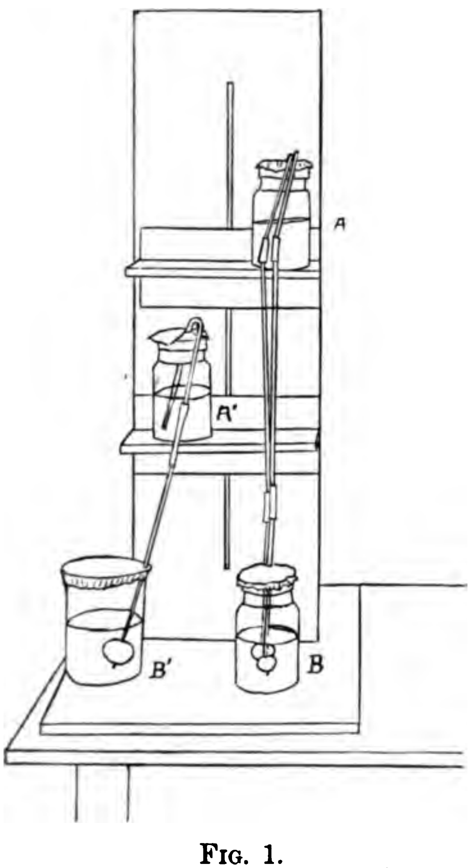

FIG. 1.

A more minute description of the method is as follows: Fig. 1 represents a rack with movable shelves, on which are placed bottles A and A′, containing the same fluid as bottles B and B′, and provided with siphons to connect with glass cannulas.

In order to insert the cannula, a hole is carefully drilled about the equator and on a meridian perpendicular to the plane in which it is desirable to obtain sections. The perforation is stretched open, rather than cut, so the sclerotic will clasp the neck of the cannula tightly. A convenient instrument for this operation is a spear-pointed dissecting needle, and not

too sharp. At the same time reach forward with the point of the needle and pierce the suspensory ligament and iris in order to open the aqueous chamber. In doing this, care is taken not to injure structures in the plane of the desired sections. A cannula of suitable size, being connected with a siphon from A or A′, is filled with the liquid and inserted. The cannula should have a fine smooth point. Great care is taken in inserting it so that the stream of fluid is not directed behind the retina to float it off. A hole is now made in the opposite side of the eye, the aqueous chamber again pierced and all aqueous and vitreous humor allowed to run out. In some animals this humor is very much more gelatinous than in others, and requires much more pressure to remove it. The hole below is then stopped with a small glass plug (Fig. 2, B), and the eye immersed in hardening fluid (Fig. 1, B). The bottles are now covered as tightly as possible with tinfoil to prevent evaporation and entrance of dust particles. The cannula and stopper should fit so tight that there is no leak. In every case the orientation of the eye is marked before it is removed from the head. This is done by sewing a small tag to the outer layers of the sclerotic (Fig. 2, C).

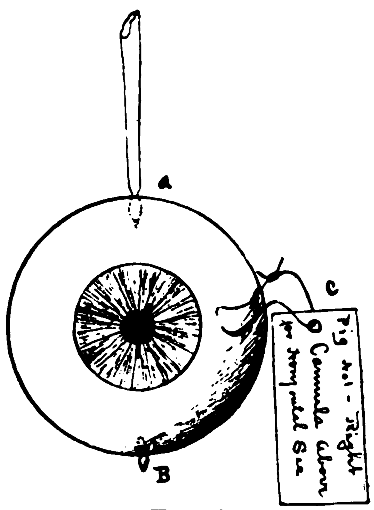

Fig. 2.

The pressure varies greatly with the kind of eye used. Those with thin walls, or containing much cartilage, birds and amphibians, require little pressure, while mammals, in general, can receive much higher. The pressures which I have found to work best vary between 28 and 36 cm.

The hardening fluid used is Perenyi's, in which the eye is allowed to remain twenty-four hours, when it is changed to 70 per cent. alcohol.

In making changes of liquids, great care should be taken that no air get into the eye, and that all the former liquid is

replaced with fresh by removing the stopper in the lower part of the eye. After remaining twenty-four hours in each of the following liquids: 80, 90, 95 per cent., absolute alcohol and absolute ether (1 part each), it is then changed to celloidin. Best results are obtained when three grades of celloidin are used—1st, very dilute; 2d, less dilute; 3d, as thick as will run. It is allowed to remain from four to six days in the first, six to eight days in the second, and ten to fifteen days in the third. If the eye is kept well under pressure throughout this process, the retina will be well preserved and lie smoothly against the choroid.

I have tried other liquids for hardening the eye whole, but with poor success. Have tried the method of Barrett and of Cuccati, but, in each case, the retina was very much wrinkled and folded, while the whole eye was much shrunken and out of shape. In vapors of osmium, I have had fairly good results with the retina, but the same trouble, due to the shrinking of the whole eye, is present. Chievitz says[2] that a fish's eye may be preserved whole, with retina lying nicely back, by simply immersing it, or even the whole head, in 80 per cent. alcohol. The hardening agent which he generally uses is 2.5 per cent. nitric acid.

Another method which I have employed with small animals, especially birds, in order to demonstrate quickly the presence or absence of a fovea, is to immerse the whole head in Perenyi's fluid for from three to five hours. This will harden the eyes so that the. cornea, lens and vitreous humor may be removed, leaving the posterior half in situ. With birds I have had good results, the retina lying back smoothly so that the fovea and entrance of the nerve, marked by the pecten, may be easily seen. Fig. 3 represents diagrammatically the appearance of the retina after the front of the eye has been removed.

In order to show the angles which the lines of vision make with the median plane, sections were made through the whole head of several animals (fish, amphibians, reptiles, birds and

[2] J. H. Chievitz, Untersuchungen über die Area centralis retinae. Archiv für Anatomie und Entwickelungsgeschichte, Sup., Band, 1889, p. 141–142.

small mammals), the plane of the section passing through each fovea on the centre of the area centralis. Fig. 4 represents such a section through the foveæ *a* and *b* of a chickadee's head (*Parus atricapillus*), while the lines G H and G I show

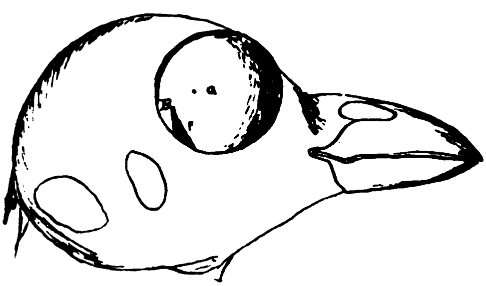

FIG. 3.

Snow-bird (*Junco hyemalis*) x 3.
A, Fovea centralis.
B, Entrance of optic nerve.
P, Pecten.

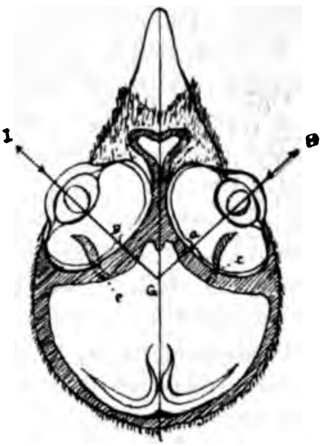

FIG. 4.

Chickadee (*Parus atricapillus*) x 3.
A and B. Foveæ.
C, C, Entrance of optic nerves.
G H and G I, Axes of vision.

the axis of vision. The dotted lines *c* mark the position of the optic nerves which enter in a plane much lower down. In order to harden the whole head, and, at the same time, decalcify the bone, it must remain longer in Perenyi's fluid (about thirty- six hours), and to preserve the cornea and lens in position, a window is made in the top of the eye that the fluids may enter.

Having had good success with simple immersion of the head, this method was tried for hardening the small eyes, and with good success. In fact, the retina proved in good condition, if not better, than when taken through by the injection method. The eye-ball, however, usually caves in when placed in 70 per cent. or 80 per cent. alcohol, but this may be prevented by simply making a small slit through the sclerotic

into the vitreous chamber before immersing in 70 per cent. alcohol to allow the liquids to pass in. Just before putting into celloidin, a window is made parallel to the plane of desired sections, and the hardened vitreous humor is easily removed without injury to the retina or other structures. This method is now used with small eyes instead of the injection, as it is so much easier of manipulation.

In order to show the relation of the retinal arteries to the area and fovea centralis, they were injected with the gelatine-carmine mass of Ranvier. In small animals this injection was made in the carotid arteries, while with large animals the eyes were removed and the injection made into that branch of the ophthalmic artery which supplies the retina. After injection, the eyes were at once cooled and hardened in alcohol. When hardened, the front half of the globe and the vitreous humor were carefully removed, exposing to view the retina, arteries, entrance of nerve, and area and fovea centralis, when present. The fovea is at once seen if it be present, but the area is sometimes very difficult to discern, and, were it not for the blood-vessels acting as land-marks, it might be overlooked altogether. Drawings were made of this posterior half, great care being taken to orient it, so that one would look into it along the axis of vision.

The results of these injections only serve to substantiate Müller's observation.[3] He states that mammals are the only class of vertebrates which possess, in the true sense, a retinal circulation, while with many mammals only a meagre circulation is present (horse and rabbit). Fish and amphibians possess a good circulation in the hyaloid membrane, while birds and many reptiles have the circulation of the pecten. Huschke states that these vessels of the hyaloid membrane and the pecten correspond to the retinal vessels in mammals. They do not, however, penetrate the retina.

With animals which have neither retinal nor hyaloid vessels, it would appear that the retina is nourished by the choroidal vessels. In fact, in animals with good retinal circulation, the capillaries do not penetrate deeper than the outer

[3] H. Müller, Anatomie und Physiologie des Auges, p. 117.

molecular layer, thus leaving the rod and cone, and outer nuclear layers without blood-vessels.[4]

Investigations show that not all vertebrates possess foveæ, but that each class has a representative which does. When there is no fovea, a well-defined area centralis is usually present. However, in some vertebrates, even an area has not been observed.

The following condensed tabulation will show the frequency of the area and fovea centralis in the eyes which have been examined.[5]

Number of different species.		No area found.	Area found.		Fovea.		
			Round.	Band-like.	Simple.	Band like or trough-like	Double.
28	Mammals	13	9	6	2		
80	Birds	1	84	32	85	23	7
9	Reptiles	1	6	2	3	2	
12	Amphibians	3	1	8	1	1	
4	Fishes	3	1		1		

From this tabulation it is readily seen, so far as experiments have gone, that in mammals the presence of a fovea is the exception while an area is the rule. The primates are the only mammals in which a fovea has been found. Most of the mammals examined have a well-defined area which is easily seen, but, in some, an area has not been demonstrated. The arrangement of the retinal vessels, however, indicates the presence of an area which is free from blood-vessels, and may correspond to the area centralis of other animals.

[4] H. Müller, Anatomie und Physiologie des Auges, p. 103.

[5] These results are partly obtained from the tabulation of J. H. Chievitz in his article: Ueber das Vorkommen der Area centralis retinae in den vier höheren Wirbelklassen. Archiv. f. Anat. u. Entwick., 1891, p. 321–325.

With birds, the presence of a fovea seems to be the rule. In fact, the domestic chicken is thus far the only exception. Many birds have a fovea and band-like area, while some have two foveæ and a band-like area connecting them.

In reptiles, the number of species provided with fovea or simple area are more nearly equal, while with amphibians and fishes, the area has frequently not been seen, and the fovea is only seldom observed.

The area centralis varies greatly in form and extent in different animals. It varies from the round form of small extent found in the cat and the weasel to the band-like form found in the horse, sheep, rabbit, frog, etc., which extends horizontally across the retina.

In the case of the fovea we also find a variety of forms and positions. In some animals it is situated on the nasal side of the entrance of the optic nerve (*fovea nasalis*), while in others it is on the temporal side (*fovea temporalis*). According to Müller,[6] in the former case we have monocular vision, while in the latter we have binocular vision. In form it varies from a mere dot-like impression, as in some lizards, to a well marked funnel-like pit in most birds, especially crow, bluejay, robin, etc., and to a trough-like depression in the crocodile which extends horizontally across the retina. Two foveæ have been found in some birds, as in swallows and terns, in which case the fovea nasalis is very near the centre of the retina, and has to do with single vision. It is also larger and deeper than the fovea temporalis, which is situated near the ora serrata and functions in double vision. According to Chievitz,[7] the tern has not only two fovæ, but a trough-like fovea connecting them, and the goose, duck and gull have a round fovea and a band-like area.

A great difference exists in the different vertebrates when their ability for acuteness of sight is considered. It varies from the most perfect sight found in man (and possibly in birds

[6] H. Müller, Ueber das Vorhandsein zweier Fovea in der Netzhaut Vieler Vogelaugen—Zehender, Klinische Monatsblätter, Sept., 1863, p. 438–440; or Anatomie und Physiologie des Auges, p. 139, 142–143.

[7] J. H. Chievitz, Ueber das Vorkommen der Area centralis retinae, Archiv. f. Anat. u. Entwick., 1891, p. 324.

also) where exceedingly fine discriminations are possible, to the limited visual power found in other animals, where only an area centralis is present. Though acute vision and a fovea have always been associated, still we cannot, at present, say that the animals which do not possess a fovea are not able to see acutely. In order to make clear the relation of sight to the habits of the animal, a much more careful observation of its visual habits, and the histological arrangement of the retinal elements will be necessary.

EDITOR'S TABLE.

—THE Antivivisectionists have been endeavoring to get a consensus of opinion on the utility of vivisection, by circulating blanks for signatures, which are attached to a few alternative opinions on the subject in point. The alternatives, excepting those expressing an unconditional affirmative and negative, were not sufficiently precise or well stated to satisfy persons of moderate views, so that it was necessary to amend them more or less to express such opinions. In the summary of the results thus obtained, the antivivisection managers omitted most of these moderate views, and only gave to the public the two extremes. The circulars were also very injudiciously distributed, as a majority of them went to persons unfamiliar with the work of scientific research, as clergymen, etc. The only persons who have a practical knowledge of the subject are original investigators in the natural sciences, physiologists and physicians. The opinions of other persons must be mostly formed at second hand.

As a body of men, those above referred to are at least as humane as any other class in the community. Their business is to relieve suffering, and they are not insensible to those of the lower animals. Naturalists, as a body, are probably more humane in their feelings towards animals than any other class in the community. Nearly all of these men are, however, well convinced not only of the propriety, but of the necessity of vivisection. It is the only method of attacking many difficult problems of physiology. It is the basis of our knowledge of the functions of the human organism, which is itself the first essential to the control of human disease and human suffering. The antivivi-

sectionists are, unwittingly, doing what they can to sustain ignorance and to prevent the relief of human suffering. They are sacrificing their fellow beings, their relatives and their friends, in preference to a few of the lower animals. Men, women and children may suffer and die; white rabbits, guinea-pigs and dogs may live. Such logic is like that of the Spanish Inquisitors, who tortured human beings under the belief that they served God and the cause of religion in so doing. There is, however, less excuse for the antivivisectionists, since knowledge is more widely distributed now than then, and the great utility of vivisection has been demonstrated over and over again.

The six national scientific societies to meet during the holidays in Philadelphia will probably express their views on this subject, and it may be confidently expected that these will accord with those of science the world over.

Intelligent people are best deceived by intelligent frauds. A fraud in order to succeed in the United States must make pretensions to superior knowledge. The alleged or actual graduate of medicine who desires to be a fraud has a pretty good field in this country; and his successes are ever with us, in spite of the opposition of the many true men of that profession. The scientific fraud has not yet developed very largely, as there is no money to be made by pretense in this direction. In fact this species of the genus is not generally a person of evil intentions, and errs chiefly through an active imagination, and perhaps sometimes through a tendency to megalomania.

We are moved to these remarks by reading an article in the December number of a Chicago Journal called Self Culture. On p. 587 we read; "Examination of the brain of such an idiot before its education has begun, shows but few brain cells, and a few nerve fibres connecting them. And when a postmortem has been made upon the child that was once an idiot but that has been lifted up by long years of patient training to citizenship in the moral and rational sphere in which we live and move, such a postmortem shows that an infinite number of brain-cells have been created *de novo;* that fibers becoming necessary have *appeared,* to connect such cells, centers of sensation and emotion and thought."

Now the author of this paragraph should refer us to the published articles which describe the removal of the brains or parts of brains of idiotic children for sectioning and microscopic investigation, and the subsequent replacement of these organs or parts of them in the crania of the children in order that they may undergo the "long years of

patient training" which follow. We would like to know the technique of the operation, and the name of the operator and that of the institution where he operates. Some grown persons might desire to secure his services, and almost everybody could point out some one else, to whom they think such a course of treatment would be useful. Some peculiar conditions might be found which it would be desirable to remove permanently, and so save the "labor of long years" etc.

The editor of the Journal on page 609 stimulates our curiosity further by saying that "Professor Elmer Gates, a psychologist who has for several years been making elaborate studies both in Washington and Philadelphia, has added not a little to our knowledge of the developments of the brain and the relation of particular parts of the brain to thought and emotion and the use of particular parts of the body." The view indeed is not new, but the confirmation given by Prof. Gates researches is very interesting" He then quotes language from Dr. Julius Althaus as to the supposed seat of mental activity in the brain, which embodies a general statement of the little knowledge we have on the subject. The question naturally arises as to the alleged researches of Dr. Gates, and the extent to which they have confirmed our hypotheses on this subject, and if so, as to where they were published? The editor does not tell us. This is a pity, for assertions without authority are useless to science. Is there any connection between these researches and the alleged vivisection of idiots recounted in the article we first quoted? The name signed to the latter is not that of Dr. Gates, so we are quite in the dark. A journal which publishes an article by Sir Wm. Dawson, and writes up the Universities, ought to give us more light no these wonderful researches.

—It is again proposed that the American Association for the Advancement of Science meet in San Francisco in the near future. The Board of Supervisors of that city are said to have extended an invitation to visit the city in 1897. The Association has had many such invitations, and they would have been accepted had the railroad authorities been willing to place their rates within reach of the members. The authorities of San Francisco have, however, this time included in their invitation the British and Australian Associations, and we are informed that the British members will have free or nominal transportation via the Canadian Pacific R. R. It is said that the Dominion of Canada will make an appropriation towards defraying their transportation expenses. Perhaps our Congress would be willing to make an appropriation for securing the transportation of our own members. The amount will not

exceed the outlay on funeral solemnities annually expended by it. Such meetings tend to bring about amicable relations among the living, and to promote the interest in and distribution of knowledge. It might be good politics if the Canadian Boundary and Venezuelan questions should be still on hand in 1897.

RECENT LITERATURE.

Petrology for Students : An Introduction to the Study of Rocks under the Microscope, by Alfred Harker, University Press, Cambridge. MacMillan & Co., New York, 1895. Pp. vi and 306; figs. 75 ; price $2.00.

This volume of the Cambridge Natural Science Manuals will be heartily welcomed by teachers and students of geology in all English-speaking countries. It presupposes a knowledge of the microscopical features of minerals, and consequently deals only with rocks. These the author divides into Plutonic, Intrusive, Volcanic and Sedimentary rocks. Under each head the general characteristics distinguishing each of the several rock classes are briefly mentioned, and descriptions of the different rock types embraced in each group are given. First come descriptions of the constituents of each rock, then follows a statement of its pecularities of structure. The principal varieties are next mentioned, and abnormal, structural and chemical forms are briefly described. The book concludes with chapters on thermal and dynamic metamorphism and one on the crystalline schists.

Of course, the treatment of the different subjects discussed is necessarily very brief, nevertheless it is full enough in most cases to give the student beginning petrography a very good view of the field. A specially important feature of the work is the large list of references to articles written in English. With this book at hand, students will no longer be required to wait until they have mastered German before beginning the study as heretofore been the case. While by no means exhaustive, the present volume will serve as an excellent introduction to the larger French and German treatises, and will, at the same time, be a good reference book for geologists who do not desire to make a specialty of microscopic lithology.—W. S. B.

Crystallography, a Treatise on the Morphology of Crystals, by N. Story-Maskelyne, Oxford, Clarendon Press, 1895. New

York: MacMillan & Co. Pp. xii and 512; figs. 597, pl. viii; price, $3.50.

This "Crystallography" is a real addition to the literature of the subject that it treats. Its appearance reminds one strongly of Groth's "Physiographische Krystallographie," although the book is by no means a reproduction of the German treatise. The latter discusses the subject from the side of solid symmetry, whereas the former deals with it rather from the analytical point of view. The first 187 pages of the volume treat of the general relations of crystal planes and of zones. The next 200 pages take up the six crystal systems beginning with the cubic, and discuss in order the holosymmetrical and the merosymmetrical forms, combination of forms and twinned forms. Chapter VIII, embracing pages 388–463, is devoted to crystal measurements and calculations, and the final chapter to the projection and drawing of crystals. The plates show the projection of the poles of the most general form and of its derived hemihedral and tetartohedral forms in each system.

It is almost needless to state that the work of the author is based exclusively on the system of indices, known generally as the Miller system. Not only are the faces of crystal forms studied through the aid of the spherical projection, but the individual planes are discussed solely in terms of their normals. No reference is made to other systems of notation, nor to other methods of projection than those elaborated. The book might have been of a little more practical value had the author at least referred to other systems, but its unity might have suffered. As it is, the volume is a very complete exposition of crystallography from the Miller standpoint, and it will, without doubt, prove of inestimable value in popularizing this—the most beautiful method of studying the subject. Of course, the treatment is purely mathematical, but the mathematics used are simple enough to be understood by any one acquainted with the methods of spherical geometry. To the student of minerals too much emphasis will seem to be placed on the theoretical aspect of the development of crystal forms, but to the specialist in crystallography, the emphasis will appear to be placed just where it belongs—on the possibility of deriving all possible symmetrical polyhedrons from certain simple abstract notions concerning pairs of planes, at the basis of which is the principle of the rationality of the indices.

There is no doubt that the treatise before us will appeal less strongly to the student of forms than it will to one of analytical proclivities. Nevertheless it is needed even by the former, if, for no other reason,

because it will impress him more strongly than ever with the exactness with which nature constructs her inorganic structures. With Dr. Williams' little book to develop the imagination of the beginner in crystallography and to interest him in the science, and the present volume to carry him on to a very thorough understanding of the relationships of crystal forms, the English-reading student-world is as well, if not as bountifully, supplied with text books on the subject as are the students of any European country.

The authors discussions are all logically developed, and all his statements are clear and simple. The figures are well drawn and the subjects they illustrate are well selected.—W. S. B.

Elementary Physical Geography, by Ralph S. Tarr. New York: MacMillan & Co., 1895. Pp. xxxi and 488; figs. 267, plates and maps 29; price $1.40.

The most striking features of Prof. Tarr's book are the freshness and wealth of its illustrations and the excellence of its typography. The volume is just what its title indicates, except that perhaps the treatment of its subject matter is a little more inclined toward the side of physiography than toward physical geography. The book is indeed elementary—more so than one would wish, sometimes; at other times it is elementary in the statement of the facts described, while leaving their causes unexplained, where a word or two might have avoided a difficulty which the teacher will surely meet with in discussions with his brightest scholars. In the arrangement of material, some fault can easily be found, but, as the author himself declares, the treatment is, "in many respects, experimental." In spite of these criticisms, the experiment is a success.

The volume is divided into three parts, with four appendices and a very good index. The first part deals with the air. It includes chapters on the earth as a planet, the atmosphere in general, distribution of temperature in the atmosphere, its general circulation, storms, its moisture, weather and climate, and the geographic distribution of plants and animals. Why the first and last chapters included in this part are discussed here is not quite plain. Part second deals with the ocean. It embraces chapters on the ocean in general, waves and currents and tides. Part third treats of the land and its features. A general description of the earth's crust is discussed in the opening chapters. Then follow chapters on denudation, the topographic features of the surface, river valleys, deltas, waterfalls, lakes, etc., glaciers, the coast line, plateaus and mountains, volcanoes, earthquakes, etc.,

man and nature and economic products. The appendices include one on meteorological instruments, methods, etc., one on maps and one containing suggestions to teachers. The last is a list of questions on the text. At the end of each chapter is a list of reference books, with their titles and prices. This is not of much value to the student, but is convenient for the teacher. A list of articles to be found in *Nature*, *Science*, the *Popular Science Monthly*, and similar periodicals might have been of more value in an elementary treatise. However, the plan of referring students to original articles on the subjects discussed is commendable. We can not dismiss the book without another reference to the many really excellent illustrations and charts it contains. The former are, without exception, fresh and new, well chosen to illustrate the author's points and well executed from the bookmaker's standpoint. Many of the charts are original. The volume is, on the whole, the most attractive that we have seen on the subject it treats, and its attractiveness is not at the expense of scientific accuracy. We can safely predict a general adoption of the book as a text in many high schools and academies, and we shall be mistaken if it is not used in some of our colleges, where the instructor desires an *aid* in his work rather than a *substitute* for work.—W. S. B.

Gray's Synoptical Flora of North America.—In 1835 or 1836, Dr. John Torrey planned a Flora of North America, with which Dr. Gray soon became identified, and, in July, 1838, the first part (Ranunculaceæ to Caryophyllaceæ) was published; a little later (October, of the same year), the second part appeared, and in June, 1840, the third and fourth parts were issued, completing Vol. I, the Polypetalæ. As will be remembered, Volume II was not completed, a portion appearing in 1841, and the work being suspended at the end of the Compositæ in 1843 (February). Here the work stopped for many years, and was resumed in 1878 by Dr. Gray (Dr. Torrey having died five years earlier) under the slightly different title of A *Synoptical* Flora of North America. In this volume the Gamopetalæ were completed; in 1884, the Compositæ and preceding families, since whose elaboration more than forty years had passed, were revived. Then shortly afterwards, 1888, came the death of Dr. Gray, followed, in 1892, by the death of Dr. Watson, before the publication of other parts.

In October, 1895, Dr. B. L. Robinson issued the first fascicle of the revision of Vol. I of the Flora, a little more than fifty-seven years since the appearance of the corresponding fascicle. This includes the polypetalous families—Ranunculaceæ to Frankeniaceæ. It includes much

of Dr. Gray's work, to which is added something of Dr. Watson's work, to which we have now added the results of Dr. Robinson's studies.

With such a history, stretching back as it does through more than half a century, it is not to be wondered at that the work is conservative to a marked degree. The sequence of families can differ little from that adopted nearly sixty years ago, and in this fascicle the citation of authorities, the matter of nomenclature, etc., have been made to conform as far as possible to the treatment accorded them seventeen years ago. This extreme conservatism is to be regretted, since science is more productive just as its followers are least tied by the traditions of the past. Yet, with all its conservatism, the Synoptical Flora will be invaluable, and every systematic botanist will hope that health and strength may not fail the present editor before his task is completed.

—CHARLES E. BESSEY.

The Natural History of Plants.[1]—About seven years ago the eminent professor of botany in the University of Vienna, gave to the botanical world a book under the title *Pflanzenleben*, with which botanists soon became familiar as a most useful work. Some time ago the welcome announcement was made that the work was to be translated and brought out simultaneously in England and America. This has now been accomplished, and the result is before us in four good sized volumes, each called a "half-volume," which are attractive externally and internally. On comparing the translation, as brought out by Messrs Holt & Co., with the original, it must be conceded that the former is the by far better done, both in the clearness of text and the perfection with which the printer has brought out the illustrations. The colored plates are especially well done, being printed from the originals by the Bibliographische Institut of Leipzig.

For those who have not seen the original, it may be well to say that it presents in a readable manner (in a *popular* manner, we might say, if the word had not been so dreadfully abused) the main facts as to the structure, biology, and physiology of plants. It is not a text book for daily conning by the student, but it is rather a most interesting work to

[1] *The Natural History of Plants*, their forms, growth, reproduction and distribution, from the German of Anton Kerner von Marilaun, Professor of Botany in the University of Vienna, by F. W. Oliver, M. A., D. Sc. Quain Professor of Botany in University College, London, with the assistance of Marian Busk, B. Sc., and Mary F. Ewart, B. Sc. With about 1000 original woodcut illustrations and 16 plates in colors. New York: Henry Holt & Company, 2 vols., large 8vo. pp. 777 and 983.

be read by not only the botanist, but by every intelligent man and woman who would know something of the deeper problems with which modern botany concerns itself. The topics noted in the table of contents will give some idea of the scope of the work as follows: The study of plants in ancient and modern times; The living principle in plants; Absorption of nutriment; Conduction of food; Formation of organic matter from the absorbed inorganic food; Metabolism and transport of materials; Growth and construction of plants; Plant forms as completed structures; The genesis of plant offspring; The history of species.

A single quotation taken from the opening chapter may serve to show the delightful style in which the work is written: "Some years ago, I rambled over the mountain district of north Italy in the lovely month of May. In a small sequestered valley, the slopes of which were densely clad with mighty oaks and tall shrubs, I found the flora developed in all its beauty. There, in full bloom, was the laburnum and manna-bush, besides broom and sweet-brier, and countless smaller shrubs and grasses. From every bush came the song of the nightingale, and the whole glorious perfection of a southern spring morning filled me with delight. Speaking, as we rested, to my guide, an Italian peasant, I expressed the pleasure I experienced in this wealth of laburnum blossoms and chorus of nightingales. Imagine the rude shock to my feelings on his replying briefly that the reason why the laburnum was so luxuriant was that its foliage were poisonous, and goats did not eat it; and that though no doubt there were plenty of nightingales, there were scarcely any hares left. For him, and, I dare say, for thousands of others, this valley clothed with flowers was nothing more than a pasture ground, and nightingales were merely things to be shot.

"This little occurrence, however, seems to me characteristic of the way in which the great majority of people look upon the world of plants and animals. To their minds, animals are game, trees are timber and firewood, herbs are vegetables (in the limited sense), or, perhaps, medicine or provender for domestic animals, whilst flowers are pretty for decoration. Turn in what direction I would, in every county I travelled for botanical purposes, the questions asked by the inhabitants were always the same. Everywhere I had to explain whether the plants I sought and gathered were poisonous or not; whether they were efficacious as a cure for this or that illness, and by what signs the medicinal or otherwise useful plants were to be recognized and distinguished from the rest."—CHARLES E. BESSEY.

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General Notes.

PETROGRAPHY.[1]

The Origin of Adinoles.—Hutchings[2] has discovered a contact rock at the Whin Sill, England, which, in the author's opinion, represents an intermediate stage in the production of an adinole from a fragmental rock. It contains corroded clastic grains of quartz and feldspar in an isotropic base containing newly crystallized grains of quartz and feldspar. The isotropic material is derived from the clastic grains by the processes of contact metamorphism, whatever they may be, as grains of quartz are often seen with portions of their masses replaced by the substance. The rock has begun its recrystallization from the isotropic material produced by solution or fusion of the original grains, but the process was arrested before the crystallization was completed. The paper concludes with some general remarks on metamorphism. The author thinks that the statement that in granite contacts no transfer of material takes place has not yet been proven true. He also thinks that more care should be taken in ascribing to dynamic metamorphism certain effects that may easily be due to the contact action of unexposed dioritic or granitic masses.

Notes from the Adirondacks.—The limestones, gneisses and igneous instrusives of the Northwestern Adirondack region are well described by Smyth.[3] The intrusions consist of granites, diorites, gabbros and diabases. The gabbro of Pitcairn varies widely in its structure and composition, from a coarse basic or a coarse, almost pure feldspathic rock to a fine grained one with the typical gabbroitic habit.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Geological Magazine, March and April, 1895.

[3] Bull. Geol. Soc. Amer., Vol. 6, p. 263.

Compact hornblende is noted as an alteration product of its augite. Where in contact with the limestones the gabbro has changed these rocks into masses of green pyroxene, garnet, scapolite and sphene. A second variety of the gabbro is hypersthenic. A third variety is characterized by its large zonal feldspars composed of cores of plagioclase surrounded by microperthite, although crystals of the latter substance alone abound in some sections. The ferromagnesian components are rare as compared with the feldspars. Nearly all specimens of these rocks are schistose, and all of the schistose varieties exhibit the cataclastic structure in perfection. Analysis of the normal (I) and of the microperthitic or acid (II) gabbros yielded:

	SiO_2	Al_2O_3	FeO	MgO	CaO	K_2O	Na_2O	H_2O	Total
I	57.00	16.01	10.30	1.62	6.20	3.53	4.35	.15	= 99.16
II	65.65	16.84	4.01	.13	2.47	5.04	5.27	.30	= 99.71

Near the contact with the limestone the gabbro is finer grained than elsewhere. Pyroxene is in larger grains than in the normal rock, but the feldspar is in smaller ones. The limestone loses its banding and is bleached to a pure white color. Between the two rocks is a fibrous zone of green pyroxene and wollastonite, together with small quantities of sphene and garnet and sometimes scapolite and feldspar. The red gneisses, common to that portion of the region studied which borders on the gabbro, are thought by the author to be largely modified portions of the intrusive rock.

The Eastern Adirondacks have been studied by Kemp.[4] The limestones of Port Henry consist of pure calcite, scattered through which are small scales of graphite, phlogopite and occasionally quartz grains, apatite and coccolite. This is cut by stringers of silicates that are granitic aggregates of plagioclase, quartz, hornblende and a host of other minerals. Ophicalcite masses are also disseminated through the limestones, and these are also penetrated by the silicate stringers. Merrill[5] has shown that the serpentine of the ophicalcite is derived from a colorless pyroxene. The schists associated with the limestones are briefly characterized by the author. At Keene Center a granulite was found on the contact of the ophicalcite with anorthosite.

Hornblende Granite and Limestones of Orange Co., N. Y. —Portions of Mts. Adam and Eve at Warwick, Orange Co., N. Y., are composed of basic hornblende granite that is in contact with the white

[4] Ibid, p. 241.

[5] Cf. AMERICAN NATURALIST, 1895, p. 1005.

limestone whose relations to the blue limestone of the same region have been so much discussed. The granite contains black hornblende, a little biotite, and so much plagioclase that some phases of it might well be called a quartzdiorite. Allanite and fluorite are also present in the rock, the former often quite abundantly. As the granite approaches the limestone it becomes more basic. Malacolite, scapolite and sphene are developed in it in such quantity, that immediately upon the contact the normal components of the granites are completely replaced. On the limestone side of the contact the rock becomes charged with silicates, the most abundant of which are hornblende, phlogopite, light green pyroxenes, sphene, spinel, chondrodite, vesuvianite, etc. The contact effects are similar in character to those between plutonic rocks and limestones elsewhere. The blue and the white limestones are regarded as the same rock, the latter variety being the metamorphosed phase.[6]

An Augengneiss from the Zillerthal.—The change of a granite porphyry into augengneiss is the subject of a recent article by Fütterer.[7] The rocks are from the Zillerthal in the Alps. The gneisses are crushed and shattered by dynamic forces until most of the evidences of their origin have disappeared. The original phenocrysts have been broken and have suffered trituration on their edges, while new feldspar, quartz, malacolite and other minerals have been formed in abundance. The groundmass of the gneiss is a mosaic whose structure is partially clastic through the fracture of the original components and partially crystalline through the production of new substances. The author's study is critical, and, though he treats the described rocks from no new point of view, he discusses them with great thoroughness, calling attention at the same time to the important diagnostic features of dynamically metamorphosed rocks.

Petrographical News.—Ransome[8] has discovered a new mineral, constituting an important component of a schist occurring in the Tiburon Peninsula, Marin Co., Cal. The other components of the schist are pale epidote, actinolite, glaucophane and red garnets. The new mineral, lawsonite, is orthorhombic with an axial ratio .6652 : 1 : .7385, a hardness of 8 and a density 3.084. The axial angle is $2V = 84° 6'$ for sodium light. Its symbol is $H_4 Ca Al_2 Si_2O_{10}$.

[6] J. F. Kemp and Arthur Hollick: N. Y. Acad. Sci., VII, p. 638.
[7] Neues Jahrb. f. Min., etc., B.B. IX, p. 509.
[8] Bull. Geol. Soc. Amer., Vol. 1, p. 301.

Fuess[9] has perfected an attachment for the microscope which enables an observer to enclose with a diamond scratch any given spot in a thin section, so that it may be easily identified for further study.

Marsters[10] describes two camptonite dykes cutting white crystalline limestones near Danbyborough, Vt. They differ from the typical camptonite in being much more feldspathic than the latter rock. They moreover, contain but one generation of hornblende, corresponding to the second generation in the typical rock, and but few well developed augite phenocrysts, although this mineral is found in two generations.

A portion of Mte. S. Angelo in Lipari consists of a porous yellowish pyroxeneandesite containing grains and partially fused crystals of cordierite, red garnets and dark green spinel.[11]

Cole[12] declares that the "hullite" described by Hardman as an isotropic mineral occurring in the glassy basalts of Co. Antrim, Ireland, is in reality an altered portion of the rock's groundmass, and is no definite mineral substance.

The same author[13] describes the old volcanoes of Tardree in Co. Antrim as having produced rhyolitic lavas instead of trachytic ones as has generally been stated.

GEOLOGY AND PALEONTOLOGY.

On the Species of Hoplophoneus.—Four species of *Hoplophoneus* have already been described; *H. cerebralis* Cope, *H. oreodontis* Cope, *H. primaevus* Leidy and Owen, *H. occidentalis* Leidy. *Dinotomius atrox* will be shown to be a synonym of the latter species. To these may be added *H. robustus* and *H. insolens* herein described. The following key may be valuable in determining the species from a few characters.

A. Skull small, occiput nearly vertical.

a. Superior sectorial with large anterior basal cusp.

1. Pms. $\frac{2}{?}$ — *H. cerebralis* John Day.

b. Superior sectorial with incipient anterior basal cusp.

[9] Neues Jahrb. f. Min., etc., 1895, I, p. 280.

[10] Amer. Geol., June, 1295, p. 368.

[11] Bergeat: Neues Jahrb. f. Min., etc., 1895, II, p. 148.

[12] Belfast Nat. Field Club Proceedings, 1894–5.

[13] Geol. Magazine, No. 373, p. 303.

2. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. oreodontis* White River.
3. Pms. $\frac{3}{2}$ *H. primaevus* White River.

B. Skull large, occiput overhanging.

Superior sectorial with incipient anterior basal cusp.

4. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. robustus* White River.
5. Pms. $\frac{3}{2}$ *H. insolens* White River.
6. Pms. $\frac{2}{2}$ inferior sectorial with no posterointernal cusp, heel reduced, *H. occidentalis* White River.

Hoplophoneus occidentalis Leidy.

In The Extinct Fauna of Dakota and Nebraska (1869) Leidy described two fragments of a mandible which he thought indicated a species larger than *Hoplophoneus primaevus* and to which he gave the name *H. occidentalis* (*Drepanodon occidentalis*), figuring the specimen in Plate V. No further material was referred to this species until 1894, when Osborn and Wortman in describing a collection of White River fossils in the Bulletin of the American Museum of Natural History determined two specimens as *H. occidentalis*, giving measurements of the more important bones of the skeleton in comparison with those of *H. primaevus*. While pursuing my studies in the American Museum through the kindness of these gentlemen, I found that a complete mandible of specimen No. 1407 from the Oreodon Beds agrees in every particular with Leidy's type, which I have had the privilege of examining in the Philadelphia Academy. A drawing of the mandible accompanied by a faithful copy of Leidy's figure is given in the accompanying plate. Associated with the mandible are several vertebrae and portions of limb bones showing the skeleton to be much larger than the specimen previously determined as *H. occidentalis* in the American Museum Bulletin. They however, agree, as does also the mandible, with *Dinotomius atrox* described by Dr. Williston in the Kansas University Quarterly, January, 1895, from a fine skull and nearly complete skeleton. This specimen which I had the pleasure of seeing last summer I now have no hesitation in referring to *H. occidentalis*. It makes possible the determination of the skeletal characters and affinities, and the restoration promised by Dr. Williston will complete our knowledge of this species. The following measurements are taken from the Kansas University Quarterly.

Length from inion to premaxillary border	260 mm.
Width of zygomata	145 "
Length of mandibular ramus	164 "

Length of humerus	240	"
Width of distal end of humerus	73	"
Length of tibia	237	"
Width of proximal end of tibia	61	"
Width of distal end of tibia	41	"

The relation of *H. occidentalis* to *Eusmilus dakotensis* Hatcher, published in the December NATURALIST, is at once apparent. By comparison with the excellent figure by Mr. Weber, republished by permission in the accompanying plate, it will be seen that *H. occidentalis* stands directly ancestral to *E. dakotensis*, the dentition agreeing very strikingly in the characters emphasized by Mr. Hatcher, but differing in showing an additional incisor and premolar and the presence of a heel on the sectorial. In *Eusmilus bidentatus* Filhol, the type of the genus, the heel is present.

Hoplophoneus insolens sp. nov.

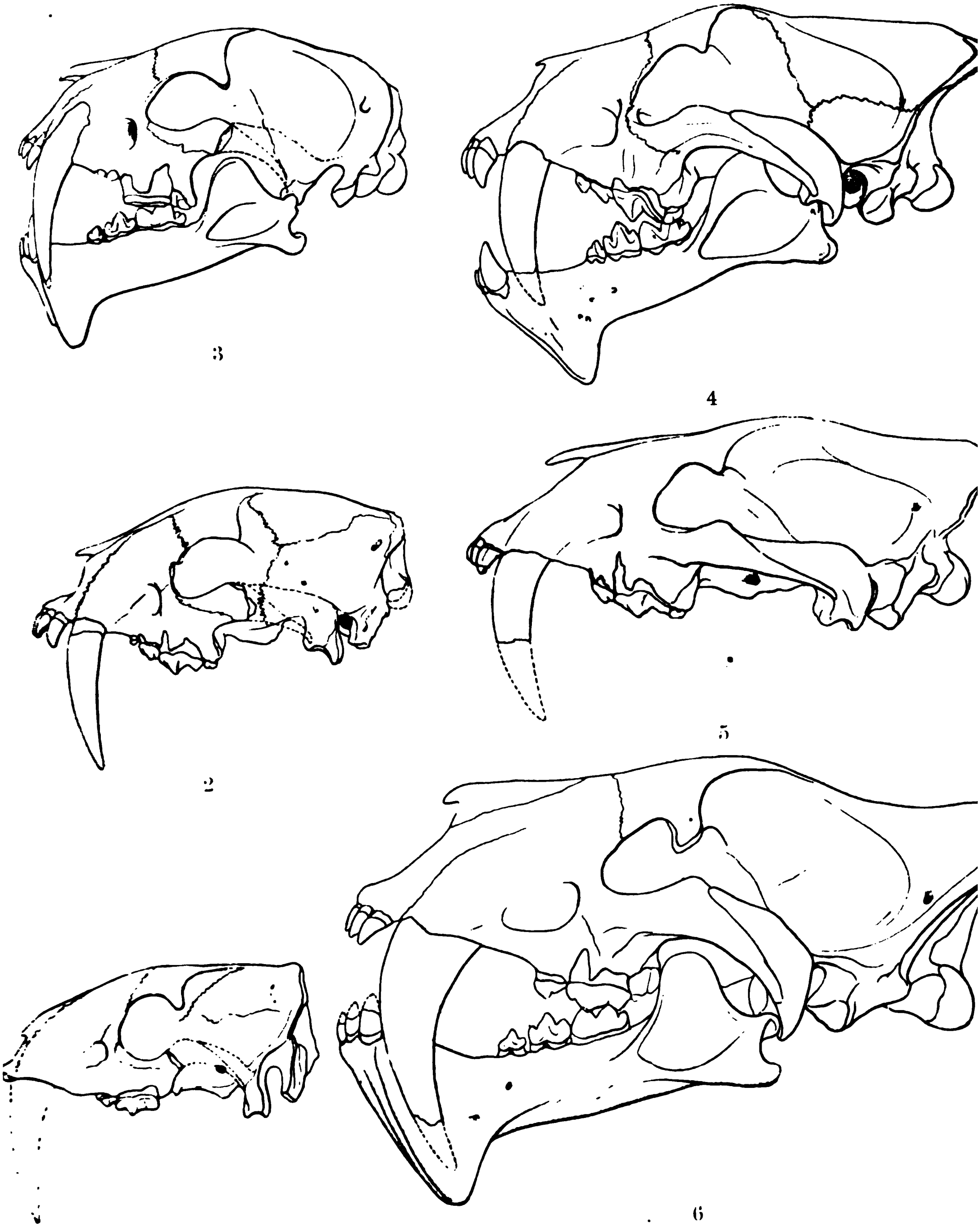
The determination of the characters of *H. occidentalis* makes it obvious that the skeleton determined as such by Osborn and Wortman is a new species. A complete skeleton (number 11,022) and a second specimen with the most of the limb bones and a skull lacking the mandible (number 11,372), both in the Princeton Museum, enable me to determine the skull of this species, a character which is lacking in the American Museum specimen. The particularly close agreement in the size of the skeletons makes either of them typical, consequently I give the measurements already published in the American Museum Bulletin along with measurements from the Princeton specimen as indicative of the size of the species.

The skull of *H. insolens* is long and low, the postorbital constriction very marked, sagittal crest slightly concave, the occiput overhanging and concave from side to side, the posttympanic process is long and massive approaching the postglenoid process and being produced as far inferiorly. The limb bones have stout shafts and relatively small extremities.

Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{2}{2}$, M $\frac{1}{1}$; the second upper premolar which is variable in the genus usually being absent in this species.

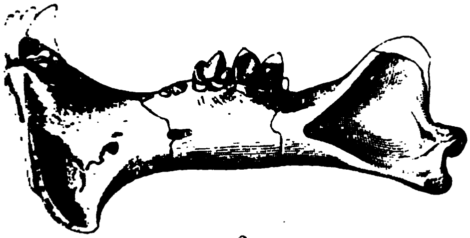
Length of skull, condyles to premaxillary border	190	mm.
Length of humerus	200	"
Length of ulna	212	"
Length of radius	160	"
Length of femur	250	"
Length of tibia	188	"
Length of pelvis	210	"

PLATE I



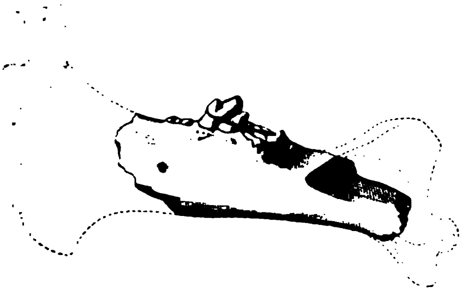
Adams on the Species of Hoplophoneus.

PLATE II.



2

3



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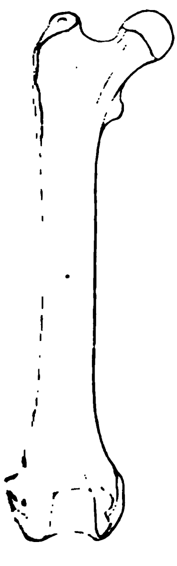

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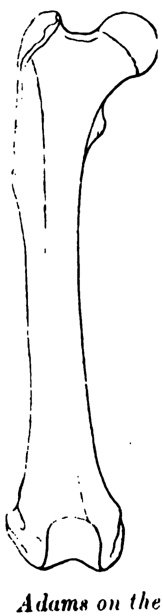

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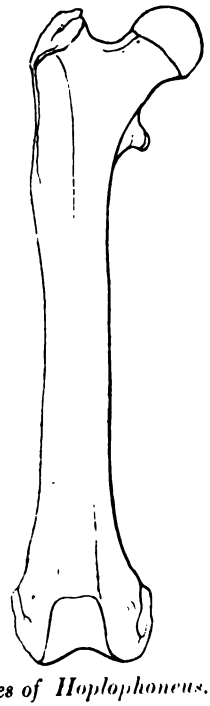
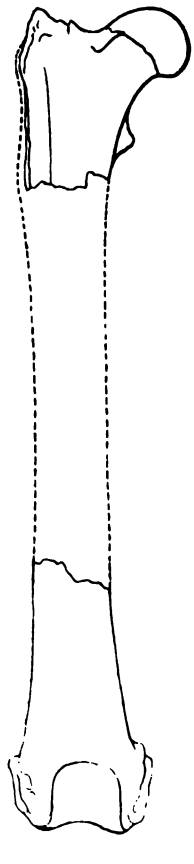


6



7

Adams on the Species of Hoplophoneus.

Hoplophoneus primaevus Leidy and Owen.

The original type of this species is figured in the The Ancient Fauna of Nebraska (1853). Later Leidy figured two skulls in the Extinct Fauna of Dakota and Nebraska, remarking that the larger one might be the skull of an old male, and that the original type was somewhat intermediate in size. The determination of the variation due to sexual characters seems impossible in the case of the extinct cats. However, the material which is available, shows that there are two types represented by these two skulls, the skeletons referable to the types differing more markedly than the skulls. Inasmuch as Leidy's original type agrees more closely with the smaller one, the difference being about such as is presented in any of the species of Machairodonts, it is taken as representative of *H. primaevus*. In the Princeton collection there is a fairly complete skull (number 11,013) and two nearly complete skeletons (numbers 10,741 and 10,934), with the latter skeleton there is also most of the skull. This makes it possible to correlate the skull with the skeletons and give the measurements of the species. The skull is short and high in the frontal region, the orbit horizontally oval, the posttympanic process short, the glenoid drooping considerably below it.

The skeleton is not rugose and the limb bones have slender shafts as in *Dinictis felina*. The dental formula is I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{3}{3}$, M $\frac{1}{1}$; the second superior premolar probably being constantly present.

Length of skull, condyles to premaxillaries, Leidy's type (approximately)	150 mm.
Length of humerus	160 "
Length of ulna	163 "
Length of radius	122 "
Length of femur	185 "
Length of tibia	143 "

Hoplophoneus robustus sp. nov.

This species is proposed as representative of Leidy's second type of *H. primaevus*. It has its most perfect type in the skeleton and skull (No. 650) determined as *H. primaevus* by Osborn and Wortman, the measurements of which were published in the American Museum Bulletin, Vol. VI, 1894, p. 228, along with those of *H. insolens* (*H. occidentalis*) and which I give here, adding the measurement of the skull. The species is represented in the Princeton collection by specimen

number 10,647, consisting of a fairly well preserved skull and mandible together with a humerus and portions of other limb bones. The skull is relatively large compared with the skeleton. The limb bones are rugose and have stout shafts, being very similar to those of *Dinictis fortis*,[1] and are thus very different from those of *H. primaevus*. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border	. 180	mm.
Length of humerus	. 170	"
Length of ulna	. 163	"
Length of radius	. 132	"
Length of femur	. 195	"
Length of tibia	. 160	"
Length of pelvis	. 180	"

Hoplophoneus oreodontis Cope.

This species is Cope's type of the genus. I introduce it here for the purpose of mentioning a complete skull in the Princeton Museum (number 10,515) which supplements the original type and is, therefore, used here for comparison. The approximate lengths of the femur and tibia are based upon the lengths of these bones associated with the type skull, the epiphyses being lost. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$ Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border, approximately :	. 135	mm.
Approximate length of femur	. 120	"
Approximate length of tibia	. 110	"

Hoplophoneus cerebralis Cope.

This species from the John Day is the smallest of the genus and at the sametime the most peculiar. Cope has pointed out its specific characters as follows: Space for the temporal muscle relatively short; brain capacity large; profile of the face very convex; sagittal crest horizontal; occiput vertical; no paroccipital processes; orbit vertically

[1] In my description of *D. fortis*, American Naturalist, June, 1895, I compared the skeleton with that of *H. occidentalis*, following the description of that species as given by Wortman and Osborn, which the foregoing determination of its skeletal characters shows to be incorrect. *D. bombifrons* which I described at that time I now find to be a synonym of *D. fortis*; the skull described being correlated with the skeleton and portion of a skull of *D. fortis* by means of specimen number 1400 of the American Museum.

oval. Dentition: I 3/, C 1/, Pm 2/′, M 1/, the third premolar being much reduced.

Length of skull, condyles to premaxillary border (approximately) . . . . 120 mm.

There are thus six species of *Hoplophoneus*, disregarding *H. strigidens* Cope, which being based upon a fragment of a canine exhibiting a peculiar form, is not characterized by any features which refer it to *Hoplophoneus* rather than any other genus. With the exception of *H. cerebralis* they are all from the White River. They present an interesting series both in the size of the skulls and skeletons. The accompanying series of femora give an idea of the relative characters of the skeletons of the larger members of the genus as regards size and strength. Unfortunately nothing is known of the skeleton of *H. cerebralis*, but judging from the size of the skull it would be the smallest of the series, although probably not much smaller than that of *H. oreodontis*. In restoring the femur of *H. occidentalis* I am indebted to Dr. Williston for information as to its length.

The series of skulls figured in outline when taken in connection with the series of femora give an idea of the relative size of the species. The gradation in size is for the most part comparable with the gradation in size of the skeletons. Each species has shown, from careful comparisons and measurements of all the available material, a limited amount of variation, but in no case losing its identity when both the skull and skeleton are taken into consideration.—GEO. I. ADAMS, Fellow of Princeton College.

EXPLANATION OF PLATES.

Plate I.

Fig. 1.—Hoplophoneus cerebralis (after Cope).
Fig. 2.—Hoplophoneus oreodontis (number 10,515 Princeton Museum).
Fig. 3.—Hoplophoneus primaevus (after Leidy).
Fig. 4.—Hoplophoneus robustus (number 650 American Museum).
Fig. 5.—Hoplophoneus insolens (number 11,022 Princeton Museum).
Fig. 6.—Hoplophoneus occidentalis (after Williston).

All $\times \frac{3}{8}$

Plate II.

Fig. 1.—Hoplophoneus occidentalis (Leidy's type).
Fig. 2.—Hoplophoneus occidentalis (number 1,047 American Museum).
Fig. 3.—Eusmilus dakotensis (after Hatcher).

Fig. 4.—Hoplophoneus primaevus.
Fig. 5.—Hoplophoneus robustus.
Fig. 6.—Hoplophoneus insolens.
Fig. 7.—Hoplophoneus occidentalis.

All $\times \frac{3}{8}$

The Goldbearing Quartz of California.—The salient characteristics of the gold quartz veins of California are briefly given by Mr. Waldemar Lindgren in a paper recently published, and the results of his observations are thus summarized:

"The auriferous deposits extend through the state of California from north to south, in an irregular and unbroken line.

"The gold quartz veins occur predominantly in the metamorphic series, while the large granitic areas are nearly barren. The contact of the two formations is not distinguished by rich or frequent deposits."

"The gold quartz veins are fissure veins, largely filled by silica along open spaces, and may dip or strike in any direction.

"The gangue is quartz, with a smaller amount of calcite; the ores are native gold and small amounts of metallic sulphides. Adjoining the veins, the wallrock is usually altered to carbonates and potassium micas by metasomatic processes.

"The veins are independent of the character of the country rock, and have been filled by ascending thermal waters charged with silica, carbonates and carbon dioxide.

"Most of the veins have been formed subsequent to the granitic intrusions which closed the Mesozoic igneous activity in the Sierra Nevada."

Regarding the origin of the gold, the author speaks with reserve. He points out the possibility of its derivation from the surrounding rocks, which theory, however, is not altogether satisfactory. He then states the following facts and the conclusion based upon them:

"First, the gold quartz veins throughout the state of California are closely connected in extent with the above described metamorphic series and that the large granite areas are almost wholly void of veins, though fissures and fractures are not absent from them.

"Second, that in the metamorphic series the gold quartz veins occur in almost any kind of rock, and that if the country rock exerts an influence on the contents of the veins, it is, at best, very slight.

"Third, that the principal contact of the metamorphic series and the granitic rocks is in no particular way distinguished by rich or frequent deposits.

"It is further apparent that gold deposits have been formed at different periods, though, by far, most abundantly in later Mesozoic times. Some of these later veins may have been locally enriched by passing through earlier impregnations in schist or old concentrations in the sandstones and conglomerates of the metamorphic series, the gold contents of which have, however, only been proved in isolated cases.

"These considerations strengthen the belief that the origin of the gold must be sought below the rocks which now make up the surface of the Sierra Nevada, possibly in granitic masses underlying the metamorphic series." (Bull. Geol. Soc. Am., Vol. 6, 1895.)

Precambrian Sponges.—M. L. Cayeux has published a preliminary note on the spicules of sponges found in the Precambrian beds of Bretagne. The author describes the different forms of the spicules, gives their dimensions, the mode of fossilization, and the probable causes for their fragmentary condition. The principal conclusions derived by M. Cayeux from his researches are (1) numerous spicules of sponges of various species are found in the Precambrian phtanite formations of Bretagne, and (2) that all the orders of sponges with silicious skeletons are represented in these formations.

A resume of the facts ascertained concerning this interesting fauna is given by the author as follows:

"It is impossible not to be struck by the ensemble of the sponges of the phtanites of Lamballe. Even excluding all the spicules which, although they certainly are sponges, yet are too fragmentary for exact identification, there remains an assemblage of forms which points to a very complex fauna.

"In the light of our present knowledge this fauna appears to be composed of Monactinellidæ, probably abundant, Tetractinellidæ, relatively rare, numerous Lithistidæ, and a few Hexactinellidæ. All the orders of Silicea are represented. The branching off of the sponges is then plainly as early as the base of the Precambrian of Bretagne.

"The oldest beds in which any remains have been found belongs to the Archean of Canada. M. G. F. Matthew has described *Cyathospongia? eozoica* from the Lower Laurentian of St. John (New Brunswick) and *Halichondrites graphitiferus* from the Upper Laurentian of the same region.

"*Cyathospongia? eozoica* may be a species of Hexactinellidæ, and *Halichondrites graphitiferus* must be referred either to Monactinellidæ or to Hexactinellidæ. The authenticity of these fossil sponges has been put beyond a doubt by M. Hermann Rauff.

"All the great groups of silicious sponges do not figure in this assemblage, but the fauna presents this character worthy of note, that the Lithistidæ and the Hexactinellidæ, that is to say, the sponges which have the most complex skeleton occupy a prominent place.

"I have called attention to these Cambrian sponges to show that there is no fundamental difference between the Precambrian and the Cambrian sponge fauna. In the one as in the other, we find already traced, the lines along which the future silicious sponges are developed." (Annales Soc. Geol. du Nord T., XXIII, 1895.)

Embryology of Diplograptus.—A large collection of specimens of Graptolites found near Dolgeville, N. Y., furnishes Mr. R. Ruedeman the data for a paper on the mode of growth and development of the genus Diplograptus. The species, *D. pristis* Hall, and *D. pristiniformis* Hall, appear as compound colonial stocks instead of single stipes, as hitherto known. From his observations the writer infers that the colonial stock was carried by a large air bladder, to the underside of which was attached the funicle. The latter was enclosed in the central disc, and this was surrounded by a verticil of vesicles, the gonangia, which produced the siculæ. Below the verticil of gonangia and suspended from the funicle was the tuft of stipes.

It is evident from the structure of these graptolites that the genus Diplograptus has the combined properties of different groups, and gives valuable hints in regard to their common ancestry. The investigation of Mr. Ruedeman is one of the most important recent acquisitions of paleontologic embryology. (Am. Journ. Sci., 1895, p. 453.)

The Upper Miocene of Montredon.—M. Ch. Deperet has just published the results obtained through the excavations he has been making in the hill of Montredon near Bize (Aude), The fossils which he has collected are found also in the peat beds where they are much broken and slightly worn, and in the white marls where he has found more complete specimens, such as skulls and parts of limbs with the bones in proper relation.

Notwithstanding an abundance of fossils, the fauna of Montredon, until now, was characterized by a paucity of species, comprising only Dinotherium, Hipparion, a Rhinoceros and an undetermined Ruminant. The discoveries of M. Deperet have increased the known vertebrates to twelve. There are, in addition to the animals just mentioned, a wild boar, agreeing with *Sus major* of Leberon; three ruminants, *Tragocerus amalthæus*, *Gazella deperdita*, and Micromeryx; three carnivores, *Si-*

mocyon diaphorus, Dinocyon, Hyænarctus arctoides. This last constitutes, says the author, a true intermediate type between Hyænarctus of the Miocene and the bears of the Pliocene, as *Ursus arvernensis* and *Ursus etruscus.* M. Deperet adds that the discovery of this animal fills a gap by revealing in a precise manner the ancestral relation of the bear type. (Revue Scientif., 1895, p. 375.)

BOTANY.[1]

The ViennaPropositions.—(*Continued from page 1100, Vol. XXIX.*)—In a succeeding number of the same journal, Dr. Kuntze replies to the foregong article at some length. A considerable portion of the reply is taken up with personalities. This is not without provocation, for Ascherson and Engler have grievously misrepresented him in more than one place in the foregoing article, e. g., in the matter of his proposed 100-year limitation, and his comparison of the changes required by 1737 and 1753—as one can readily see by glancing at Revisio Generum 3[1]. Indeed, they substantially concede the injustice of their accusation as to Knntze's statement with reference to the changes required by 1753, a few paragraphs beyond, when they discuss their proposed limitation of fifty years. The anonymous correspondent of the *Journal of Botany* who was so pained at the supposed bitterness prevailing in America, is respectfully referred to the pages of the *Oesterreiche Botanische Zeitschrift* for an example of the state of feeling in other lands.

The following extracts will give an idea of Dr. Kuntze's reply.

Of the six propositions of Ascherson and Engler he says: "Numbers 1–4 are not new; No. 5 is a *principium inhonestans,* and No. 6 a supplement to No. 5. The new principle is a year limitation proposal with retroactive force. I had previously proposed a limitation of 100 years only for names sought to be revived in the future, which would only affect old names which are mostly doubtful and undetermined, so that by my proposed limitation, the doubtful cases would be disposed of and greater stability of nomenclature brought about. By the proposition of Messrs. Ascherson and Engler on the other hand, acquired rights would be violated. The gentlemen, indeed, in their last account no longer recognize this right, even as little as the right o political legitimism. These gentlemen now reject also the law o

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

priority, and their proposals have never conformed to the Paris code. One must ask involuntarily what laws Messrs. Ascherson and Engler do recognize in nomenclature at all. With the best intentions, I cannot perceive any trace of a 'Rechtsboden.'"

"The Paris code" he continues, "is in my opinion better than the proposals and deviating principles which Engler, Ascherson and Pfitzer suggest and which they themselves follow only in part. Supposing one followed out the deviating principles honestly and consistently, many more name alterations and complications would result than through following the Paris code."

Since Ascherson and Engler have been at some pains to expose what they deem fundamental errors, one may well suggest a fundamental error upon which they proceed. Their whole argument is based upon the notion that there is a current nomenclature. It is this very notion, indeed, which creates a large part of the opposition to all systematic attempts to bring order into nomenclature. When a systematist goes goes about the work of adjusting the nomenclature of his particular group, current nomenclature does not trouble him at all. There he sets about him with vigor, and even, perhaps, in accordance with rule and principle. But as he looks about him beyond the range of his own group, he feels that it would be very convenient if names could stand as they are in the nearest book at hand, and he becomes conscious of something which he calls current nomenclature. It may be safely affirmed that if Dr. Kuntze had taken up a small group and worked out its nomenclature with the care and thoroughness he bestowed upon all the Phanerogams, no one would have made more than a passing objection, and before long his names would have found themselves current. Who ever said anything about the radical changes made in the nomenclature of the *Uredineæ* when Winter and afterwards Schroeter replaced name after name by the old specific names of *Æcidium* and *Uredo* forms? Very little that Dr. Kuntze has done is more radical than that—and their changes are as current as anything can be said to be at the present day. Before we set about preserving a current nomenclature, we must produce one, and that can only be done by adhering consistently to rules.

As to the propositions made by Escherson and Engler, not much need be said. The 5th and 6th are avowedly only another form of the discredited 4th Berlin thesis. The whole object of the authors seems to be to save their list of eighty-one names—if not by one means then by another. They are as radical as the best of us as far as specific nomenclature is concerned, and one might well suggest that their attitude

towards the eighty-one names they are bent on saving at all hazards, savors quite as much of "legitimism" as anything in the nomenclature controversy. Moreover the propositions are by no means as easy of application as they might appear. The work of restoring prior names has been going on pretty steadily for many years. Since 1891 it has gone on quite rapidly. Are the names restored since the reform movement began to stand, or are we to add a 7th proposition, something like this: "No name recognized since 1891 is to be deemed withdrawn from the operation of the 5th rule?" Then again it must be decided what shall be considered "use" of a name. If a name appears in a work of wide circulation there is a presumption that it has been used more or less. How many other works must cite it to give it validity? And must they cite it with approval, or will citation as a synonym or without comment suffice? What sort of works shall be referred to to ascertain whether a name has been used? Are names used in catalogues and printed lists used? If a writer publish two books, say five years apart, and cite his own names, if one of the books comes within the limit, have the names he quoted from himself been used? Or must some other author use them? The room for individual eccentricity in the application of such a rule is too great to make the rule practicable.

Besides what need is there of pretending to begin the nomenclature of genera with 1753, when in fact it is begun with 1845? As Ascherson and Engler point out, their limitation substantially makes it immaterial whether the nominal starting point is 1753 or 1690. The labored distinction between generic and specific nomenclature amounts to very little. It is only partially true that the alteration of a generic name entails the alteration of the name of every species in that genus. Under the Kew Rule it might, perhaps, but otherwise it can scarcely be said that a change of a generic name burdens the memory any more than the change of a specific name. So long as the distinguishing portion of the binominal remains unchanged, each new binominal does not have to be learned over.

In conclusion, without going into the merits of the controversy between Kuntze and Ascherson and Engler, I may say that Dr. Kuntze never hides behind vague general statements, but supports his assertions by citations and actual instances, so that they may be verified Whether one accedes to Kuntze's conclusions or not, he may always know upon what they are based. It would be much easier to determine the value of the assertions made by his opponents if they were in the habit of doing the same. It is easy to declaim against "disagreeable alterations" and to make insinuations as to the motives of the reform-

ers. But the fact remains that Dr. Kuntze has only attempted to do, a little radically perhaps, for all the flowering plants at one stroke, what monographers had been doing piecemeal in every group of the vegetable kingdom. No one objected to their motives, and few to their alterations. Their alterations became a part of "current nomenclature." Had the reform been conducted haphazard and piecemeal, it would have seemed quite proper to many who now vigorously denounce it.—ROSCOE POUND.

The Flora of Ohio.—In the "Catalogue of Ohio Plants" in Vol. VII of the Geology of Ohio, Professor W. A. Kellerman and W. C. Werner make an admirable contribution to our knowledge of the plants of one of the older regions west of the Allegheny Mountains. The catalogue is prefaced by twenty pages or so of historical matter in which we learn that the earliest catalogue of Ohio plants (Miami County) was prepared in 1815 by Dr. Daniel Drake; this was followed in 1818 by a paper on the Scenery, Geology, Mineralogy, Botany, etc., of Belmont County, Ohio, by Caleb Atwater in the *American Journal of Science* (Vol. I), and later, 1831, by Short and Eaton's paper (Southern Ohio) and two by Riddell,—Franklin County, in 1834, and the Flora of the Western States, in 1835, to which a supplement was added in 1836. Then follow lists by Sullivant (1840), Bigelow (1841), Lea (1849), Clark (1852 and 1865), Lapham (1854), Klippart (1858 and 1860), Newberry (1859), Hussey (1872), Beardslee (1874), Wright (1889), besides many short papers in periodicals.

Following the introductory pages one comes at once to the enumeration of plants, in which the arrangement of the families is that of Engler and Prantl, but oddly enough—in *reversed order*. Why the authors gave themselves the trouble to invert the natural sequence is not stated. It is awkward, to say the least. We notice with pleasure that the revised nomenclature has been used, and that all specific names have been decapitalized. Double citations of authorities are given when necessary, and varieties are given as trinomials. Altogether the catalogue is a modern one in plan and execution.

After the Angiosperms, there follow the Gymnosperms, Vascular Cryptogams, Bryophyta, Hepaticæ, Lichenes, Fungi, Algæ and Myxomycetes. Of the last six groups the authors state that the list "must be considered very fragmentary and a mere beginning," yet this is an excellent beginning, of which the State of Ohio needs by no means to be ashamed.—CHARLES E. BESSEY.

The Flora of the Sand Hills of Nebraska.—Mr. P. A. Rydberg has recently published in the Contributions from the U. S. National Herbarium (Vol. III, No. 3) the results of his careful exploration of the Sand Hills of Central Nebraska in the year 1893. Two or three counties in about the center of the sand hill region were selected as the ground to be thoroughly studied, and three months were given to this limited area. Two streams transverse this area, the Middle Loup River and the Dismal River. The former is a rapid stream running down a slope of 8½ to 13 feet to the mile, with hills from 200 to 300 feet high on each side of the rather wide valley (½ to 1½ miles). In its narrower portions the valley is filled with lagoons and swamps, the remains of old river beds. The Dismal River runs through a narrower valley, and the bluffs are higher, ranging from 300 to 600 feet. Away from the rivers Mr. Rydberg found three kinds of sand hills, the first of these are called by him the "barren sand hills," not because they are without vegetation, for they are not, but because they are at present of very little use to man. Here one finds the true Sand Hill vegetation, and when seen from the higher points "the hills appear likes the billows of the ocean."

The Dry Valley Sand Hills constitute the second kind. The hills are long ridges running mostly east and west with long valleys between. The underground drainage is so perfect that little or no water gathers in the valleys, but their rich soil readily yields good crops, or excellent pasturage.

The Wet Valley Sand Hills differ from the last in the greater abruptness of the ridges, which are, in fact, sometimes impassable, and in the less perfect drainage, ponds of water generally occurring at the easterly end of the valleys. In no case is there "surface drainage," every pond being destitute of an outlet. About these ponds grasses grow luxuriantly.

It is evident that the Sand Hill flora is not a homogeneous one. The plants growing along the rivers and about the ponds are very different in character from those which occur on the wooded summits of the "barren sand hills," or the steep slopes of the hills which border the dry and wet valleys. In summing up a discussion of the matter, Mr. Rydberg says: "The most characteristic plants of the sand hills are the four blowout grasses, *Calamovilfa longifolia*, *Eragrostis tenuis*, *Redfieldia flexuosa*, *Muhlenbergia pungens*, of which the first two are found on nearly every sand hill. Next to these the following are the most common or characteristic herbaceous plants:

Andropogon scoparius
Andropogon hallii
Stipa spartea
Stipa comata
Psoralea lanceolata
Psoralea digitata
Carduus plattensis
Opuntia rafinesquii
Euphorbia petaloidia
Euphorbia geyeri
Chrysopsis villosa
Cristatella jamesii
Corispermum hyssopifolium
Croton texensis
Acerates viridiflora
Acerates angustifolia
Acerates lanuginosa
Astragalus ceramicus longifolius
Commelina virginica
Tradescantia virginica
Yucca glauca
Amaranthus torreyi
Frœlichia floridana
Cyperus schweinitzii
Laciniaria squamosa
Cycloloma atriplicifolia
Argemone albiflora

"The most abundant woody plant is *Amorpha canescens*, which is common all over the sand hills. Next comes the Western Sand Cherry (*Prunus besseyi*). On the sand hills around Thedford the third in order is *Ceanothus ovatus*. *Kuhniastera villosa*, which should, perhaps, be classed among the undershrubs, is as common as any of the class. All these belong to the true sand hill flora. Nearly all the other woody plants are confined to the Middle Loup and Dismal River Valleys. A few, as for instance, *Salix fluvialilis*, *Symphoricarpus occidentalis*, *Prunus americana*, *Amorpha fruticosa* are also found in some of the wet valleys."

The other woody plants along the streams are *Cornus stolonifera*, *Ribes floridum*, *Rhus radicans*, *Rosa fendleri*, *Rosa arkansana*, *Ribes aureum*, *Rhus trilobata*, *Acer negundo*, *Fraxinus pennsylvanica*, *F. pennsylvanica lanceolata*, *Populus deltoides*, *Celtis occidentalis*, *Juniperus virginiana*, *Parthenocissus quinquefolia*, *Vitis vulpina*, *Celastrus scandens*, *Rubus occidentalis*, *Ribes gracile*, *Crataegus coccinea*, *Ulmus americana* and *Rhus glabra*.—CHARLES E. BESSEY.

Recent Botanical Papers.—Dairy Bacteriology by Professor H. W. Conn comes to us from the U. S. Department of Agriculture, giving the results of the author's work the past three years.—From the same source we have papers on Grass Gardens and Alfalfa, by Jared G. Smith; Fertilization of the Soil as affecting the Orange in Health and Disease, by H. J. Webber; The Grain Smuts, their Cause and Prevention, by Walter T. Swingle; Water as a Factor in the Growth of Plants, by B. T. Galloway and A. F. Woods; Forestry for Farmers, by B. E. Fernow.—From the Proceedings of the Iowa Academy of

Sciences we have Pollination of Cucurbits, Diseases of Plants at Ames in 1894, and Distribution of Some Weeds in the United States, by Professor L. H. Pammel.—Dissemination of Plants chiefly by their Seeds, is the title of a pamphlet of fifteen pages based upon the specimens collected by the lamented young botanist Miss Mary E. Gilbreth, and after her death presented to Radcliffe College. It will prove to be very suggestive to those who wish to prepare similar collections.—"A Guide to find the names of all wild-growing Trees and Shrubs of New England by their Leaves," and "Ferns and Evergreens of New England," are two pamphlets by Edward Knobel, which deserve to be widely used in the public schools. They consist of good figures of the leaves, which should make it possible for even the non-botanical teacher to direct the attention of children to the trees and ferns. They are sold by Bradlee Whidden of Boston for fifty cents each.—We may notice here the beautiful photogravures of fungi issued by C. G. Lloyd, of Cincinnati, Ohio; the last numbers are *Coprinus comatus*, *Crucibulum vulgare*, *Lycoperdon separans* and *Urnula craterium*.—Professor T. A. Williams has published (Bulletin 43, Agricultural Experiment Station) a paper upon the Native Trees and Shrubs of South Dakota, in which he lists 37 trees and 80 shrubs. Of these, twelve trees and thirteen shrubs are found in all regions of the State. In the Black Hills, a small region including not more than one-eighth of the whole area of the State, no less than eighty-two of the one hundred and seventeen trees and shrubs are found.—Professor MacDougal writes on Botanic Gardens in the October *Minnesota Magazine*. A half tone illustration of the Botanic Institute at Leipzig, and another of the Botanic Garden at Buitenzorg, Java, accompany the paper.

VEGETABLE PHYSIOLOGY.

Changes Due to an Alpine Climate.—For ten years M. Gaston Bonnier, of Paris, has carried on experiments in various parts of France to determine just what changes occur in plants when they are transported from the lowlands to high elevations. These are described in a bulky paper in *Annales des Sciences Naturelles*: *Botanique*, Sé. VII, T. 20, Nos. 4, 5, 6, entitled Recherches expérimentales sur l'adaptation des plantes au climat alpin. Plants of many genera were removed from the plains, the roots or root-stocks divided into equal parts, and these parts set in similar soil and situations at various elevations, up to several thousand metres, in the Alps and the Pyrenees,

and examined from time to time for anatomical and physiological changes. These soon made their appearance and were as follows, the changes in the plants exposed to the alpine conditions being attributed principally to (1) More intense light; (2) Drier air; (3) A lower temperature. *Change of form and structure:* (1) The subterranean parts as a whole are relatively better developed than the parts above ground. (2) The rhizomes and the roots show little modification, except that the calibre of the vessels is generally smaller and the bark more precocious; (3) The ærial stems are shorter, more hairy, more spread out, closer to the soil and with shorter and less numerous internodes; (4) In general the stems have a cortical tissue that is less thick in proportion to the diameter of the central cylinder; the epidermal cells have thicker walls and the cuticle is more pronounced; often the epidermis is reinforced by a certain number of sub-epidermal layers; the different tissues of the central cylinder are ordinarily less differentiated; when bark exists, it appears earlier and is relatively thicker on branches of the same age; when there are secretory canals, they are relatively, or even absolutely, larger; finally, the stomata are more numerous; (5) Usually the leaves are smaller, except sometimes in sub-alpine regions, more hairy, thicker in proportion to their surface and often absolutely thicker, and deeper green by reflected or transmitted light; (6) The blade of the leaf acquires tissues better suited for assimilation; the palisade tissue is more strongly developed, either by a narrowing and elongation of its cells or by a considerable increase in the number of rows, the cells also contain a greater number of chlorophyll bodies and often each grain of chlorophyll has a greener tint. when there are secretory canals the diameter is relatively or absolutely greater; the epidermis of the leaf shows less differences than that of the stem, nevertheless, in general it is better developed, especially on persistent leaves, which have besides better developed protective sub-epidermal cells; the cells of the epidermis are ordinarily smaller and often the number of stomata per unit of surface is greater, especially on the upper face as M. Wagner was the first to show; (7) The petiole shows modifications generally analogous to those of the stems but much less pronounced; (8) The flowers are relatively much larger and sometimes even absolutely larger; they are more brightly colored and when the color is due to chromoleucites it is the same as in case of the chlorophyll grains, the number in a cell is greater, and often each chromoleucite is of a deeper color; the heightened color occurs also when it is due to substances dissolved in the cell sap. Experiments during eight years with *Teucrium* also show that modifications acquired

by the plant when it is taken from the plain to the mountain, or vice versa, disappear at the end of the same time when the plant is put back into its own climate. *Modification of functions*: (1) If a plant grown on the mountains is transported immediately to the level of one grown on the plains (both originally from the same root) we find for the same surface and under the same conditions, the chlorophyllian assimilation and the chlorovaporisation are more intense in the leaves brought from the alpine region; (2) If the respiration and the transpiration in the dark are compared in the same way, we find that for equal weights these functions have about the same intensity, or are less in the alpine specimens. The paper contains numerous wood cuts showing anatomical details and eleven lithographic plates comparing alpine and lowland individuals of the same species. The last is a double plate in color, illustrating the brighter hues of the mountain flowers. Foot notes refer to the principal literature.—ERWIN F. SMITH.

Spore Formation Controlled by External Conditions.—Einfluss der ausseren Bedingungen auf die Sporenbildung von Thamnidium elegans Link, by Johann Bachmann, is the title of the leading paper in *Botanische Zeitung* for July 16, 1895. *Thamnidium elegans* is a graceful little mould bearing two sorts of sporangia. The sporophore consists of a slender upright stalk, 2–4 cm. high and usually terminated by a single large sporangium, having a columella and bearing many spores. Midway down the sporophore there are usually one to ten or more whorls of branches which ramify dichotomously, often as many as ten times, the terminal divisions bearing singly on their ends small sporangia (sporangiola) generally only 6–8 μ in diameter and containing only a very few spores, usually 1–4. Sometimes only the end sporangium develops and sometimes only the dichotomous sporangioliferous branches; but the cause of this variation which is undoubtedly what led De Bary into the error of supposing Thamnidium a stage in the development of *Mucor*, has remained unknown. By varying his culture media Bachmann has discovered that he can at will produce sporophores with or without end sporangia and with or without sporongiola; in the same way he has been able to change the tiny sporangiola, which frequently bear only a single spore, into big sporangia provided with a columella and bearing many spores. As the result of his experiments he divides the fungus into six types as follows: (1) End sporangium present; sporangiola appearing very early on finely dichotomous branches which may reach the tenth subdivision, spores few. This form occured on more than a dozen differ-

ent media, the best results being obtained from the following: fresh, damp horse dung; dung decoction; agar-agar with 2½ per cent peptone; agar-agar with 4 per cent peptone and 0.5 per cent nitrate of potash. (2) End sporangia present; sporangiola 16–60 μ in diameter, with numerous spores and frequently with a columella and partial swelling up of the membrane. This type was obtained in nine different media, including the following: thoroughly cooked plums; damp bread; eggs; oranges; malt. (3) Only the end sporangium present. Obtained on slightly cooked plums and on 1 volume of malt extract in 2 vol. water. (4) Only the sporangiola present. Obtained in various culture media by raising the temperature to 27–30° C. (5) *a.* Mycelium with thick ends and gemmæ. This form was obtained in the following media: plum decoction with peptone; 1 vol. grape must in 4 vol. water with peptone; 1 vol. malt extract in ½ vol. water. *b.* Mycelium with fine ends and without gemmæ. Obtained in the following fluids: 1 per cent nitrate of potash with 1 per cent Nährlösung; almond oil with Nährlösung; oleic acid with Nährlösung; cane sugar in various percents. (6) Formation of zygospores. Not observed. According to the author, *Th. elegans* is the only fungus known which can be induced to form this or that sporangium, or none at all, by means of purely external, known conditions. He believes the production of the first type is due to substrata in which nitrogenous substances preponderate and fats and carbohydrates are present in only small qnantities, and that the second type is due to the reverse of these conditions. The paper contains 24 pages and is illustrated by a double plate.—ERWIN F. SMITH.

Germination of Refractory Spores.—In spite of every effort, it occasionally happens that the spores of a fungus refuse to germinate either in water or artificial media. This is true of various oospores, teleutospores and ascospores, and particularly and notably of the basidiospores of the whole group of the Gastromycetes, scarcely anything being known of the early stages of species of this group, owing to this fact. Recently, Dr. Jacob Eriksson, of Stockholm, has tried cold on a number of uredospores and æcidiospores with partial success. His method consists in placing the spores for several hours on blocks of ice or in a refrigerator at temperatures ranging down to minus 10° C. In a number of instances spores which refused to germinate in water at room temperatures, either wholly or in great part, did so freely and speedily after being on ice or in a refrigerator. In other cases the cold appeared harmful or without sensible influence, even on the same species. The opinion has been current for a long time that sudden great

changes in temperature favor the development of rust in cereals but usually this has been attributed to the indirect influence of cold in causing a deposit of dew in which the spores could germinate. In the light of these experiments this explanation can hardly be the true one. Spores which refused to germinate after lying in water several days germinated readily after exposure to cold. It would seem as if the cold were capable of stimulating the spores to germinate only when the latter have been rendered receptive by exposure to rainy weather, but further experiments and observations are necessary. It is at least certain that the spores of *Æcidium berberidis*, which germinated badly after cooling, were gathered in dry weather, while those which germinated abnndantly after cooling were gathered (on three different occasions) after several rainy days. The fungi tried by Dr. Eriksson were *Aecidium berberidis*, *Ae. rhamni*, *Ae. magelhænicum*, *Peridermium strobi*, *Uredo glumarum*, *U. alchemillæ*, *U. graminis* and *U. coronata*. The original paper, entitled Ueber die Förderung der Pilzsporenkeimung durch Kälte, may be consulted in *Centralblatt fur Bakteriologie und Parasitenkunde*, *Allg.*, Bd. I, p. 557.—ERWIN F. SMITH.

Botany at the British Association.—The presidential address of W. T. Thistleton Dyer before the new Section K (Botany) of the British Association at the Ipswich meeting (*Nature*, Sept. 26, 1895) is an exceedingly well written and interesting paper and one likely to obtain a wide reading. It deals with such topics as the following: Retrospect, Henslow, botanical teaching, museum arrangement, old school of natural history, modern school, nomenclature, publications, paleobotany, vegetable physiology, assimilation, and protoplasmic chemistry. The two and a half columns of sensible remarks on botanical nomenclature are specially commendable to American readers, as also what is said on teaching and in the last three topics of the address. It is certainly a surprise to learn that cramming for examinations from printed texts should be so largely taking the place of the careful study of plant phenomena in many English schools, the tendency in this country of recent years being happily in the other direction.—ERWIN F. SMITH.

Nitrifying Organisms.—Messrs. Burri and Stutzer, of the agricultural experimental station in Bonn, have discovered a bacillus (See *Centrb. f. Bak. u. Par. Allg.*, Bd. I, No. 20–21, 1895) capable of changing nitrites into nitrates and in many respects resembling Winogradsky's organism, but which *grows readily in bouillon and on gelatine.* This bacillus is much larger than the measurements given by Wino-

gradsky; like his it is incapable of converting salts of ammonia into nitrate, but unlike his is motile (when taken from colonies on gelatine or silicates), stains readily, causes slow liquefaction of gelatine, and is not yellowish but varies from colorless to bluish when grown on sillicates. The chemical activity is almost exactly the same as that of Winogradsky's bacillus and these authors, who have been studying the subject for two years, seem to think that it may after all turn out to be the same organism, the differences being less important than would seem at first sight, and resting perhaps on incomplete observations. The most important distinction appears to be the ability of this organism to grow on organic substances, but it does not appear from Winogradsky's publications whether he tried to transfer his organism from sillicate-plate cultures to bouillon, or gelatine, and failed.—Erwin F. Smith.

Relation of Sugars to the Growth of Bacteria.—Unquestionably the most discriminating and important paper that has yet appeared on this subject is a recent one, Ueber die Bedeutung des Zuckers in Kulturmedien für Bakterien (*Centrb. f. Bak. u. Par., Med.*, Bd. XVIII, No. 1), by Dr. Theobald Smith, now of Harvard. Reference is made to the literature of the subject but this is contradictory and many of Dr. Smith's interesting conclusions are largely or wholly the result of his own laborious and brilliant researches. The propositions are stated clearly and it is safe to say that hereafter no one will undertake the study of bacterial fermentation and gas production without first consulting this paper. The author's summary is as follows, but many things are not mentioned in this and the whole paper will repay the careful perusal of all who have groped about in this field of bacteriology: (1) In ordinary meat bouillon, souring and gas formation are only observed when sugar is present. Dextrose is the sugar most commonly attacked and muscle sugar is probably identical with it. (2) The formation of acid results from the breaking up of the sugar; the formation of alkali in the presence of oxygen results, on the contrary, from the multiplication of the bacteria themselves. So far as tested, the production of acid is common to all anærobic bacteria (facultative or obligate). (3) Facultative anærobiosis is made possible by the presence of sugar. (4) Rauschbrand and tetanus bacilli grew in fermentation tubes only when sugar was present. In test tubes containing the same sugar bouillon multiplication was never seen. (5) As far as tested, all gas-forming species produce along with CO_2 an explosive gas. (6) Souring as well as the production of gas are valuable diagnostic

characters, when at least three sorts of sugar are tested (with exclusion of muscle sugar). (7) Not only must the formation of gas be determined but also the progress of the same, the total quantity, and the quantity of CO_2. (8) For the differentiation of species and varieties it is of value to determine by titration the total amount of acid in 1 per cent sugar bouillon, as well as the germicidal power of such cultures on the bacteria themselves. (9) The division of bacteria into acid and alkali producers must be given up and the conditions govering the production of acid investigated more critically for each species. (10) The existence of fermentable carbohydrates in the digestive tract and in the fluids of the body is probably very favorable to the establishment and multiplication of pathogenic bacteria (both facultative anærobic and obligate, especially the latter).—ERWIN F. SMITH.

Algal Parasite on Coffee.—Under the title *Cephaleurus coffeæ*, eine neue parasitische Chroolepidee, Dr. F. A. F. C. Went describes in *Centrb. f. Bak. u. Par., Allg.*, Bd. I, No. 18–19, 1895, p. 681, an alga which he has found attacking the Liberian coffee at Kagok-Tegal in Java. This parasite appears on the leaves and berries in the form of round orange-brown spots which look bristly to the naked eye. The alga not only forms a thallus on the surface but sends its threads deep into the intercellular spaces of the host. The presence of the parasite in and on the leaf causes an interesting, protective hypertrophy of the surrounding tissue, the further progress of the alga being soon limited by a dense encircling mass of thick-walled, non-lacunose tissue, developed out of the palisade cells and spongy parenchyma of the leaf. No algal threads were found in this tissue. The berry not being able to defend itself in this way suffers most, becoming gradually brown and finally black and wrinkling and drying prematurely, so that the seed does not ripen. All parts of the alga are subject to the attacks of a fungus, which also appears to be capable of growing in the berries apart from the threads of the alga, but the relation of which to the latter and to the causation of the disease is left by the author in a rather unsatisfactory state. The paper is accompanied by a lithographic plate showing details of the alga and sections of the normal and hypertrophied tissue.—ERWIN F. SMITH.

ZOOLOGY.

On Bodo urinarius.—Although the discovery of certain peculiar infusoria in human urine dates so far back as 1859, but little is known of these animalculæ. M. Barrois has been investigating the subject

and has recently published his conclusions. According to his account Hassall was the first to detect this microscopic creature in its chosen habitat. He described it under the name of *Bodo urinarius*, as an animalcula $\frac{1}{1800}$ inch long and $\frac{1}{3000}$ inch wide, of rapid motion, generally round or oval, presenting a granular appearance, sometimes they are broader at one end. The long lashes, by means of which they move, are variable in number and proceed when there are two or three to each animalcule from opposite extremities; reproduction by longitudinal fission. In 1885 Kunstler found "small monads . . . . flagellate, transparent and very active . . . . probably *Bodo urinarius*."

In reviewing the subject, M. Barrois gives detailed accounts of these discoveries, and of the condition of the urine in which they appear. He then describes his own methods of investigation, and compares the drawings of specimens, after Hasslar and Kuntsler, with the infusoria he himself had found existing under similar conditions as those described by the authors mentioned. M. Barrois lays particular stress upon the fact that the infusoria found by him, only appeared in urine plainly alkaline, which contained animal matter (broken down epithelial cells, pus, albumen), and which had been exposed sometime to the air. In no case did he find them in fresh urine. Hassall's notes show a similar set of conditions in his case. Kunstler, however, claims to have found the infusoria in fresh urine in company with several species of bacteria. M. Barrois is of the opinion that Kunstler was deceived as to the age of the urine given him for examination, since in all other respects the conditions (as to animal matter, etc.) agree with those of Hassler and the author. In view of these conditions M. Barrois does not agree with the statement made that *Bodo urinarius* is a parasite. He is rather of the opinion that it exists in the air in a spore-like form ready to develop whenever it is brought in contact with a suitable nidus. This it finds in urine conditioned as above described.

In the course of his discussion, M. Barrois refers to *Trichomonas vaginalis* Douné, found by Salisbury in the urine and vaginal mucous of a young girl aged sixteen, supposed to be parasitic, and to certain Trichomonads found by Marchand and also by Miura; in all probability *T. vaginalis*. In the two latter cases, the infusoria was found living in freshly voided urine, so it would appear to be a true parasite. In both cases the urine was loaded with decomposing matter.

By an ingenious experiment, Miura demonstrated that the Trichomonads lived in the urethra only, and was not found in the bladder.

As to the classification of the Monads, M. Barrois considers it extremely unsatisfactory, since it is based on the number and disposition of

the flagella. In fact, *Bodo urinarius*, by reason of its polymorphism, can have no place in such a scheme of classification.

In conclusion, the author compares *Bodo urinarius* with *Oekomonas mutabilis* Saville-Kent, which propagates both from spores and by fission in infusions of vegetable matter, and also with *O. rostratum* Sav.-Kent, found in both fresh and salt water containing vegetable debris. He finds the three species so similar in appearance, that one might infer that their only difference is in their habitat.

M. Barrois repeats, as a final statement, that *Bodo urinarius* Kunstler (=*Cystomonas urinaria* R. Bl.=*Plagiomonas urinaria* M. Braun) can hardly be given a place among the parasites of man. (Revue Biol., Feb., 1895.)

Influence of the Winter 1894-1895 upon the Marine Fauna of the Coast of France.—M. Pierre Fauvel calls attention to the considerable influence which the exceptional lowering of temperature, and long duration of cold, during the last winter, exercised upon the marine fauna of the coasts of France.

Sharp frosts, at the time of high tide, would destroy innumerable quantities of animals that the ebb tide would leave exposed. Annelids, Actinans and Fish were found dead or unconscious, paralyzed by the cold. This mortality, strange to say, extended to depths which the change of temperature could not have affected directly.

Another effect of the cold has been to bring in shore animals ordinarily seen in deeper water, and also certain species very rare or entirely unknown in our fauna. The Spring was marked by an extraordinary abundance of *Balanus porcatus*, which covered with a continuous bed the surface of the boulders and rocks, and by the return of the Mussels which had nearly disappeared. During some weeks *Mytilus edulis* took possession of all the rocks exposed to the southwest wind and formed veritable "moulières" at Dent, Pointe de Réville and at Draguet. Parallel changes are noticed in the annelid fauna. Thus certain species which were common last year have either become rare, or totally extinct, while new species are continually taking their places. (Revue Scientif., 1895, p. 374.)

Preliminary Outline of a New Classification of the Family Muricidæ. By F. C. Baker (Bull. Chicago Acad. Sciences, 1895). On reading this paper we regret to find that Mr. Baker has been putting his new wine into old bottles. In other words, he has borrowed largely from the phraseology of a conchological paper published in 1892, as the following parallel passages show :

Pilsbry, 1892.

"For several years the writer has been accumulating data bearing upon the natural classification of the Helicoid land snails. It has been thought desirable to place before students of this group some of the general results attained, and to invite their friendly criticism.

" * * * the author's aim being simply to place before malacologists the outlines of a classification essentially modern and essentially original."[1]

Baker, 1895.

"For several years the writer has been accumulating data bearing upon the natural classification of the Gastropod family Muricidæ. It has been thought desirable to place before students some of the results elucidated, and to invite their friendly criticism.

"The author's aim in the present paper has been simply to place before malacologists the outline of a classification essentially modern and essentially original."

[1] The above quotation is from Pilsbry's Preliminary Outline of a New Classification of the Helices, Proc. Acad. Nat. Sci., Phila., 1892, p. 387. Good taste should have forbidden the reproduction by Mr. B. of the second paragraph here quoted, the egotism of which is excusable only in view of its undeniable truth in relation to the 1892 publication. This excuse seems to be lacking in the case of Mr. Baker's paper.

More to the same effect might be quoted, but the above is sufficient on this score.

We do not wish to imply that there is any great harm in using borrowed phrases; they are not copyrighted, and their original author probably does not expect to make use of the same sentences again; but, still, if anybody has ideas worth expression, they surely ought to be worthy of fresh verbiage.

In regard to Baker's subfamilies, we do not see that they differ from those of Tryon and Fischer, except that Baker includes *Coralliophila* and its allies as a third subfamily. As this group lacks teeth, it seems much better to treat it as a family. In this connection it may be well to state that *Latiaxis mawæ* is not a monstrosity as Baker's foot-note (p. 188) would seem to imply.

The diagnoses of subfamilies given are rather absurd in view of their contents, which contradict every word of the descriptions. Not all the genera placed in "*Muricinæ*" have spinous or foliated varices, not all have the nucleus of operculum apical, and not all have few cusps on the rhachidian teeth. What is the use, then, of such a "subfamily?" Among the genera we notice, on a cursory inspection, that *Murex tenuispina* Lamarck is quoted as type of *Murex* Linné. How can

Lamarck's species, published a half century *later* than Linnæus' genus, be the type of that genus? The type of *Pterorhytis* Conrad ("Pterohytis" Baker) is not *Ocinebra nuttalli* Conr. but *Murex umbrifer*. Other mistakes of this nature occur, but we have not space to notice more.

The citation of the pre-Linnæan "genera" of Klein is contrary to all codes of nomenclature recognized by modern zoologists, and the continuation of such anomalies is to be deprecated. In retaining *Tribulus*, *Pentadactylus*, etc., as of Klein, Mr. Baker is clearly in error.

Most, if not all of the innovations in nomenclature proposed in this paper, are borrowed from Fischer and Dall. We find no new facts in regard to either soft anatomy or shell structure in the entire article, so that Mr. Baker's claims for originalty and modernness do not seem sufficiently apparent to call for special remark.—H. A. PILSBRY.

Herpetology of Angola.—The Herpetology of the Portugese possession in Western Africa, just published by Barboza du Bocage at Lisbon comprises descriptions of 185 species, distributed as follows; Chelonia 10, Loricata 3, Sauria 57, Ophidia 74, Batrachia 41. Of the specimens described, 62 species and varieties belong exclusively to the fauna of Angola and Congo. In order to better appreciate the relation which the herpetological fauna of these two areas bears to that of the rest of Africa, a table of the geographical distribution of the species described is given and forms an important adjunct to the paper. A number of new species are described, and synonymy is corrected. The paper is handsomely illustrated, and forms an important contribution to the knowledge of the subject.

Among the points of interest embraced in the paper are the discovery of the new species: *Naja anchietæ*, *Dendraspis neglectus*, *Vipera heraldica* and *Python anchietæ*; the southern range of the West African *Osteolæmus tetraspes*, *Feylinia currorii*, *Atheri squamigera*, and *Hylambates aubryi*; the northern range of the South African *Mancus macrolepis*, *Zonurus cordylus*, etc. and westward range of the central African *Causus resimus*.

Zoological News. ;BIRDS.—In regard to the question of the value of the forms of the tongues of birds for classification, Mr. F. A. Lucas concludes that in the Woodpeckers the evidence favors the view that the modifications of the tongue are directly related to the character of the food, and are not of value for classification. (Bull. No. 7. Div. Ornith. and Mam. U. S. Dept. Agric., 1895.)

In the study of the hyoid bone of certain parrots, Mr. Mivart finds that the whole order of Psittaci is distinguished from every other order of birds by the shape of its hyoid. The distinctive characters are (1) Basihyal much broadened posteriorly. (2) Basihyal developing on either side a forwardly and upwardly directed process. (3) An *os entoglossum* in the form of a single broad bone with a considerable central foramen, or, in the form of two lateral parts, entoglossals, medianly united in front by cartilage and leaving a vacant space between this and their attachment behind to the basihyal. (Proceeds. Zool. Soc. London, 1895, p. 162.)

MAMMALS.—Mr. Outram Bangs distinguishes the Skunks of eastern North America as follows:

Mephitis mephitica (Shaw), ranging through the Hudsonian and Canadian zones of the east, south to about Massachusetts.

Mephitis mephitica elongata (Bangs), found in Florida and the southern Atlantic states and ranges north to about Connecticut.

Both of these species differ from the western skunks, which form a separate group.

Among the latter the author recognizes Richardson's *Mephitis americana* var. *hudsonica* as a good species which must therefore bear the name *M. hudsonica* (Richardson). It is the largest of all the skunks, and has an extensive range in the northern prairies, extending east as far as Minnesota. (Proceeds. Boston Soc. Nat. Hist., Vol. XXVI.)

ENTOMOLOGY.[1]

Insects in the National Museum.—The staff of the Department of Insects of the U. S. National Museum has been reorganized, as a result of the sad death of the former Honorary Curator, Professor C. V. Riley.

The reorganization has been effected by the appointment of Mr. L. O. Howard, Entomologist of the U. S. Department of Agriculture, to the position of Honorary Curator to the Department of Insects; of Mr. Wm. H. Ashmead to the position of Custodian of Hymenoptera, and Mr. D. W. Coquillett to the position of Custodian of Diptera. All museum custodians are honorary officers. Mr. M. L. Linell will remain as general assistant to the Honorary Curator.

The Department is, at present, in excellent working condition. It contains a very great amount of material in all orders, and, in many

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

unusual directions, surpasses any collection in the country. Among others, the following are of especial interest:

(1) The large collection, in all orders, of the late Dr. C. V. Riley.

(2) All of the material gathered during the past 18 years by correspondents, field agents, and the office staff of the Division of Entomology, U. S. Department of Agriculture.

(3) The greater part of the collection of the late Asa Fitch.

(4) The large collection, in all orders, of the late G. W. Belfrage.

(5) The collections in Lepidoptera and Coleoptera made by Dr. John B. Smith down to 1889, together with the types of the Noctuidæ. since described by Dr. Smith.

(6) The collection of the Lepidoptera of the late O. Neske.

(7) The collection of Lepidoptera of G. Beyer.

(8) The collection of Coleoptera of M. L. Linell.

(9) The bulk of the collection, in all orders, of the late H. K. Morrison.

(10) The collection of Diptera of the late Edward Burgess.

(11) The type collection of Syrphidæ made by Dr. S. W. Williston.

(12) The collection of Ixodidæ of the late Dr. George Marx.

(13) The collection of Myriopoda of the late C. H. Bollman.

(14) Sets of the neo-tropical collections of Herbert Smith.

(15) The collection of Hymenoptera of Wm. J. Fox.

(16) The collection of Tineina of Wm. Beutenmuller.

(17) The large Japanese collection, in all orders, of Dr. K. Mitsukuri.

(18) The African collections, in all orders, of Dr. W. S. Abbott, Wm. Astor Chanler, J. F. Brady, the last "Eclipse" expedition to West Africa, and of several missionaries.

(19) The large collection from South California of D. W. Coquillett, in Coleoptera, Hymenoptera, Lepidoptera and Orthoptera.

(20) The Townend Glover manuscripts and plates.

In addition to this material, there are minor collections which have been the result of the work of government expeditions, or are gifts from United States Consuls and many private individuals.

This enormous mass of material is being cared for by the active and honorary forces of the Department, and the perpetuity of the collection is assured. The National Museum building is fireproof, and this, together with the fact that it is a national institution, renders the Department of Insects perhaps the best place in this country for the permanent deposits of types by working specialists in entomology, and for the ultimate resting-place of large collections made by individuals.

The policy of the Museum at large, with regard to the use of its collections by students, is a broad and liberal one. Students are welcome in all departments, and every facility is given to systematists of recognized standing.

On the Girdling of Elm Twigs by the Larvæ of Orgyia leucostigma, and its Results.[1]—The white-marked tussock-moth *Orgyia leucostigma*, has, for a long term of years, been exceedingly destructive to the foliage of the elms, horse-chestnut and fruit trees in Albany. Fruit trees of considerable size have been killed by their defoliation within a few days, toward the maturity of the caterpillar. Large elms and horse-chestnuts have had the foliage entirely consumed, only the ribs and principal veins remaining.

In the summer of 1883, a new form of attack by this insect was observed by me in Albany. About the middle of June of that year, the sidewalks, streets and public parks where the white elm, *Ulmus americana* was growing, were seen to be thickly strewn with the tips of elms two to three inches in length, bearing from four to ten fresh leaves, and comprising nearly all of the new growth of the season. On examination, it was found that above the point where the tips had been broken off, the bark had been removed for an extent averaging about $\frac{1}{10}$ of an inch, apparently by an insect.

As the Orgyia larvæ were then occurring in abundance on the trees, they were suspected as being the authors of this injury, and the suspicion was verified by ascending to a house-top, where the roof was found to be heaped in the corners with the severed tips, and the caterpillars engaged upon the branches in the girdling. The explanation of the breaking-off was simple. With the removal of the bark, the decorticated portion—not exceeding, in many instances, in thickness the diameter of a large pin—dried, and becoming brittle, was readily broken by a moderate swaying of the wind.

The girdling of the twigs in this manner could serve the Orgyia no such purpose as attends the girdling of several other insects, as the *Elaphidion* pruners of oaks and maples, where it enables the insect to attain greater security for its transformations through this method of reaching the ground, or the *Oncideres* twig-girdler, where the dead wood affords suitable food for the larva. Probably the conditions of growth during the spring of this year were such as to render the young bark at the point attacked particularly attractive to the larvæ; but why, after feeding upon it to so limited an extent, it should cease and resume its feeding on the leaves, can not be explained. In a few in-

[1] Read before the American Association for the Advancement of Science, at its Springfield meeting, Sept. 3, 1895.

stances where the twigs had become detached quite near the node marking the commencement of the year's growth, the bark had been irregularly eaten for an inch or more in extent.

While the Orgyia is a serious pest in Albany, it has its years of remarkable abundance and of comparative scarcity. Girdled tips, as above described, have been seen each year since 1883, but by no means corresponding in number to the degree of abundance of the caterpillar. My attention had not been drawn to them the present year, until much later than the usual time—toward the end of August. At this time (21st of August), many tips of unusual length and with perfectly fresh leaves were collected from beneath a large American elm. Each one had broken at the base of the girdling, which had probably been quite near the node of the year's growth. They were of special interest from their great length, varying from 10 to 18 inches. From the growth they had attained, it was evident that the girdling had not been done in the spring or early summer, but in the late summer after the usual brood had completed its transformations. It was clearly the work of a second brood of the insect, and this was confirmed by my having seen, a few days previously from a house-top, while making observations on the elm-leaf beetle, the Orgyia larva about one-half grown.

A distinct second brood of the Orgyia has not been recorded in Albany, although it is known to be double-brooded in Washington and Philadelphia, and probably in Brooklyn, and has also been observed in Boston. The present year, however, has been an exceptional one in the remarkable abundance, the rapid development, and the injuriousness of several of our more common insect pests.

Another interesting feature connected with these tips was the illustration they gave of the manner in which woody structure is built up —the sap ascending through the sap-wood, and, after its assimilation in the leaves, returning through the inner bark and depositing its organized material. The bark above the girdling, in healing in a rough and irregular manner, had swollen out at this point in a bulbous-like enlargement, showing very clearly the arrest and deposit of the returning sap consequent on the absence of its natural channels, and the drying and the death of the decorticated wood below it. In a specimen gathered in which the node of the preceeding year remained attached to the fallen twig, the diameter of the new growth above the bulb was at least twice that of the starved node below.

This peculiar form of Orgyia attack has not been seen upon the horse chestnut, maple, apple or plum, or any of its other food-plants.

J. A. Lintner.

Albany, N. Y.

EMBRYOLOGY.[1]

Experimental Embryology.——Recent numbers of Roux's Archiv für Entwickelungsmechanik contain numerous additions to our knowledge of the possibilities resident in the early stages of the development of animals, possibilities unsuspected till direct experimental interference made them evident.

T. H. MORGAN of Bryn Maur presents evidence[2] to show that two blastulæ of the sea urchin, Sphærechinus, may fuse together and form one embryo. When eggs are shaken just after fertilization they may loose their membranes and afterwards some of the resulting blastulæ are found to have twice the normal size though otherwise like the usual blastulæ in appearance. Such large blastulæ are stated to arise from the fusion of two common blastulæ.

Notwithstanding this complete fusion the future development of such enlarged blastulæ gives evidence of their dual origin. At the gastrula stage *two* invaginations are formed.

One may be much the greater and the two may not appear at the same time. The two invaginations stand in no fixed relation to one another and may appear in all parts of the compounded blastula.

Later the larva that develops from two fused blastulæ tends to develop two sets of arms and two systems of skeletal rods, but those accompanying the lesser invagination are much reduced in size and less perfect than the rods associated with the main invagination.

A second paper[3] by the same worker records a variation in the cleavage of the above sea urchin when some of the eggs were shaken.

While most of the eggs divide into 2, 4, 8 and 16 cells some were found to divide at once into *three*. These 3 cells are elongated parallel to the planes that produced them. When they next divide they all do so lengthwise, in flat contradiction to "Hertwig's law." These six equal cells lie in a plane at right angles to the two cleavage planes that have produced them.

Such eggs may develop into gastrulæ. They form six small cells or micromeres at one pole of the mass in place of the normal four. The author thinks "a micromere field must have been present in the egg prior to division."

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

[2] Vol. II, pps. 65–70.

[3] Idem, pps. 72–80.

Eggs that have not been shaken sometimes divide at once into four cells.

In both these unusual forms of cleavage the author finds that the three or the four archoplasmic centres present in the egg take unequal numbers of chromosomes. Thus in one case one centre was accompanied by 17, another by 14, another by 33 and the fourth by 0 chromosomes.

That this inequality is greater in the four-fold than in the three-fold division explains, the author suggests, the fact that fewer eggs develop from the four-fold than from the three-fold cleavage.

A third paper[4] gives a detailed account of the partial larvæ obtained when the eggs of Sphærechinus are shaken into fragments. Very minute gastrulæ only $\frac{1}{84}$ part of the volume of a normal gastrula are thought to come from isolated pieces with $\frac{1}{40}$ to $\frac{1}{50}$ the volume of the whole egg.

It is found that the number of cells in such small blastulæ is less than the normal number and roughly proportional to the size of the blastula.

The size of the nuclei, and probably of the cells also, is less in the small blastulæ than in the normal ones.

If one of first two cells of a cleaving egg be isolated it may form a blastula with $\frac{1}{2}$ the normal number of cells. One of the first four cells gives a blastula with $\frac{1}{4}$ the normal number, or with a little more than $\frac{1}{4}$; while one of the first eight cells when isolated produces a blastula with more than $\frac{1}{8}$ the normal number.

Such blastulæ will develop into gastrulæ.

A piece of the wall of a blastula when broken off by shaking may develop into a gastrula.

The little blastulæ formed from fragments of eggs tend to invaginate as many cells as possible up to the normal number for a normal gastrula.

These remarkable numerical relations lead the author to suppose that the reason why isolated cells of later stages in cleavage are not able to develop by themselves lies not in any differentiation of nuclear substance but in the fact that such cells being themselves the results of a series of cleavages cannot produce cells enough for the next stages of development.

Morgan and Driesch publish conjointly[5] their reinvestigation of the remarkable halflarvæ obtained by Chun.

[4] Idem, pps. 81–124.

[5] Idem, pps. 203-226.

Chun's work was done 18 years ago and was, as stated in a letter to Roux ;[6] as follows.

When the first two cells of the eggs of the lobate Ctenophore, *Bolina hydatina* were separated by shaking each developed as a halflarva with four ribs or bands of locomoter appendages instead of the normal eight, two entodermal sacs in place of four and only one tentacle in place of two.

The first cleavage plane coincides with the sagittal plane of the adult and the second with the transverse.

Half-larvæ with 4 ribs, 4 meridional vessels one tentacle and an oblique stomach *may become sexually mature*, developing eggs and sperm under the two subventral meridional vessels!

The missing half is regenerated during the postembryonic metamorphosis.

Driesch and Morgan worked on another Ctenophore, a nontentaculated form, *Beroë ovata* and finding it impossible to employ the shaking method cut the eggs with special scissors.

Isolated cells of the two cell stage develop into blastulæ, gastrulæ and finally into larvæ that are most remarkable in being neither complete nor halflarvæ but larvæ deficient in certain organs.

The cleavage of such an isolated cell is much as it would be if still associated with the other cell in a normal egg: it is a half cleavage as compared with a normal egg. This, however, is not true of the cells that form the ectoderm but only of the peculiar group of cells forming the entoderm. The former cells grow over the half-group of entoderm cells and form a larva that is complete on the surface.

The final larva is abnormal in usually having only 4 ribs instead of 8 and 3 pouches instead of 4.

A second series of experiments seems to throw much light upon the influence of protoplasm versus nucleus in the causation of such imperfect development.

When a piece of the protoplasm of an entire egg is cut off, the egg, deprived of some protoplasm but with it nucleus intact, as far as known, develops into a larva that may be deficient in just the same way as is a larva reared from one of the isolated cells.

In another paper[7] Morgan finds the shaking method will not succeed with the blastulæ of Sphærechinus as they die when shaken. In Echinus, however, both blastulæ and gastrulæ may be shaken into pieces that will live.

[6] Idem, Oct. 25.95, pps. 444–447.

[7] Idem, pps. 257–266.

When pieces of the wall of the Echinus blastula are broken off they may form little blastulæ again and these may gastrulate. When these little blastulæ invaginate they tend to form more entoderm cells, in proportion to the entire number, than is the case in the normal blastula.

In Sphærechinus the normal blastula has about 500 cells and tends to invaginate about 50 ; in Echinus about the *same fraction of the whole* is invaginated, for of about 1000 cells about 100 go in to form the entoderm.

When young gastrulæ are shaken they may form abnormal larvæ owing, apparently, to changes in the mesoderm inducing abnormal skeletal growths and corresponding abnormal arms.

Pieces shaken out of the wall of a gastrula will not form into a blastula nor into a gastrula.

Likewise the entoderm when shaken out does not develop. Yet a gastrula that has had its entoderm removed by shaking will continue to grow and form a normal skeleton and arms.

A paper[8] on cross fertilization and the fertilization of non-nucleated pieces of eggs also by MORGAN goes over part of the ground of Boveri's remarkable work.

It is shown that small pieces of eggs of *Echinus miliaris* may be fertilized and develop as far as to the 16 cell stage. As the number of chromosomes in such cleaving masses is, in each nucleus, half the normal number it is inferred that such cleaving masses are the results of the entrance of one spermatozoa into a non-nucleated piece of an egg.

In attempting to cross fertilize pieces of eggs of Sphærechinus with sperm of Echinus it was found that the sperm entered the pieces only in few cases ; there is the same difficulty in crossing pieces of eggs as in crossing the whole egg with foreign sperm. The reverse is also the case; sperm of Sphærechinus will not readily enter pieces of eggs of Echinus.

It is, therefore not surprising that no larvæ were found that could be traced to non-nucleated pieces of eggs fertilized by a sperm of another species, which is the great desideratum in attempting to repeat Boveri's work.

When the whole eggs of Sphærechinus are fertilized by sperm of Echinus bastards result that are very variable and not all exact middle states between the larvæ of these two species. When the converse cross is attempted the larvæ are "for the most part very abnormal in appearance."

When the eggs of Sphærechinus are crossed with the sperm of Strongylocentratus the larva is very variable and not an intermediate form.

[8] Idem, pps. 268–280.

The converse bastards also show great variation in the skeleton.

BOVERI[9] republishes, is an amplified form with many new illustrations, his remarkable work on the cross-fertilization of enucleated fragments of sea urchin eggs, translated in the AMERICAN NATURALIST March 1, 1893. After considering the opposing results obtained by Morgan and by Seeliger the author still maintains that he has shown that a larva may be obtained from a piece of an egg without any nucleus and sperm of another species and that such a larva has none of the maternal characters but only those of the male parent, thus showing that the nucleus may transmit characters but that egg cytoplasm alone cannot do so.

The evidence for his conclusions is, however, of an inferential nature and a cautious jury may well hesitate before convicting Boveri of having deprived the cytoplasm of its share in the affairs of heredity.

The evidence as he now presents it seems to be about as follows.

1. When at Naples in 1889 he shook a lot of sea urchin eggs, *Echinus microtuberculatus* in a testtube many were broken into pieces of various sizes, with or without nuclei; when this collection was treated with sperm of the same species larvæ of all sizes down to $\frac{1}{20}$ of the normal were found.

2. When pieces that contain no nucleus, as far as could be seen under the microscope, were isolated and fertilized with sperm of the same species they developed into dwarf larvæ.

3. When the normal eggs of *Sphrærchinus granularis* are treated with the sperm of *Echinus microtuberculatus* some few bastards result. In Boveri's original experiment all these bastards were half way between the larvæ of the parent species, both in external form and in the skeleton, which were very different in each pure larvæ form.

4. When the eggs of *E. microtuberculatus* are shaken and treated with sperm of the same species the larvæ present many abnormalities and some may have characters resembling those of another species.

5. When shaken eggs of *S. granularis* are treated with sperm of *E. microtuberculatus* large and small larvæ are formed; some few of the small ones, only a very few, 10 or 12 in all, were entirely of the Echinus or father type.

6. It was observed that the nuclei in any given area of a larva formed from a nonnucleated piece of Echinus egg and the sperm of the same species were, *on the average*, smaller than the nuclei in the corresponding area of a smaller larva formed from a nucleated piece.

7. The above few bastard larvæ of pure Echinus type had, on the average, smaller nuclei than similar larvæ of type intermediate between the two crossed parent.

[9] Idem, Oct. 22, 1895, pps. 394–441.

The author maintains that the few dwarf bastards that were like the male parent came from nonnucleated pieces of Sphærechinus penetrated by a sperm of Echinus. This, however, is an indirect result of all the above facts, It should also be borne in mind that Morgan, as cited above, was not successful in obtaining a cross fertilization of enucleated fragments and that both Morgan and Seeliger[10] find the bastards between Sphærechinus and Echinus so variable that Boveri's experiment in which they appeared as exact intermediate forms seem an exception and hence may be withdrawn from the evidence.

HANS DRIESCH starting from the standpoint that it has been proved that isolated cleavage cells may produce an entire organism seeks to find where the limits of this power appear in the subsequent stages of development.

He cut blastulæ of Sphærechinus into pieces, chopping at random with scissors in a dish full of gastrulæ. When isolated the larger pieces formed gastrulæ or later larvæ, in most cases.

When the gastrulæ of the starfish, *Asterias glacialis* were cut in the same way some lost the inner end of the gastric invagination together with much ectoderm. After healing over the wound the new end of the gastric invagination enlarged and sprouted out the two cœlomic pouches that would have, normally, been formed from the part that was cut away: the power to form cœlemic pouches was thus vicariously assumed by a part that would normally have produced part of the definitive digestive tract. Such larvæ go on to form a normally three chambered digestive tract from what would, normally, have formed but part of the whole.

The author concludes that the powers of the ectoderm or of the ento. derm cells are as yet not restricted as to what organs or parts they may form in their proper germ layer.

When larvæ with the mesenchyme were cut and a piece with only ectodermal cells was isolated in 53 cut of 99 cases no gastrulation took place but only a healing of the wound though life and activity might last for a week.

In a few cases when the digestive tube was removed from such larvæ it did not grow but died after a few days. In 19 cases where the end of the gastric invagination was removed after it had enlarged and sprouted out the cœlemic pouches 17 did not form new cœlemic pouches. In like manner when a cut happened to remove the skeleton of one side it was not formed again.

We thus soon come to a state in which the primitive tendency of cells to replace others in organ formation seems lost.

[10] See AMERICAN NATURALIST, March, 1895.

In all these cases the author will not grant that any *true regeneration* of lost parts takes place.

In the course of his discussion of the influence of yolk upon gastrulation PAUL SAMASSA[11] states that he has performed the following experiments upon frogs' eggs.

The eggs were injured, without breaking the egg membrane, by an induction shock applied from needle points to certain cells or groups of cells in the eight-cell stage of cleavage.

When the four vegetative cells are injured there may result a cell mass lying in two layers upon a mass of yolk and evidently representing, as seen in actions, a normal gastrula minus parts of its ectodermal structures. On the other hand injury to the animal cells results in a mass of cells lying near an inert mass of matter and possibly representing only entodermal cells.

These facts are interpreted as meaning that the animal or the vegetative cells may continue their development for some time independently of life, or at least of the perfect state, of the other cells.

These eggs die without reforming the injured parts by postgeneration.

In an extensive theoretical part of the paper the author places himself in the main, on the side of Hertwig as believing in epigenesis rather than in any form of preformation.

The last paper that we can notice here is that of AMEDEO HERLIZKA[12] who succeeded in tying a thread about the eggs of *Triton cristatus* in such a way as to completely separate the first two cleavage cells.

Hertwig compressed eggs by this method so that they formed hourglass shaped masses a single embryo finally resulted.

But in these experiments where the egg is separated into two distinct halves each develops by itself. From one half of the egg there results an embryo that may live to have a medullary tube and a notochord. This embryo was formed by a process of cleavage like that of an entire egg and not like the cleavage of half an egg.

The author holds that neither Weismann's nor any other ideas of preformation will suffice to explain such phenomena, but that we must accept some form of epigenesis.

He thinks that each of the first two cells is *totipotent* and normally makes half of the embryo when in the normal union with its fellow because of some *inhibition* of its power to produce the whole. In a case of postgeneration we might suppose that this inhibition was removed for a while and then resumed.

[11] Idem, Oct. 22, 1895.

[12] Idem, pps. 352–366.

ANTHROPOLOGY.[1]

Discoveries at Caddington, England, by Mr. Worthington G. Smith.—M. Renach informed the writer at the St. Germain Museum, in 1893, that a hermit was needed in France to live in the Drift Gravel Quarries and pounce upon chipped blades as they were brought to light in the excavations. This was to illustrate the fact that about four-fifths of the alleged paleolithic implements on exhibition in France were either found on the surface and not in place in the gravels, or bought by collectors, professors of geology and curators of museums, as I bought mine from workmen at the gravel pits.

Nevertheless, there is sufficient evidence of a Plistocene blade chipper in western Europe to satisfy the American critic who will take nothing on faith, and the best of this in recent years is embodied in the work of Mr. F. G. Spurrell, who found a stone blade workshop of Plistocene age under drift gravel at Crayford, England, and in the indefatigable explorations of Mr. W. G. Smith at North London (Stoke Newington) and at Caddington, Bedfordshire.

"Man, the Primeval Savage," by Worthington G. Smith, London (Edward Stanford, 27 Cockspur St., Charing Cross, 1894), tells of the striking discoveries made by the latter in some brick-kiln pits on a hill top near Caddington. These cuttings through the drift, discovered by tracing up relic bearing road ballast to its source, and watched for six years, during which time they were often filled with water or abandoned by workmen at critical moments, revealed what Mr. Smith calls a Paleolithic floor or older surface on which rested a stone blade workshop of Plistocene Age. This was covered by a mantle five to ten feet thick, of contorted drift, unfortunately containing no animal remains, that here overspreads the hill, and developed upon examination the following interesting and novel facts·

1. The blade factory was undisturbed, thus presenting an association of artifical objects full of significance and duplicating the results of Mr. Spurrell at Crayford. Other discoverers had found scattered and isolated specimens in the gravel, here the raw material, the blades more or less finished, the chips and the tools lay just as the Post-Glacial workmen had left them.

2. To the envy of the ordinary searcher for isolated objects in the drift, this range of specimens from one place included scrapers worked

[1] The department is edited by Henry C. Mercer, University of Penna, Phila.

on one side, well specialized leaf-shaped blades, either worked all round or sharpened to points, "punches," knife-shaped blades, hammer stones, "anvils," flaked cores and nodules worked in an exceptional way.

3. Discovered blocks of raw material, flint nodules with chalk still adhering to them, showing that the workmen had pulled them out of neighboring flint bearing chalk beds, lay in piles at the site.

4. Several large nodules had been sharpened at one end, leaving the rest of the nodular surface untouched.

5. The hammer stones found were not the numerous oval flint pebbles lying about the site and showing no signs of pounding (though they had been brought to the spot by workmen), but less regular fragments of flint, sometimes knocked into shape and scored with the marks of battering. Sometimes they weighed from five to six pounds.

6. Large flint masses, called by Mr. Smith "anvil stones," were found, showing slight traces of bruising, which, owing to slight doubts of the explorer, were not preserved.

7. The punches discovered were thin, stalactite-shaped nodules, bruised at both ends, weighing sometimes a pound or more, which with "fabricators," pieces of nicked flint used for flaking, in the explorer's opinion, were found mixed with the blade refuse. As opposed to Mr. Smith's view of flaking by means of stone punches and "fabricators," we know that the North American Indians, when working under similar circumstances, used bone, though a relic forger showed the explorer how the Caddington specimens could be accurately reproduced with an iron hammer and a broken gimlet or awl used as a punch.

8. Cores were discovered from which flakes had been worked (a) by careful blows, (b) by smashing with heavy blocks.

9. A beautifully veined pebble, found at the spot, had been brought there as an object of value by the ancient blade workers.

10. Several piles of apparently selected flakes were discovered.

11. A twin flake, held together by a fine, unsplit section, ready to break at a slight jar, was found with the refuse, showing that the workshop site, an area probably covering nearly an acre, had been very gently overspread with the now overlying drift-material, a deposition which had failed to seriously disturb the situation. Mr. Smith, who was present at the brick-pits, at short intervals, for nearly six years, in gathering this remarkable evidence, repeated observations previously made by him at Stoke Newington, Common, London, where, besides duplicates of many of the specimens referred to above, he found two artifically pointed stakes, a scratched log and a chipped blade resting on the scapula of a Mammoth (now on exhibition at the British Mu-

seum). At another place near Caddington, he had found associated with drift blades and in place a horde of two hundred of the bead-like fossils (*Cocinopora globularis*), with holes artificially enlarged, though at none of the sites were drawings on bone, bone needles or lance heads discovered. One of the most interesting features of the work at Caddington consists in what Mr. Smith calls "replacement," a process previously invented by Mr. F. G. Spurrell, and never before, to my knowledge, applied to drift specimens found in situ.

The two thousand two hundred and fifty-nine flakes unearthed at Caddington were grouped according to color on small trays easily shifted from table to table, and a laborious experimental study of them, lasting for three years, demonstrated the interesting fact that many sets of them fitted together, sometimes reconstructing the original nodule on which the blade maker had worked, sometimes hedging about hollows which, on pouring in plaster of Paris, reproduced the form of the resultant and missing blade.

"I examined and re-examined the stones," says Mr. Smith, "almost daily. I looked at them as a relief from other work and at times when I was tired.

"Not only did I keep my selected stones on the tables for this length of time, but I kept a vast number of blocks, rude pieces and flakes, on certain undisturbed grassy places in the brick-fields for the same three years. Whilst working upon my tables, I sometimes suddenly remembered one or more like examples on the grass, and at an early opportunity, fetched them from Caddington. In making up some of the blocks of conjoined flakes, it often happened that one or more interior pieces would be missing. In some cases, these missing pieces were never found, but in other instances, after the lapse of months, or even more than a year, a missing piece would come to light on the paleolithic floor. It is certain that I have not replaced all the flakes in my collection that are capable of replacement—one reason for this is that many flakes are very different in color and markings on one side from what they are on the other, and it is difficult to remember the markings on both sides. Another reason is that the time at my disposal has not been *unlimited*."

All this demonstrates in a manner, as conclusive as it is novel, that the Caddington site is an *undisturbed* workshop, while the analyses of Mr. Smith and the facts described in his work—Man, the Primæval Savage—take precedence over all recent evidence upon the subject, and throw a new light upon the more ancient subdivision of the Stone Age in Europe.

He who has spent earnest hours upon the problems of Plistocene humanity would gladly have seen a department of a museum specially devoted to these unique discoveries and demonstrations, but in a visit to Caddington in 1894, I learned with regret that the series, highly important from its entirety, and not jealously guarded as a whole, had been dissipated for the sake of collectors who wished to illustrate certain phases of Paleolithic blade manufacture with "fine specimens."

Theory, and with it the desire to propound formulæ for the blade-making process in general, yield respectfully to these toilsome investigations and to the persistent ransacking of quarries by a faithful observer whose work alone answers many of the doubts of the American student, and counteracts the questionable impression left upon the mind of the visitors to European museums by rows of typical specimens bought from workmen or gathered upon the surface.

H. C. MERCER.

Recent Explorations of Captain Theobert Maler in Yucatan.—[Extract from a letter received by the editor, December 9th, 1895].—After your departure from Yucatan, I undertook an expedition to the *Peten'Itza* region (Guatemala), crossing the entire pensinula, whose interior or southern part is nearly unknown.

After examining the country around the great Laguna of *Peten'Itza*, I embarked on a small canoe on the Rio Dela Pasión ("which, farther down, is named *Usumutsintla* [Land of Apes, *Usumatli* = with reverence, *Usumatsin* = Ape; *tla* = there is, there are, place of]). Arriving, finally, after many difficulties at Tenosique (State of Tabasco), from whence the traveler finds at his disposition small steamers plying to Laguna del Carmen, and thence by sea to Progresso. On this journey I had the luck to discover and photograph several highly interesting and unknown cities, with remarkable monuments and splendid sculptures, some in the neighborhood of Laguna del Peten, others on the right and left shores of the Rio Pasión (*Usumatsintla*).

On my return to Ticul, I found your letters and also one from Mr. Ashmead, which latter I answered, referring him on the subject of aboriginal Syphilis and Lupus to some passages in the ancient Spanish authors.

As to pottery-making, I have observed that it is the work of women solely, who exercise the art, in my opinion, in the ancient manner serving themselves nearly exclusively with the hands and feet and without special instruments. Here at Ticul, it is easy to see them at work, as the industry is a common one in the suburbs.

My collection of ancient earthen vessels is quite interesting, but as you left Ticul in such a hurry I could not show them to you. Several

of my vases have quadrangular inscriptions, of which I have not yet had time to make photographs. Lately the Globus published accounts of several of my smaller expeditions, accompanied by some twenty photographical illustrations which you may perhaps see in the Globus, Nos. 16 and 18, for 1895.

Some days ago, an earthen vessel, full of little implements of worked stone, was found at a hacienda near Ticul. I have been promised the specimens, and will communicate with you in case they turn out to be of interest. From the cave of *Loltun,* I have several very good photographs *Lol* = *Bejuco,* the Haytian name for hanging plants (the name *Vana* is not used in Mexico); *tun* = stone; *Loltun* = stalactites = hanging stones or stones like hanging plants.

I shall be glad to publish, from time to time, in American scientific or popular journals, small articles describing my Yucatĕckan discoveries, and when my present work of enlarging photographic negatives is finished, shall be ready to prepare for you a series of accounts of my work, accompanied by the necessary celluloid positives from which it is easy to make reversed negatives for the photolithographic process.

Next year I shall return to the States of Tabasco and Chiapas, where I have still to explore several entirely unknown ruins hidden in the wilderness occupied by the *Lacandones* Indians.

—THEOBERT MALER.

Ticul, November 20, 1895.

SCIENTIFIC NEWS.

The Biological Station of the University of Illinois is first to issue its circular for the summer of 1896. The station staff is composed of Professor S. A. Forbes, Director; Dr. C. A. Kofoid, Superintendent; Frank Smith and Adolph Hempell, Zoological Assistants; Dr. A. W. Palmer and C. V. Millar, Chemists; C. A. Hart, Entomologist and B. M. Duggar, Botanist. The station is situated upon the Illinois River near Havana, Ill., and is equipped with every facility for collection and study. There is a floating laboratory sixty feet long and twenty wide, a steam launch, licensed to carry 17 persons, and all the necessary supplies of tables, microscopes, aquaria, nets, chemicals, etc., as well as a specially selected library. As there are accommodations for only 16 in addition to the station staff, applications for the coming summer will be received only from those who have had sufficient experience to place them beyond the need of continuous supervision in their investigations, and, other things being equal, instructors in biology in colleges and high schools will receive the preference. The station will be open

during June, July and August. An incidental fee of $5.00 a month will be charged, and no application for tables should be made for less than two weeks. Board and rooms can be had in Havana at from $4.00 to $5.00 a week. All applications should be addressed to the Director, Professor S. A. Forbes, Urbana, Ill.

The announcement is made that Professor Marshall Ward has been elected to the Chair of Botany in the University of Cambridge, England, to fill the vacancy occasioned by the death (July 22, 1895) of the venerable Professor C. C. Babington.

The University of Cambridge receives the botanical collection of the late Professor Babington.

Mr. F. B. Stead, of Cambridge, England, has been appointed to carry on the investigations of the fisheries at the Plymouth Laboratory, and Mr. T. V. Hodgson as Director's Assistant in the same institution.

After an interregnum of several years, Washburn University, Topeka, Kan., has appointed Dr. G. P. Grimsby, of Columbus, Ohio, to the Chair of Geology and Natural History.

Drs. Walter B. Rankin and C. F. W. McClure, of Princeton, have been advanced to Professorships in Biology in the College of New Jersey.

The Government of the Cape of Good Hope has recently established a geological commission to carry on a survey of that region.

Dr. R. H. True has been appointed Instructor in Pharmacognostical Botany in the University of Wisconsin.

Dr. W. S. Strong, of Colorado, is called to the Chair of Geology in Bates College, Lewiston, Maine.

Bernard H. Woodward has been appointed Curator of the Museum at Perth, W. Australia.

Dr. R. Metzner has been elected Professor of Physiology in the University of Barcelona.

Dr. Dalle-Torre is now Assistant Professor of Zoology in the University of Innsbruck.

Dr. Hans Lenk has been appointed Professor of Geology in Erlangen.

Dr. Ducleaux has been elected President of the Pasteur Institute.

THE

AMERICAN NATURALIST

VOL. XXX. February, 1896. 350

ON HEREDITY AND REJUVENATION.

BY CHARLES SEDGWICK MINOT.

(*Continued from page* 9.)

III. A COMPARISON OF LARVA AND EMBRYO.[8]

It has long been known that animals develop according to two types, appearing in their younger stages, either as larvæ or as embryos. The larvæ lead a free life and must obtain their own food. Embryos, on the contrary, do not lead a free life and are nourished by the yolk accumulated in the parent ovum. There is, of course, no absolute demarcation between the two classes; nevertheless, a general comparison between them establishes several conclusions which throw valuable light upon some recent biological hypothesis.

First of all, it must be remarked that the larval development is primitive, and that the embryonic development has been evolved later. Geologists are able to present two principal supports for this assertion: 1. In the lower animals we encounter only larvæ, never embryos; sponges, colenterates, echinoderms and worms, all pass through the early stages of

[8] Read before the Amer. Soc. of Morphologists, December, 1893.

their ontogeny as larvæ. It would, therefore, be superfluous to linger for the defense of a view which is already accepted by all biologists. 2. The embryonic development depends on the presence of yolk. Now we have learned that the yolk has developed very gradually and in all the lower animals appears only in small quantities. It was not until the increase of yolk material had become enormous, as, for example, in the meroblastic vertebrates, that we find the development completely embryonic in type. With the increase of the yolk comes the gradual transition from larval to embryonic development. Since the embryo is dependent on the yolk, and since the yolk exists only in the higher forms in sufficient quantity, it follows that fully typical embryos can occur exclusively in the higher (later developed) animal types.

The fact that larvæ represent the primitive forms of development, obliges us to conclude that the correctnesss of Weismann's theory of the continuity of germ plasm can be tested better in larvæ than in embryos, since in embryos the relations have undergone profound modifications by secondary changes, which in this connection might easily deceive us.

I do not venture to assert that I know what the present form of Weismann's continuity theory may be; I hold, however, the exact form of this much discussed theory to be nonessential, because, according to my conviction, the theory can in no form be brought into agreement with our present knowledge. Nussbaum founded the theory, and opened the way along which we certainly hope to make great advance. Let me acknowledge the great value and the strictly scientific character of Nussbaum's work; doing this not merely because I esteem it, but also because the unjust attempt has been made to diminish his claim. Nussbaum[9] thought that the germ cells are *direct* decendents of the fertilized ovum, keeping the germinating power. while the rest of the cells developed from the egg are transformed into the tissue of the body. He brought forward several facts which could be interpreted in favor of his theory. By this theory the whole problem of her-

[9] M. Nussbaum, Zur Differenzierung des Geschlechts im Tirreich, Arch. f. Mikrosk. Anatomie, XVIII, 1--121, (1880).

edity and development was stated in an entirely new form. Since this publication of Nussbaum's we are seeking for the explanation of the germinating power, and the propagation of this power; formerly we sought for the causes of the inheritance of parental parts. The difference may be illustrated by the following example. Before Nussbaum we were ruled by Darwin's conception of Pangenesis, and we investigated accordingly for the agency by which the eye of the father reproduced itself in the child. Since Nussbaum we leave Pangenesis behind—it belongs henceforth to the past—and try to determine how the germinal substance behaves, and especially in what way it is perpetuated from the ovum through the following developmental stages, so that it is finally still present for the creation of the next generation. It is the conception of the continuity of the germinal substance which we prize so highly, and owe to Nussbaum.

Larvæ teach us that it cannot be special cells which affect this continuity. In fact, we find the organs of larval life fully differentiated before any sexual organs are recognizable, and indeed, in the majority of known larvæ we cannot recognize even the rudiments of the sexual glands. On the contrary, we find in larvæ unmistakably differentiated locomotive apparatus, such as cilia and often muscle fibres, a digestive canal, sensory organs, and, in many cases, also special excretory organs, and yet, only in a very few and exceptional cases can we distinguish the cells which belong to the future sexual glands. Thus, in regard to the primitive or larval type of development, we cannot say that the germ cells are constantly separated from the somatic cells during the segmentation of the ovum, but must rather draw precisely the opposite conclusion, namely, that the germ cells belong to the tissues which arise latest. We often meet many tissues in larvæ at a time when there is still no indication of germ cells. We find the same relations in embryos also, since in them the principal tissues become recognizable before germ cells are present. This fact was well established for vertebrates many years ago. It is characteristic of Weismann that he long defended the continuity of germ cells, in defiance of the facts. He has since

given up this wrong view and put in its place his hypothesis of the continuity of germ plasm. Of Nussbaum's conceptions, Weismann has left out the fruitful part, and has sown broadcast those ideas which were incapable of fruitful development. He has attempted to defend his notion of the difference between the elements of the embryo destined for the construction of the body, on the one hand, and those elements destined for sexual propagation on the other. Now, since the sexual cells usually develop from somatic cells, he was forced to assume that there is a mysterious substance which he names "*Keimplasma.*" This substance is supposed to store itself in the body by some secret way, to separate itself at command from the histogenic plasm, to appear unchanged and ready to be the exclusive agent of hereditary transmission.

Nussbaum furnished the conception of the continuity of germinal substance, which appears to be of immeasurable importance for the scientific investigation of the phenomena of heredity. But this continuity holds for all cells which arise from the fertilized ovum, as explained in the first section of this article. We must, therefore, seek for the causes of the differentiation of cells, that is to say, for the causes of the production of nerve cells, muscle cells, gland cells, etc, *and* of the production of germ cells.

I will now try to make clear the significance of the comparison between larvæ and embryos for the interpretation of germ cells. This calls for a short digression.

In the course of my investigations on "Senescence and Rejuvenation," of which only the first part has been published (Journal of Physiology, xii, 97), I learned that as cells become older there occurs an increase of the protoplasm in proportion to the nucleus, and I further succeeded in proving, as an essential process in reproduction, the formation of cells with comparatively little protoplasm. Further, it was found probable that a rapid multiplication of cells is only then possible when the cells have small protoplasmatic bodies (Proc. A. A. A. S., XXXIX (1890). We, therefore, have learned that the power of development depends on a special condition of the cell. By these facts I have been led directly to the following hypothesis:

The development of an organism does not depend on a substance stored in special cells, but on a special condition (stage) of organization. As a corollary of this hypothesis may be given this conclusion : *Germ plasm, in Weismann's sense, does not exist.*

According to my view, every part inherits from the germ, and every part of the animal body, as well as its germ cells, possesses the multiplying morphogenetic force, the action of which, however, is inhibited to the condition of the parts themselves. What this condition may be is not yet exactly known, but this much we do know, that the morphogenetic force is found in full activity in cells with little protoplasm. It is indeed highly probable that the slight development of protoplasm in proportion to the nucleus is an unavoidable condition of morphogenesis, or in other words, of the action of heredity. In fact we see that the first processes of development —as I have elsewhere explained (Proc. A. A. A. S., XIX)— show in the most varied cases a remarkable uniformity, for they always accomplish the production of cells with little protoplasm. Compare, in this respect, the vegetation points of plants, the root buds of slips, the budding zones of Annelids, the germinal layers of vertebrates, etc. The condition which allows the morphogenetic or hereditary force to act, arises under differing conditions, of which the fertilization of the ovum is one only.

Weismann tries to make comprehensible to us this one case, that of the fertilized ovum, by a special explanation which is available for no other case. Oscar Hertwig has recently (Zeit und Streitfragen, Heft I) clearly shown that Weismann's explanation is a speculative assumption, which can only be saved from rejection by numerous and often selfcontradictory additional assumptions. As I fully agree with Hertwig's criticism, I need only refer to his essay.

We will return to our proper theme. The next point is to determine whether there is a difference in the condition of the cells, as, regards their capacity for development, between larvæ, on the one hand, and embryos on the other. It can be proved that this is the case, by the following considerations. So far as we yet know, it is chiefly two factors which inhibit

development: *first*, the increase of protoplasm; *second*, the progress of organization, i. e., of differentiation.

As I was about to close this article, I received through the kindness of the author, Nussbaum's address on differation, in which he has defended essentially the same views as those which I hold. Such an agreement is of great value to me.

Now we know that larvæ are animal forms which have to obtain their own food and to protect themselves against enemies, and therefore are provided with differentiated tissues. Embryos, on the contrary, take their nutriment simply from the ovum, and the cells continue for a long time, developing and multiplying, while the protoplasm of the single cells increases very slightly, and *the beginning of the differentiation proper is correspondingly postponed.* I believe that we here have to deal with causal relations. From the actual relations just described, I conclude that the most essential difference hitherto known between larvæ and embryos, is to be found in the differing lengths of the period of multiplication of undifferentiated cells. In consequence of the shorter duration of the period in larvæ they have a much smaller total number of undifferentiated cells than embryos, or reversely expressed, embryos are much better equipped with material for the construction of the adult body, than are larvæ. As already stated, embryos are produced by the higher animals. This fact finds its explanation in the relations just described, because the increased number of undifferentiated, or so called embryonic cells, is precisely the necessary preliminary condition of the greater complexity of the differentiation by which the animal becomes more highly organized.

For the sake of clearness I have put aside all complications which might come in to play. It goes without saying, that the relations, in many respects, are by no means simple, nevertheless, the main conclusion above given seems a secure gain.

I therefore interpret the embryo as a device to render possible the increase of undifferentiated cells, and consequently a higher ultimate organization. The origin of this device is conditioned by a supply of food independent of the embryo.

From our present standpoint it is a matter of indifference whether the independent food supply comes from the yolk or from the uterus, however important the difference may be from other points of view.

It is to be further noted that our interpretation of the significance of the embryo is also opposed to Weismann's theory of germ plasm, because it emphasizes the importance of the *condition* as opposed to the assumption of a germinal substance or plasm. This road also leads to the conclusion reached above by other ways, the conclusion, namely: Reproduction involves rejuvenation, and rejuvenation is characterized by the production of cells with little, and that little not differentiated, protoplasm. Since rejuvenated cells arise by asexual as well as by sexual reproduction, since they appear in much greater numbers in embryos than in larvæ, and since they may be interpolated, as in the pupæ of butterflies, in the midst of the development of an individual, we must admit that the hereditary impulse (*vererbende Kraft*) is distributed in very different cells and is probably distributed equally through all cells. Hertwig has reached the same conclusion, with which Weismann's theory of germ plasm cannot be made to agree.

As Weismann has neglected the problem of rejuvenation, he has necessarily often gone astray in his discussion of phenomena in which rejuvenation plays the principal role. One is astonished at the slight attention bestowed on rejuvenation, when one recalls that it is the central problem of all questions of heredity treated by him.

Rejuvenation is one of the principal phenomena of life, and the rejuvenated condition of the cell is probably an unavoidable preliminary of heredity. We know that at least one *anatomical* sign of the rejuvenated condition is to be found in the preponderance of the nucleus in proportion to the protoplasm; a second *anatomical* sign is found in the structure of the protoplasm, which, in young cells always remains without differerentiation. The chief *physiological* sign of rejuvenation in cells which we as yet know is the power of rapid multiplication. Thus, we see, in case of sexual rejuvenation, that the

development of the fertilized ovum begins with an excessive proliferation of the nuclei, by which numerous cells are created, each with little protoplasm. Histogenetic differentiation begins later. The asexual rejuvenation has a similar course, but needs more thorough investigation.

Now differentiation is the sign of inheritance, and this morphological inheritance cannot develop itself fully until the senescence of the cells becomes recognizable by the growth of their protoplasm. On the other hand, we see complete inheritance develop itself, after preceeding rejuvenation. Accordingly we gain two conceptions: *first*, the hereditary impulse belongs to the inherent and constant properties of cells in general; *second*, the activity of their impulse may be inhibited by the condition of the cells. My view may be expressed in the following way: Somatic cells are simply cells in which the activity of the hereditary impulse is inhibited in consequence of their senescence, or, in other words, differentiation; but under suitable conditions the somatic cells may pass over into the rejuvenated stage, and thereupon develop the most complete hereditary possibilities.

The importance of rejuvenation must also be recognized when we consider the phylogenetic origin of single organs. Let us take a simple example. We may safely assume that the ancestors of mammals possessed a smooth skin, and that the covering of hairs is a new acquisition. Each hair is the product of a local growth. If we investigate the germ of a hair, we find that it consists of rejuvenated cells, that is to say, of cells with little protoplasm, or, as we are accustomed to say, of the embryonic type. Thus the formation of hairs depends on numerous centers of rejuvenation. In the multiplication of striped muscle fibres we find the agents to be the muscle buds, which are small, protoplasmatic structures, with relatively numerous nuclei. If we observe a developing gland, let us say a pancreas or a sweat gland, we find the rudiment to consist of rejuvenated cells; the cells multiply rapidly, and after the organ has its essential form, the histogenetic differentiation begins. It would be easy to multiply such examples a thousandfold.

The consideration of the role of rejuvenation in the origin of organs leads us to the theory of POST-SELECTION (Nachauslese). The theory is by no means new, but I wish to emphasize its far reaching importance. The preceding discussion teaches us to divide the origin of a new morphological part into two stages. The first stage is the development of the rudiment (*anlage*) by multiplication of the cells. The second stage is characterized by the gradual differentiation of the cells, by which they become capable of their ultimate functions. Especially in embryos is the difference in time very marked between the formation and the differentiation of the "*Anlage.*" Now it is evident that the undifferentiated "*Anlage*" is not useful, but becomes useful later. The formation and conservation of the "*Anlage,*" therefore, are due to selection, working, not directly upon the "*Anlage,*" but indirectly through preservation of the fully developed organ. The conception advanced is very simple and appears to me a necessary consequence of our knowledge. For the conception itself there has been hitherto to no definite term, I propose, therefore, to call it "*Post-selection*" (in German, "*Nachauslese*). To avoid possible misunderstanding, I give another example of post-selection. A parasitic wasp lays its egg in a certain caterpillar; the mother wasp gains no advantage, natural selection does not touch her, but only her progeny, the wasp larva. Nevertheless, the survival of the fittest rules.

In conclusion, I should like to direct the reader's attention to a problem which, so far as I am acquainted with the literature of biology, has been left almost unconsidered. This present translation enables me to insert a qualification of the preceding sentence, which ought to have been inserted in the original article, namely, that the problem has been the subject of important discussions by Hyatt, Cope and a very few others among paleontologists. I am glad to be able to refer to the article by Professor Hyatt, (see Jan. Naturalist) and presents the paleontological theory of the loss of ancestral characteristics. The problem above referred to is the *problem of lost characteristics,* which seems to me one of the fundamental problems of the doctrine of evolution, because we cannot un-

derstand the development of the higher organisms until this problem is solved. Everybody is writing about the origin of new organs, and we take lively pleasure in discussions about acquired characteristics. But if we consider the circumstances closely, we recognize that the loss of ancestral characteristics almost equals in importance the acquisition of new characteristics for the formation of new species. We assume that man had fishlike ancestors, and we strengthen ourselves in this belief by the comparison so often made between the human embryo, on the one side, and the adult fish on the other. But if the comparison be impartial we are forced to admit that nearly everything which is most characteristic of the fish is conspicuously lacking in the human embryo. Taking the embryo at the stage when the gill clefts have their maximum development, we find the following relations: the body is not straight but coiled up, and this coiling up is indispensable, in order to bring about the proper distribution of the human nerves, blood vessels, and so forth; the gill clefts are closed; gills are wanting; the digestive canal has no glands; the epidermis has no scales; the chorda dorsalis does not form a large axis of the body, but is a minute string of cells. In short, the *Biogenetisches Grundgesetz* (Recapitulation theory or von Baer's law, according to Adam Sedgwick) is scarcely half true. I have previously defended this conclusion at a meeting of the American Society of Morphologists, in December, 1893, Subsequently, but independently, Adam Sedgwick has reached a similar conclusion, see his paper "*On the Law of Development, etc.*" (Quart. Jour. Micros. Sci., XXXVI, 35). Were it not, as above implied, that the departures from the fish type are in great excess, there would be no embryo at all, and consequently no man, for the adult form is a consequence of the embryonic. The embryo is the mechanical cause of the adult body. How has the disappearance of the ancestral fish characteristics been effected? The question remains unanswered. It will, perhaps, be replied "through disuse" or "through panmixia." But "disuse" is merely a name, not an explanation of the phenomena. Panmixia is an hypothesis erected on nothing. In fact, this hypothesis as-

sumes that the majority of variations fall below the value maintained by natural selection, and consequently that when the influence of natural selection is eliminated (as in disuse), the mere variation will bring the traits concerned to disappearance. It marks Weismann's style of thought to find that he has entirely omitted to determine whether his assumption was correct, and nevertheless in his book, "The Germ Plasm," presents panmixia as an established law. As a matter of fact, the statistics of variations which we already have, show that his assumption is erroneous, and that it is equally probable that mere variation will magnify a characteristic as it is that it will diminish it.

Let us return to the embryo. The following hypothesis may be advanced:

The loss of ancestral characteristics in the embryo is due to post-selection, the cells being kept in a rejuvenated stage, in order that they may afterwards accomplish new differentiations.

This conclusion follows directly from the preceding considerations, and, therefore, needs no further defense.

IV. Concluding Remarks.

The views presented in the preceeding sections are intimately connected one with another and collectively determine our conception of the process of heredity. The conception concerns only the process and not the essential character or cause of heredity. According to my view, heredity exists in all cells, but its display is inhibited by organization of the living substance, and can be complete only in embryonic cells; embryonic cells arise under very various conditions. That which is novel in this theory is the significance attributed to embryonic cells. Embryonic cells I prefer to designate as rejuvenated cells.

The theory above presented is an unavoidable consequence of the facts known, and stands in absolute contradiction with Weismann's theory of the germ plasm.

I have read with the greatest conscientiousness every article hitherto published by Weismann, which deals with his theories of heredity. My final impression from this study is that

the "*Theory of Germ Plasm*" corresponds to the personal inclinations of its author and is in no sense a logical deduction won by the collation of facts. The assumption of a difference between germ plasm and histogenic plasm explains nothing. Even according to Weismann's own exposition it explains nothing, for the supposed phenomena which the assumption is said to explain, according to Weismann, do not exist. According to him, the circumstances are the following: The phenomena due to the germ plasm do not occur in somatic cells, therefore they have a different plasm, namely, histogenic; further, these phenomena do occur in somatic cells, therefore, they have germ plasm. Attention must be directed also, and explicitly, to the fact that Weismann offers no *observations* to support his fundamental assumption. His theory is mystical to an extreme degree. In Weismann's book, "*The Germ plasm*," one finds one hypothesis after another in order to support his tottering first hypothesis—germ plasm and histogenic plasm are special and separate substances. I demand of Weismann that he lay aside *all his hypotheses*, and present to us solely the *facts*, which support his theory of germ plasm. Then he will learn, as other investigators have already learned, that his hypothesis has been built up without sufficient foundation.

Let an investigator enquire for a possibility of testing the existence of the "Ids," "Biophors," "Determinants," etc., asserted by Weismann, and he will discover that the whole fabric is woven by speculative imagination. Confirmation of his ideas has, strictly speaking, not been attempted by Weismann. Indeed, confirmation is altogether impossible, for his conceptions are far beyond the limits of present human means of investigation.

It is time to finally discard a theory which leads astray and which, although it arose without scientific justification, is again and again pushed to the front by its promulgator. It is a scientific duty to take an unhesitating stand against Weismann's theory, for only so can it become known that those who have specially occupied themselves with the problem of heredity reject Weismann's theory of germ plasm unconditionally.

APPENDIX.

THE THEORY OF PANPLASM.

It appears desirable that the modern theory of heredity should be designated by a brief and appropriate name, and accordingly I propose the term "*Panplasm*," and that the theory be called "The Theory of Panplasm." By panplasm will be understood the physical basis of hereditary transmission, which is supposed to be distributed through all cells, and which accounts for the phenomena of sexual reproduction, regeneration and asexual reproduction. Panplasm is not a collection of gemmules or biophors. The term "panplasm" was first used by me at a meeting of the Society of Arts, in Boston, November 14, 1895.

On another occasion I hope to discuss the theories of pangenesis and panplasm in their historical aspects.

THE FORMULATION OF THE NATURAL SCIENCES.[1]

BY E. D. COPE.

Formulation is the method of presentation of the forms of our thoughts. Our observations of the facts of material nature are embodied in such classifications as we think best express their relations, and by means of these classifications expressed in language, we convey to others our conclusions in the premises. As the vehicle of presentation, formulation is one of the aspects of language, which as the medium of communication between men, enables them to accumulate knowledge. It is highly important then that the system of formulation should be uniform, so as to convey definite meaning and preserve the truth. The vast number of facts to be marshaled in orderly array, which constitute the natural sciences, require a

[1] Presidential address delivered before the American Society of Naturalists in Philadelphia, Dec. 26th, 1895.

correspondingly complex and exact formulation. The advent of the doctrine of evolution into the organic sciences involves the necessity of making such readjustments of our method of formulation as may be called for. It is with reference to this condition and the present action of naturalists regarding it, that I address you to-day. The subject may be considered under the three heads of Taxonomy, Phylogeny, and Nomenclature.

I. Taxonomy.

Taxonomy or classification is an orderly record of the structural characters of organic beings. The order observed is an order of values of these characters. Thus we have what we call specific or species value, generic value, family value, and so on. These values are not imaginary or artificial, as some would have us believe, but they are found in nature. Their recognition by the naturalist is a matter of experience, and the expression of them is a question of tact. Their recognition rests on a knowledge of morphology, or the knowledge of true identities and differences of the parts of which organic beings are composed. The formulation of these values in classification foreshadows the evolutionary explanation of their origin, and is always the first step necessary to the discovery of a phylogeny.

Taxonomy, then, is, and always has been, an arranging of organic beings in the order of their evolution. This accounts for the independence of the values of taxonomic characters, of any other test. Thus, no character can be alleged to be of high value because it has a physiological value, or because it has no physiological value. A physiological character may or may not have a taxonomic value. The practiced taxonomist finds a different test of values, which is this. He first endeavors to discover the series of organic forms which he studies. He learns the difference between its beginning and its ending. His natural divisions are the steps or stages which separate the one extremity from the other. The series may be greater or they may be lesser, i. e., more or less comprehensive, and it is to the series of different grades that we give the different names of the genus, family, order, etc.

We know that the characters of specific value in given cases are usually more numerous than those of higher groups. We know that they are matters of proportions, dimensions, textures, patterns, colors, etc., which are many. - The characters of the higher groups, on the contrary, are what we call structural, i. e., the presence, absence, separation or fusion of elemental parts, as estimated by a common morphologic standard; and it is the business of the morphologist to determine each case on this basis. In these characters lies the key to the larger evolution, that of the higher aggregations of living things. On the contrary, the study of the origin of species characters gives us the evolution of species within the genus, but of nothing more, except by inference.

Classification, then, is a record of characters, arranged according to their values. There still lingers, in some quarters, a different opinion. This holds that there is such a thing as a "natural system," as contrasted with "an anatomical system." Examination shows that the supporters of this view suppose that there is some bond of affinity between certain living beings which is not expressed in anatomical characters. A general resemblance apparent to the eye is valued by them more highly than a structural character. If this "general appearance" is analyzed, however, it is found to be simply an aggregate of characters usually of the species type, which by no means precludes the presence of anatomical differences. And these anatomical differences may indicate little relationship, in spite of the general resemblance of the species concerned, or they may have only the smallest value attached to such characters, i. e., the generic. It is with regard to the generic characters that the chief difference of practice exists. But it is clear that the record of this grade of characters cannot be modified by questions of specific characters. The two questions are distinct. Both represent nature, and must be formulated. In fact, I have long since pointed out that the same species, so far as species characters go, may have different generic characters in different regions. Also that allied species of different genera may have more specific characters in common than remote species of the same genus.

The anticipation naturally intrudes itself that the characters which distinguish the steps in a single evolutionary or genealogical line must disappear with discovery, and new ones appear, and that they must be all variable at certain geological periods, and hence must become valueless as taxonomic criteria. And it is therefore concluded that our systematic edifice must lose precision and becomes a shadow rather than a reality. I think that as a matter of fact this will not be the result, and for the following reasons. In the first place, when, say all the generic forms of a genealogical line, shall have been discovered, we will find that each one of them will differ from its neighbor in one character only. This naturally follows from the fact that two characters rarely, if ever, appear and disappear contemporaneously. Hence, generic characters will not be drawn up so as to include several points. For a while, there will be found to be combinations of two or three characters which will serve as definitions, but discovery will relegate them to a genus each. Each of these characters will be found to have what I have called the "expression point," or the moment of completeness, before which it cannot be said to exist. In illustration I cite the case of the eruption of a tooth. Before it passes the line of the alveolus it is not in use; it is not in place as an adult organism. When it passes that line it has become mature, has reached its expression point, comes into functional use, and may be counted as a character. Such will be found to be the case with all separate parts; there always will be a time when they are not completed, and then there will be a time when they are. These lines, then, will always remain as our boundaries, as they are now, for all natural divisions from the generic upwards. This condition cannot exist in characters of proportionate dimensions, which which will necessarily exhibit complete transitions in evolution. Hence, proportions alone can only be used ultimately as specific characters.

Some systematists desire to regard phyletic series as the only natural divisions. This may be the ultimate outcome of paleontologic discovery, but at present such a practice seems to me to be premature. In the first place, as all natural divisions

rest on characters, we must continue to depend on their indications, no matter whether the result gives us phyletic series or not. In the next place, we must remember that we have in every country interruptions in the sequence of the geological formations, which will give us structural breaks until they are filled. There are also periods when organic remains were not preserved; these also will give us interruptions in our series. So we shall have to adhere to our customary method without regard to theory, and if the phyletic idea is correct, as I believe it to be, it will appear in the final result, and at some future time.

Authors are frequently careless in their definitions. Very often they include, in the definition of the order, characters which belong in that of the family, and in that of the family those that belong in the genus. Characters of different values are thus mixed. The tendency, especially with naturalists who have only studied limited groups, is to overestimate the importance of characters. Thus the tendency is to propose too many genera and other divisions of the higher grades. In some groups structure has been lost sight of altogether, and color patterns, dimensions, and even geographical range, treated as characters of genera. As the mass of knowledge increases, however, the necessity for precision will become so pressing that this kind of formulation will be discarded, and definitions which mean something will be employed. Search will be made especially for that one character which the nature of the series renders it probable will survive, as discoveries of intermediate forms are successively made, and here the tact and precision of the taxonomist has the opportunity for exercise. In the selection of these characters, one problem will occasionally present itself. The sexes of the same species sometimes display great disparity of developmental status, sometimes the male, but more frequently the female, remaining in a relatively immature stage, or in others presenting an extraordinary degeneracy. In these cases the sex that displays what one might call the genius, or in other words, the tendency, of the entire group, will furnish the definitions. This will generally be that one which displays the most numerous char-

acters. In both the cases mentioned the male will furnish these rather than the female; but in a few cases the female furnishes them. The fact that both sexes do not present them does not invalidate them, any more than the possession of distinct reproductive systems would refer the sexes to different natural divisions.

I have seen characters objected to as of little value because they were absent or inconstant in the young. I only mention the objection to show how superficially the subject of taxonomy may be treated. So that a character is constant in the adult, the time of its appearance in development is immaterial in a taxonomic sense, though it may have important phylogenetic significance.

II. Phylogeny.

The formulation of a phylogeny or genealogy involves, as a preliminary, a clear taxonomy. I refer to hypothetical phylogenies, such as those which we can at present construct are in large part. A perfect phylogeny would be a clear taxonomy in itself, so far as it should go, did we possess one; and such we may hope to have ere long, as a result of paleontological research. But so long as we can only supply parts of our phyletic trees from actual knowledge, we must depend on a clear analysis of structure as set forth in a satisfactory taxonomy, such as I have defined above.

Confusion in taxonomy necessarily introduces confusion into phylogeny. Confusion of ideas is even more apparent in the work of phylogenists than in that of the taxonomists, because a new but allied element enters into the formulation. It is in the highest degree important for the phylogenist, whether he be constructing a genealogic tree himself or endeavoring to read that constructed by some one else, to be clear as to just what it is of which he is tracing the descent. Is he tracing the descent of species from each other, or of genera from each other, or of orders from each other, or what? When I trace the phylogeny of the horse, unless I specify, it cannot be known whether I am tracing that of the species *Equus caballus*, or that of the genus Equus, or that of

the family Equidæ. When one is tracing the phylogeny of species, he is tracing the descent of the numerous characters which define a species. This is a complex problem, and but little progress has been made in it from the paleontologic point of view. Something has been done with regard to the descent of some living species from each other. But when we are considering the descent of a genus, we restrict ourselves to a much more simple problem, i. e., the descent of the few simple characters that distinguish the genus from other genera. Hence, we have made much more progress in this kind of phylogeny than with that of species, especially from the paleontologic point of view. The problem is simplified as we rise to still higher divisions, i. e., to the investigation of the origin of the characters which define them. We can positively affirm many things now as to the origin of particular families and orders, especially among the Mammalia, where the field has been better explored than elsewhere.

It is in this field that the unaccustomed hand is often seen. Supposing some phyletic tree alleges that such and such has been the line of descent of such and such orders or families, as the case may be; soon a critic appears who says that this or that point is clearly incorrect, and gives his reasons. These reasons are that there is some want of correspondence of generic characters between the genera of the say two families alleged to be phyletically related. And this want of correspondence is supposed to invalidate the allegation of phyletic relation between the families. But here is a case of irrelevancy; a generic character cannot be introduced in a comparison of family characters. In the case selected, the condition is to be explained by the fact that although the families are phyletically related, one or both of the two juxtaposed genera through which the transition was accomplished has or have not been discovered. The same objection may be made against an allegation of descent of some genus from another, because the phyletic relation between the known species of the two genera cannot be demonstrated. I cite as an example the two genera, Hippotherium and Equus, of which the latter has

been asserted with good reason to have descended from the former. It has been shown, however, that the *Equus caballus* could not have descended from the European *Hippotherium mediterraneum*, and hence some writers have jumped to the conclusion that the alleged phyletic relation of the two genera does not exist. The reasons for denying this descent are, however, presented by specific characters only, and the generic characters are in no way affected. Further, we know several species of Hippotherium which could have given origin to the *Equus caballus* probably through intermediate species of Equus.

Some naturalists are very uncritical in criticising phylogenies in the manner I have just described. They often neglect to ascertain the definitions given by an author to a group alleged by him to be ancestral; but fitting to it some definition of their own, proceed to state that the ancestral position assigned to it cannot be correct, and to propose some new division to take its place. It is necessary to examine, in such cases, whether the new group so proposed is not really included in the definition of the old one which is discarded.

The fact that existing genera, families, etc., are contemporary need not invalidate their phyletic relation. Group No. 1 must have been contemporary with group No. 2, at the time that it gave origin to the latter, and frequently, though always, a certain number of representatives of group No. 1 have not changed, but have persisted to later periods. Some genera, as, e. g., Crocodilus, have given origin to other genera (i. e., Diplocynodon) and have outlasted it, for the latter genus is now extinct. The lung fishes, Ceratodus, are probably ancestral to the Lepidosirens, but both exist to-day. Series of genera, clearly phyletic, of Batrachia Salientia, are contemporaries. Of course we expect that the paleontologic record will show that their appearance in time has been successive. But many ancestors are living at the same modern period as their descendents, though not always in the same geographic region.

III. Nomenclature.

Nomenclature is like pens, ink and paper; it is not science, but it is essential to the pursuit of science. It is, of course, for convenience that we use it but it does not follow from that that every kind of use of it is convenient. It is a rather common form of apology for misuse of it to state that as it is a matter of convenience, it makes no difference how many or how few names we recognize or use. An illustration of this bad method is the practice of subdividing a genus of many species into many genera, simply because it has many species. The author who does this ignores the fact that a genus has a definite value, no matter whether it has one or five hundred species. I do not mean to maintain that the genus or any other value has an absolute fixity in all cases. They undoubtedly grade into each other at particular places in the system, but these cases must be judged on their own merits. In general there is no such gradation.

Nomenclature is then orderly because the things named have definite relations which it is the business of taxonomy, and nomenclature its spokesman, to state. Here we have a fixed basis of procedure. In order to reach entire fixity, a rule which decides between rival names for the same thing is in force. This is the natural and rational law of priority. With the exception of some conservative botanists, all naturalists are, so for as I am aware, in the habit of observing this rule. The result of a failure to do so is self evident. There is, however, some difference of opinion as to what constitutes priority. Some of the aspects of the problem are simple, others more difficult. Thus there is little or no difference of opinion as to the rule that the name of a species is the first binomial which it received. This is not a single date for all species, since some early authors who used trinomials and polynomials occasionally used binomials. A second rule which is found in all the codes, is that a name in order to be a candidate for adoption, must be accompanied by a descriptive diagnosis or a plate. As divisions above species cannot be defined by a plate, a description is essential in every such case.

It is on the question of description that a certain amount of difference of opinion exists. From the codes of the Associations for the Advancement of Science, and of the Zoological Congresses, no difference of opinion can be inferred, but the practice of a number of naturalists both zoologists and paleontologists in America, and paleontologists in Europe, is not in accord with the rule requiring definition of all groups above species. It has always appeared to me remarkable that a rule of such self evident necessity should not meet with universal adoption. However, the objections to it, such as they are, I will briefly consider. It is alleged that the definitions when first given are more or less imperfect, and have to be subsequently amended, hence it is argued they have no authority. However, the first definitions, if drawn up with reference to the principles enumerated in the first part of this address, need not be imperfect. Also an old time diagnosis of a division which we have subsequently found it necessary to divide, is not imperfect on that account alone, but it may be and often is, the definition of a higher group. But you are familiar with all this class of objections, and the answers to them, so I will refer only to the positive reasons which have induced the majority of naturalists to adhere to the rule.

It is self evident that so soon as we abandon definitions for words, we have left science and have gone into a kind of literature. In pursuing such a course we load ourselves with rubbish, and place ourselves in a position to have more of it placed upon us. The load of necessary names is quite sufficient, and we must have a reason for every one of them, in order to feel that it is necessary to carry it. Next, it is essential that every line of scientific writing should be intelligible. A man should be required to give a sufficient reason for everything that he does in science. Thus much on behalf of clearness and precision. There is another aspect of the case which is ethical. I am aware that some students do not think that ethical considerations should enter into scientific work. To this I answer that I do not know of any field of human labor into which ethical considerations do not necessarily enter. The reasons for sustaining the law of priority are partly

ethical, for we instinctively wish to see every man credited with his own work, and not some other man. The law of priority in nomenclature goes no further in this direction than the nature of each case requires. Nomenclature may be an index of much meritorious work, or it may represent comparatively little work; but it is to the interest of all of us that it be not used to sustain a false pretence of work that has not been done at all. By insisting on this essential test of honest intentions we retain the taxonomic and phylogenetic work within the circle of a class of men who are competent to it, and cease to hold out rewards to picture makers and cataloguers.

Another contention of some of the nomenclators who use systematic names proposed without description, is, that the spelling in which they were first printed must not be corrected if they contain orthographical and typographical errors. That this view should be sustained by men who have not had the advantage of a classical education, might not be surprising, although one would think they would prefer to avoid publicly displaying the fact, and would be willing to travel some distance in order to find some person who could help them in the matter of spelling. But when well educated men support such a doctrine, one feels that they have created out of the law of priority a fetish which they worship with a devotion quite too narrow. The form of our nomenclature being Latin, the rules of Latin orthography and grammar are as incumbent on us to observe, as are the corresponding rules of English grammar in our ordinary speech. This cult so far as I know, exists only in the United States and among certain members of the American Ornithologists Union. The preservation of names which their authors never defined; of names which their proposers misspelled; of names from the Greek in Greek instead of Latin form; of English hyphens in Latin composition; and of hybrid combinations of Greek and Latin, are objects hardly worth contending for. Some few authors are quite independent of rules in the use of gender terminations, but I notice the A. O. U. requires these to be printed correctly. Apart from this I notice in the second edition of their check list of North American Birds just issued, only

eighteen misspellings out of a total number of 768 specific and subspecific names, and the generic and other names accompanying. These are of course not due to ignorance on the part of the members of this body, some of whom are distinguished for scholarship, but because of an extreme view of the law of priority.

In closing I wish to utter a plea for euphony and brevity in the construction of names. In some quarters the making of such names is an unknown art. The simple and appropriate names of Linneus and Cuvier can be still duplicated if students would look into the matter. A great number of such names can be devised by the use of significant Greek prefixes attached to substantiatives which may or may not have been often used. Personal names in Greek have much significance, and they are generally short and euphonious. The unappropriated wealth in this direction is so great that there is really no necessity for poverty in this direction. It should be rarely necessary, for instance, to construct generic names by adding prefixes and suffixes of no meaning to a standard generic name already in use.

SOME LOCALITIES FOR LARAMIE MAMMALS AND HORNED DINOSAURS.

By J. B. Hatcher.

It is the purpose of this paper to give brief but accurate descriptions of the localities for the most important and best preserved specimens of Laramie mammals and horned and other dinosaurs collected by the writer for the U. S. Geological Survey, and now carefully stored in the Yale Museum at New Haven; with a map of the most important locality at present known and suggestions to collectors visiting this, or other localities as to the most promising places and best methods to be employed in order to attain the greatest degree of success.

History of the Discovery of Laramie Horned Dinosaurs.

As early as 1872, Professor Cope[1] described under the name of *Agathaumas sylvestre* a portion of the skeleton of a horned dinosaur from Laramie beds near Black Butte in southwestern Wyoming. In various publications from 1874–1877 which appeared in THE AMERICAN NATURALIST, Proceedings Philadelphia Academy Sciences and Bulletins of the U. S. Geological Survey, Cope has added much to our knowledge of these strange forms, chiefly from material collected by himself and Mr. Charles H. Sternberg from the vicinity of Cow Island on the upper Missouri River in Montana.

In 1887 a new locality for horned dinosaurs was found near Denver, Colorado, by Mr. George L. Cannon. The most important specimen, consisting of a pair of horn cores, was sent to Professor Marsh for identification and description. They were not characteristic, and owing to their striking resemblance to the horns of certain fossil Bisons, they were referred by Marsh to that genus and described under the name of *Bison alticornis*; the beds in which they were found being referred to late Pliocene and denominated the *Bison beds*.[2]

In 1888 the writer secured in the same locality in which Cope had operated in 1875 and 1876 on the upper Missouri, parts of several skulls of a horned dinosaur, some of which Marsh has described, creating for them a new genus *Ceratops*, and several new species. A comparison of the types of Cope's *Monoclonius recurvicornis* and Marsh's *Ceratops montanus*, both from the same locality in Montana, would doubtless establish the generic identity of the two.

Not until 1889 was a locality found where remains of these animals were sufficiently abundant and well preserved to afford material which would give us an adequate idea of their structure and habits. In the fall of 1888 the writer's attention was called to a pair of horncores belonging to Mr. C. A. Guernsey, of Douglas, Wyoming. Upon inquiry it was learned that they had been taken from a huge skull found by Mr. E. B. Wilson

[1] Proc. Am. Phil. Soc., 1872, p. 482.

[2] See notice of new Fossil Mammals, Am. Jour. Sci., Oct., 1887.

on Buck Creek, some of 35 miles north of Lusk, Wyoming. Early in the spring of 1889 the writer proceeded to Lusk, near which place Mr. Wilson still lived, and easily succeeded in getting that most accomodating gentleman to show him the skull from which he had taken the horns. This has proved a most important locality, and material obtained from it has increased many fold our knowledge of the Laramie reptilian and mammalian faunas. In the nearly four years spent by the writer in working these beds, 31 skulls and several fairly complete skeletons of horned dinosaurs were secured, besides two quite complete skeletons of *Diclonius* (*Claosaurus*), about 5000 isolated jaws and teeth of Laramie mammals and numerous remains of other dinosaurs, turtles, lizards, birds and fishes, as well as extensive collections of freshwater invertebrates from the same beds. In all over 300 large boxes of fossils were collected for the U. S. Geological Survey, and are now carefully stored in the Yale Museum, many of them as yet unopened.

At present remains of horned dinosaurs are known from only four widely separated localities; one of these, that of Black Butte, Wyoming, is west of the main range of the Rocky Mountains, and the other three including the Denver locality in Colorado; the Converse Co. locality in the extreme eastern portion of central Wyoming, and the Judith River or Cow Island locality in northern Montana, lie east of the main range. There are other localities known to the writer, but they are as yet of minor importance, since little collecting has been done in them and no material has been described from them. They will be referred to later.

The Ceratops Beds.

In the American Journal of Science for December, 1889, Professor Marsh applied the name *Ceratops beds* to certain strata in the west from which horned dinosaurs had been secured. He did not then, nor has he at any time since, designated just what he considered the geographical distribution of these beds nor their upper and lower delimitations in the geological scale. In order that the reader may not be misled in regard to Professor Marsh's position on this question I will quote him some-

what fully. In speaking of the horned dinosaurs in the publication just cited he says: "The geological deposits, also, in which their remains are found have been carefully explored during the past season, and the known localities of importance examined by the writer, to ascertain what other fossils occur in them, and what were the special conditions which preserved so many relics of this unique fauna.

"The geological horizon of these strange reptiles is a distinct one in the upper Cretaceous, *and has now been traced nearly eight hundred miles along the eastern flank of the Rocky Mountains.* It is marked almost everywhere by remains of these reptiles, and hence the strata containing them may be called the Ceratops beds. They are freshwater or brackish deposits, which form a part of the so-called Laramie, *but are below the uppermost beds referred to that group.* In some places, at least, they rest upon marine beds which contain invertebrate fossils characteristic of the Fox Hills deposits." Italics mine.

If we accept literally Marsh's statement that the Ceratops beds have been traced for eight hundred miles along the eastern flank of the Rocky Mountains, it will be necessary to suppose that he includes in the Ceratops beds not only the beds in Converse Co., Wyoming, but also the *Bison beds* (*Denver beds of Cross*) at Denver, and the *Judith River beds* on the upper Missouri. These are very widely separated localities, and no attempt has ever been made to trace the continuity of the strata from the one to the other, nor is it at all probable that such an attempt would meet with success. Professor Marsh did in the autumn of 1889 spend nearly two days in the Converse Co. locality, and again in 1891 he spent one full day in the same locality; but his time was occupied in visiting a few of the localities in which dinosaur skulls and skeletons and Laramie mammals had been found. No time was taken to determine the upper and lower limits of the beds or to trace the outcrops of the strata. After his visit in 1889 when he spent nearly two days with our party in the Converse Co. locality, he took the train for Denver, and in the company of Mr. George L. Cannon of that city, he spent one-half day examining the *Bison beds* (Denver beds). This constitutes Professor

time of the deposition of the Laramie beds of the Judith River and Black Butte localities, marine beds with Fox Hills types of fossils were being deposited over the region of what is now eastern and central Wyoming. This would account for the absence in this region, of the lower Laramie, with the smaller and less specialized horned, and other dinosaurs, characteristic of it.

Other Localities for Horned Dinosaurs.

In addition to the localities already mentioned, the writer has seen remains of horned dinosaurs and Hadrosauridæ on the North Platte River opposite the mouth of the Medicine Bow, about 35 miles below Fort Steele, Wyoming; along the eastern flank of the Big Horn Mountains about 40 miles south of Buffalo, Wyoming; on the west side of the Big Horn River between Fort Custer and Custer Station, Montana; and on Willow Creek 13 miles north of Musselshell postoffice in Montana. Another region not examined, but which looked very promising, is near the town of Havre on the Great Northern Railroad just north of the Bear Paw Mountains in northern Montana.

Suggestions to Collectors.

Of all the localities for Laramie dinosaurs and mammals known to the writer, that of Converse Co., Wyoming, is by far the most promising, and the earnest and intelligent collector will there meet with a fair amount of success for many years to come. As will be seen, by reference to the map accompanying this paper, this region is easy of access from the town of Lusk on the F. E. and M. V. R. R. At this station all necessary camp supplies can be obtained. The fossil beds are easily worked, the country being quite open and its surface not disfigured by deep and impassable cañons. There is abundant grass for horses, wood for fuel, and frequent small springs of fairly good water.

Vertebrate fossils are never abundant in the Laramie, but every exposure in this region should be carefully searched and especially the large sandstone concretions which contain many

of the best skulls and skeletons found in these beds. Fossils are found in both the shales and sandstones, but are best preserved in the latter. The small mammals are pretty generally distributed but are never abundant, and on account of their small size are seen with difficulty. They will be most frequently found in what are locally known as "blow outs" and are almost always associated with garpike scales and teeth, and teeth and bones of other fish, crocodiles, lizards and small dinosaurs. These remains are frequently so abundant in "blow outs" as to easily attract attention, and when such a place is found careful search will almost always be rewarded by the discovery of a few jaws and teeth of mammals. In such places the ant hills, which in this region are quite numerous, should be carefully inspected as they will almost always yield a goodly number of mammal teeth. It is well to be provided with a small flour sifter with which to sift the sand contained in these ant hills, thus freeing it of the finer materials and subjecting the coarser material remaining in the sieve to a thorough inspection for mammals. By this method the writer has frequently secured from 200 to 300 teeth and jaws from one ant hill. In localities where these ants have not yet established themselves, but where mammals are found to be fairly abundant it is well to bring a few shovels full of sand with ants from other ant hills which are sure to be found in the vicinity, and plant them on the mammal locality. They will at once establish new colonies and, if visited in succeeding years, will be found to have done efficient service in collecting mammal teeth and other small fossils, together with small gravels, all used in the construction of their future homes. As an instance of this, I will mention that when spending two days in this region in 1893, I introduced a colony of ants in a mammal locality, and on revisiting the same place last season I secured in a short time from the exterior of this one hill 33 mammal teeth.

Another way to secure these small teeth is to transport the material to a small stream and there wash it in a large sieve in the water, the finer material being washed away, but this treatment is too harsh to give the best results, what few jaws there are always being broken to bits.

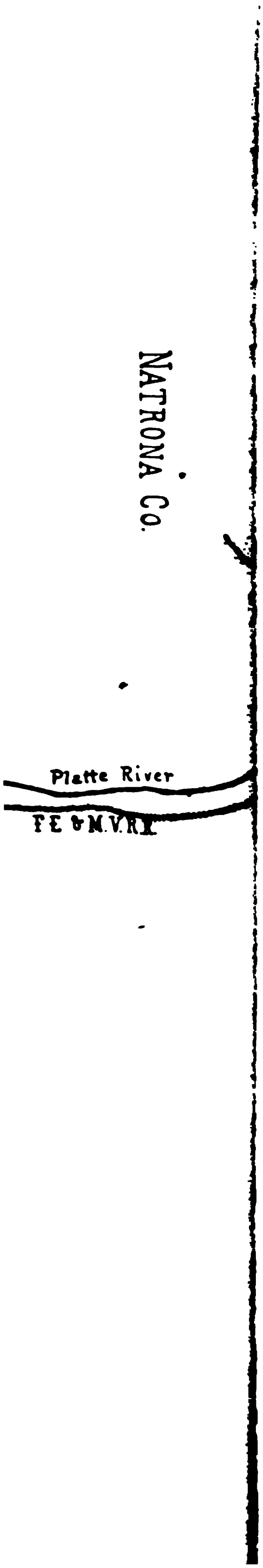
In the map accompanying this paper the dotted line marked Cer. border, indicates approximately the boundaries of the *Ceratops beds* in Converse Co. The line to the south and east indicating the outcrop of the beds; that to the west the point where they pass under the overlying beds. They extend on uninterruptedly into Weston Co., but have not been worked farther north than Schneider Creek. A working party encamped at the mouth of Schneider Creek on the Cheyenne River, would doubtless meet with much success. The map explains itself; it was drawn by the writer to accompany his paper on the Ceratops beds above referred to, but was not then published. A copy of it was given by him to Professor Marsh and a portion of it redrawn, will appear in the 16th Annual Report of the U. S. Geological Survey accompanying a memoir by Professor Marsh.

Princeton, N. J., Jan. 6, 1896.

RECENT LITERATURE.

Recent Books on Vegetable Pathology.—(1) Kirchner, Dr. Oskar: *Die Krankheiten und Beschädigungen unserer landwirtschaftlichen Kulturpflanzen.* Stuttgart, 1890, pp. VI, 637; (2) Comes, Dr. O.: *Crittogamia Agraria.* Naples, 1891, pp. 600; (3) Ward, Dr. H. Marshall: *Diseases of Plants.* London, (no date), pp. 196; (4) Ludwig, Dr. Friedrich: *Lehrbuch der niederen Kryptogamen.* Stuttgart, 1892, pp. XV, 672; (5) Tubeuf, Dr. Karl Freiherr von: *Pflanzenkrankheiten durch Kryptogame Parasiten verursacht.* Berlin, 1895, pp. XII, 599; (6) Frank, Dr. A. B.: *Die Krankheiten der Pflanzen.* Breslau, 1895–1896, Vol. 1, pp. XII, 344; Vol. II, pp. XI, 574; (7) Prillieux, Ed.: *Maladies des Plantes agricoles et des arbres fruitiers et forestiers causé par des parasites végétaux.* Paris, 1895, Vol. I, pp. XVI, 421.

It is desired only to call attention to these books at this time by means of the briefest mention. Some have been published long enough to enable one to speak freely of their merits and demerits; others are very recent additions to the literature of vegetable pathology and use has not yet demonstrated strong or weak points.


NATRONA Co.
Platte River
F.E.&M.V.R.R.

Dr. Kirchner's book deals with diseases due both to animal and vegetable parasites. Its statements are reasonably accurate and it is so arranged as to greatly facilitate identification of diseases. No illustrations.

Dr. Comes' book contains quite a full account of some parasitic diseases and brief mention of many others. It was the first book of its kind to pay much attention to bacterial diseases of plants. Its statements are frequently inaccurate and the 17 plates illustrating fungi and fungous diseases are poorly executed and add nothing to the value of the book.

Prof. Ward's little book is by far the best thing in English. It discusses only a few diseases and all of these in a very elementary, popular way, but there are many interesting suggestions, and the facts which are given are usually stated accurately. There are 53 text figures and a brief index. A book of about the same size and style by the same author, on *Timber and Some of its Diseases*, (1889) makes a good companion volume.

Dr. Ludwig's book is uneven in its make up, some parts being quite free from erroneous statements and others, those dealing presumably with the subjects least familiar to the author, needing careful revision. The book certainly deserves a second edition. From the pains taken to say something about everything, it is perhaps more generally useful than any of the preceding or than the following work.

Dr. Tubeuf's book is very attractive. The type is large and clear, and the unhackneyed character of the illustrations, many of which were prepared expressly for this work, is especially commendable. The treatment of certain subjects indicates that the author depended upon imperfect reviews rather than on the original papers, e. g., Wakker's bacterial disease of hyacinths, and Mayer's mosaic disease of tobacco; but the book as a whole has not been read carefully enough to warrant any extended criticism.

Dr. Frank's book is the second revised edition of his well known handbook, *Die Krankheiten der Pflanzen*, published in 1880, and now sadly out of date. Much new matter has been added and an earnest effort made to bring t. ; subject up to date. This has succeeded as well, perhaps, as the rapidly growing state of the science will admit. The first volume deals with non-parasitic diseases; the second with fungous parasites. Most of the figures appear to be old, and the letter press is indifferent.

Dr. Prillieux's book is attractive in appearance, but some of it is sketchy and rather unsatisfactory, and due credit is not always given.

Quite often the reader finds himself wishing the author had stated some matter exactly rather than vaguely, e. g., germination of the oospores of *Plasmopara viticola.* Prillieux is probably right in maintaining that Viala has not satisfactorily determined the aetiology of Brunnisure and the California vine disease, the microscopic appearances ascribed to a Plasmodiophora being quite as likely due to the effect of strong reagents on the protoplasm of the cell. Some of the figures in this book are excellent, others are very poor. There is no index.

It is to be hoped that Dr. Sorauer will now bring out another edition of his *Handbuch der Pflanzenkrankheiten*, or at least of the 2nd volume on parasitic plants which was issued in 1886 and needs revision badly. All of these books are useful to American students, and should certainly find place on the book shelves of every vegetable pathologist. It would seem that the time is not ripe for the appearance of standard American works on this subject. There is, however, great activity in the study of plant diseases in this country, and we may look for a crop of them within the next decade.—ERWIN F. SMITH.

The Iowa University Bahama Expedition.[1]—The history of an educational and scientific experiment is given Mr. C. C. Nutting in this octavo volume of 251 pages. It is published as Bulletins Nos. 1 and 2, Vol. III, of the laboratories of Natural History of the Iowa State University. The zoology of the region visited is treated of in a general way with a view to giving an idea of the facies of the collections from the several localities. The marine and land invertebrata are treated of quite fully, but none of the vertebrates receive much attention excepting the birds. The beauties of marine life are graphically described, and a considerable number of illustrations add to the general excellence of the get up of the book. An appendix gives a list of commissary stores actually used during the expedition.

Mr. Nutting, in summing up the results of the expedition, draws attention to the fact that this enterprise demonstrates the practicability of accomplishing such results at a cost which is merely nominal.

The Shrews of North America.[2]—The tenth number in the North American Fauna series published by the U. S. Department of Agriculture, contains three papers on the Shrews: A revision of the genera Blarina and Notiosorex by Dr. C. H. Merriam, a synopsis of the

[1] The Bahama Expedition. Bulls. Nos. 1 and 2, Vol. III, Laboratories Nat. Hist. Iowa State Univ. Iowa City 1895.

[2] North American Fauna No. 10, Washington, 1895. Comprising papers by C. Hart Merriam and G. S. Miller, Jr.

genus Sorex by the same author, and a discussion of the long-tailed Shrews of eastern United States by G. S. Miller, Jr.

In regard to the short tailed Shrews of the genus Blarina, Dr. Merriam states that up to the present time 8 valid species have been described from the United States, 2 from Mexico, 1 from Guatemala and 2 from Costa Rica. Twelve new forms are here added, 3 from the eastern United States and 9 from Mexico, making 20 members of the genus now known. The type localities are given and the geographical distribution. A complete synonomy accompanies each description.

Dr. Merriam's second paper is a synopsis of the species of Sorex, and is based on an examination of 1200 specimens. In this material were found 20 new forms which are here described. In this paper, as in the first, careful attention has been given to the synonomy.

The only genera of Soricidae included in this monograph by Dr. Merriam are Blarina Gray, Notiosorex Baird and Sorex Linn.

Mr. Miller's contribution is a study of the long tailed Shrews of the eastern United States. The author gives in detail the history of each species. The descriptions include the type locality, geographic distribution, and detailed information under the head of general remarks.

Figures of all the species described are given on 12 page plates, and they are of excellent quality. The monographs are the most important contributions to the subject that have been made, and are indispensable to the student of N. American mammalia.

Iowa Geological Survey, Vol. III.[3]—A quarto volume containing the several reports of the geological corps, with accompanying papers of the geology of special formations and areas. The work in the southwest half of the State was done under the immediate supervision of Dr. Keyes who contributes three papers on the geology of that section, and also one on the glacial scorings in Iowa. Mr. Calvin discusses the composition and origin of the Iowa Chalk. The Paleozoic strata in the northeastern part of the State, and certain Carboniferous and Devonian outliers in the eastern region are reported upon by Mr. Norton. The Cretaceous deposits of the Sioux Valley by Mr. Bain and certain buried River Channels by Mr. Gordon. The illustrations include 37 plates, a number of maps, and 34 figures in the text. We are glad to learn that the survey is in a prosperous condition, and hope that its work will be appreciated at its true worth by the State authorities.

[3] Iowa Geological Survey, Vol. III. Second Annual Report, 1895, with accompanying papers. Des Moines, 1895.

Duration of Niagara Falls and History of the Great Lakes.[4]—This work contains the researches of the author which have been published in America and Europe, on the Origin of the Great Lake Basins; Changes of Continental Altitudes; Deformation of Beaches; Glacial Dams; Births of Lakes Ontario, Erie, Huron, etc.; Changes of River Courses; and the History and Duration of Niagara Falls. It is one of the most important works on geological science that has been produced in this or any other country as an original research. It furnishes a standard of estimation of postglacial history for this continent, which must always be referred to in all questions relating to the antiquity of man, as well as those relating to the present distribution of land and water.

The text is fully illustrated with maps, section drawings, etc. One of the fine page plates which accompany the work is a reproduction from a camera obscura drawing made by Henry Ransford in 1832, the oldest accurate picture of the Falls known to the author.

The author estimates that the period which has elapsed since the falls were at Lake Ontario amounts to 32000 years.

Korean Games.[5]—In pursuance of a theory that games must be regarded as survivals from primitive conditions, under which they originated in magical rites and chiefly as a means of divination, Mr. Stewart Culin has made an extensive study of the games of Korea. He finds that there were two principal systems of divination in Eastern Asia from which games arose, in both of which the arrow or its substitute was employed as the implement of magic. Of the 97 games described in his book, 23 are directly connected with some such use of the arrow. A large number of the other games described consist of athletic sports ceremonially practiced in the sacred pavilions of Korea, and like the divatory tugofwar, still retain traces of their primeval divinatory character.

The illustrations are almost entirely by native artists, and they give the book a value altogether unique. They comprise 22 colored plates and 135 figures in the text. The subject is a very curious one, and as treated by Mr. Culin, it becomes an important guide to the history of human migrations and human thought.

[4] The Duration of Niagara Falls and the History of the Great Lakes. By J. W. Spencer. New York, Humboldt Publishing Co.

[5] Korean Games. With Notes on the Corresponding Games of China and Japan. By Stewart Culin. Philadelphia, 1895.

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General Notes.

PETROGRAPHY.[1]

Igneous Rocks of St. John, N. B.—W. N. Mathew has continued his work on the igneous rocks of St. John, N. B.,[2] contributing in a recent article an account of the effusive and dyke rocks of the region. All the rocks described are believed to be pre-Cambrian in age. They embrace quartz-porphyries, felsites, porphyries, diabases and feldspar-porphyrites among the effusive rocks, and diorite-porphyrites, diabases and augite-porphyrites among the dyke forms. In some of the quartz-porphyries perlitic cracks may still be recognized, and in the felsite porphyries some spherulites. Tuffs of all the effusives are abundant. A soda granite with augite and green hornblende and probably a little glaucophane was also met with. It is intrusive, and has a composition represented by the figures:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	CaO	MgO	Na_2O	K_2O	CO_2	Loss
64.86	.70	15.02	5.53	1.01	.18	2.61	1.42	3.92	2.37	.55	1.73

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Trans. N. Y. Acad. Science, XIV, p. 187.

The diorite-porphyrite has a groundmass of idiomorphic hornblende, lathshaped feldspars and some interstitial quartz, with phenocrysts of the same minerals, but principally of feldspar. Among the diabases is a quartzose variety.

Eruptive Rocks from Montana.—Among some specimens of eruptive rocks obtained from Gallatin, Jefferson and Madison Counties, Montana, Merrill[3] finds basalts, andesites, lamprophyres, syenites, porphyrites, wehrlites, harzburgites and websterites, some of which possess peculiar characteristics. A hornblende andesite, for instance, contains large corroded brickred pleochroic apatite crystals, whose color is due to innumerable inclusions scattered through them. The groundmass of some of the basalts has a spherulitic structure. The wehrlite is a holocrystalline aggregate of pale green diallage, reddish brown biotite, colorless olivine and a few patches of plagioclase. Its structure is cataclastic or granulitic, the larger crystals being surrounded by an aggregate of smaller ones. The websterite consists of green diallage and colorless enstatite with included foliae of mica and occasional interstitial areas of feldspar, and is thus related to gabbro. Some of the lamprophyres are composed of groups of polysomatic olivines or of olivine and augite in a scaly granular groundmass of lighter colored minerals, through which are scattered small flakes of brown biotite and tiny augite microlites. This structure is accounted for on the supposition that the granular groups of olivine and of olivine and augite belong to an older series of crystalline products than those of the groundmass.

Porphyrites and the Porphyritic Structure.—In a general account of the laccolitic mountains of Colorado, Utah and Arizona, Cross[4] gives a brief synopsis of the characteristics of the rocks that constitute their cores. These rocks comprise augite, hornblende and hornblende mica-porphyrites, diorites and quartz-porphyrites. All contain phenocrysts of plagioclase and of the iron bearing silicates, with the feldspars largely predominating. These upon separating left for consolidation into the groundmass a magma which upon crystallization yielded a granular aggregate consisting largely of quartz and orthoclase. No pressure effects were seen in any of the sections studied. All are porphyritic with a granular groundmass, which differs in the different rocks, principally in the proportion of its constituents. The porphyritic structure as defined by the author is not the result of the recur-

[3] Proc. U. S. Nat. Museum, XVII, p. 637.
[4] 14th Ann. Rep. U. S. Geol. Survey.

rence of crystallization, producing several generations of crystals, but it is a structure exhibiting contrasts in the size and form of the component crystals of a rock, resulting from the differences in conditions under which the different minerals crystallized.

Granophyre of Carrock Fell, England.—In the Carrock Fell district is a red granophyre closely associated with the gabbros. This rock has recently been studied by Harker,[5] who had previously investigated the gabbros. The normal type of the granophyre is an augitic variety in which the augite occurs as a deep green species which is idiomorphic toward the feldspars. Oligoclase is also present as idiomorphic crystals in a reddish quartz-feldspar groundmass with the typical granophyric structure. The composition of the rocks is represented as follows:

SiO_2	Al_2O_3	Fe_2O_3	MgO	CaO	Na_2O	K_2O	Loss	Total
71.60	13.60	2.40	.21	2.30	5.55	3.53	.70	= 99.89

As the rock approaches the gabbro it becomes less acid and the proportion of augite in it increases. This is the lower portion of the mass as it was originally intruded. Its more basic nature as compared with the rest of the rock is explained as due to the absorption of parts of the gabbro with which the granophyre is in contact.

The same author[6] also records the existence of a greisen, which is a phase of the well known Skiddau granite. The greisen consists essentially of quartz and muscovite, but remnants of orthoclose are still to be detected in it. The mica is regarded as having been derived largely from the feldspar.

Sheet and Neck Basalts in the Lausitz.—The basalts of the neighborhood of Seifeirnersdorf and Warnsdorf in the Lausitz, Saxony, occurs in sheets according to Hazard,[7] and in volcanic rocks. The sheet rocks are nepheline basalts, nepheline basanites and feldspathic glass basalts. The neck forms are hornblende basalts, sometimes with and sometimes without nepheline. The constituents of all are magnetite, apatite, augite, biotite, nepheline and glass in varying quantities, with feldspar, olivine and hornblende in different phases. Sometimes the mineral nepheline is absent, but this happens mainly in the glassy varieties, where its components are to be found in the glassy base. There are intermediate varieties between the hornblende and the oli-

[5] Quart. Journ. Geol. Soc., 1895, p. 125.
[6] Ibid, p. 139.
[7] Min. u. Petrog. Mitth., XIV, p. 297.

vine basalts corresponding to geological masses intermediate in characteristics between volcanic sheets and necks. In many of the neck rocks the hornblende is seen to have been partially resorbed and changed to augite. The continuation of the resorbtive process until every trace of the hornblende was dissolved, may account for the absence of the mineral in the sheet rocks.

Petrographical Notes.—In an article whose aim is to call forth more accurate determinations of the feldspars in volcanic rocks, and one which gives a practical method for making this determination, Fouqué[8] has described briefly the volcanic rocks of the Upper Auvergne, the acid volcanics of the Isle of Milo and the most important rocks in the Peleponeses and in Santorin. Among the varieties described are doleritic basalts, andesitic basalts, labradorites, andesites, obsidians, trachyte andesites, phonolites, andesitic diabases, rhyolites, dacites and normal basalts. The labradorites are composed largely of microlites of labradorite with a few augites and tiny crystals of olivine in an altered glassy base. In all these cases the author has shown that the rocks contain several different feldspars at the same time, and in each case he has determined their nature. The method made use of in the determination is based on the observation of extinction angles in plates cut perpendicular to the bisectrices.

In a well written article on complementary rocks and radial dykes Pirsson[9] suggests the name of oxyphyre for the acid complementary rock, corresponding to the term lamprophyre for the basic forms. He also calls attention to the fact that the dykes radiating from eruptive centers are usually filled with younger material than that which composes the core at the center. The dykes cutting the central mass will generally be oxyphyres and the more distant ones lamprophyres.

Cordierite gneisses are reported by Katzer[10] from Deutshbrod and Humpolitz in Bohemia, where they are intruded by granite veins, and where masses of them are occasionally completely surrounded by granitic material.

In the examination of a large series of granites and gneisses from the borders of the White Sea, Federow[11] discovered that garnet is present in large quantities when plagioclase is absent and vice versa.

In a general article on the Catoctin belt in Maryland and Virginia,

[8] Bull. Soc. Franc. d. Min., XVII, p. 429.
[9] Amer. Journ. Sci., 1895, p. 116.
[10] Min. u. Petrog. Mitth., XIV, p. 483.
[11] Ibid, p. 550.

Keith[12] gives very brief descriptions of the granites, quartz porphyries, andesites and the Catoctin schist of the region. The last named rock is apparently a sheared basic volcanic. All the rocks present evidence of having suffered pressure metamorphism.

GEOLOGY AND PALEONTOLOGY.

Notes on the Fossil Mammalia of Europe.—I, COMPARISON OF THE AMERICAN AND EUROPEAN FORMS OF HYRACOTHERIUM.—Historically speaking Hyracotherium is one of the oldest of known fossil Perissodactyla, and it is of importance phylogenetically to compare the representatives of this genus in Europe with those of America, in order to acquire an exact knowledge as to the evolution of the molar cusps of the New and Old World species. My attention was called to this subject on account of having studied *Euprotogonia* of the Puerco, a genus which as well known, is considered to have one of the most primitive types of Ungulate molars.

The importance of having accurate drawings of the teeth of fossil mammals is nowhere better illustrated than in *Hyracotherium*. In the case of the enlarged drawing of the teeth[1] of *H.* (=*Pliolophus*) *vulpiceps* which has been copied extensively in works on vertebrate palæontology, we obtain quite an erroneous idea of the exact form of the molar cusps.

Kowalevsky[2] in his great work on "*Anthracotherium*" figures some of the molars of the type of *Hyracotherium* namely: *H. leporinum*, and I should judge from his description that he had studied Owen's type in London. However, his criticism of Owen's drawing of the type of *Hyracotherium* is very accurate, and as Kowalevsky remarks, Owen's figures gives one the idea that the teeth of the type are strictly bunodont, whereas they are really transitional in structure between a real bunodont type, such as *Euprotogonia* and a truly lophodont form like *Systemodon*.

[12] 14th Ann. Rep. U. S. Geol. Survey, p. 285.

[1] Jour. of the Geog. Soc. of London, 1858, p. 54.

[2] Monographie der Gattung Anthracotherium, p. 205.

On my recent visit to London[3] I took the opportunity to examine both of the types of *Hyracotherium* (*H. leporinum* and *H.* (=*Pliolophus*) *vulpiceps*). In general the external cusps of the superior molars in both forms is lenticular in section, being considerably drawn out anteroposteriorly, and the intermediate tubercles are well extended transversely. In the drawing of the type specimen of *H* (=*P.*) *vulpiceps* the molars are represented as greatly enlarged, and their internal portions shown as complete. In the original specimen the teeth are damaged internally, and it is with some diffiulty that the form of the cusps can be made out. However, I am satisfied that the internal cusps are not really bunodont as shown in the plate, but like the American forms of *Hyracotherium* these cusps are extended transversely and form by wear slight ridges with the intermediate tubercles. In comparing the upper molars of both types of *Hyracotherium* of England with those of the Wasatch of America, I find them to be in exactly the same stage of evolution as to the form of the cusps.

Lydekker[4] speaks of the posterior transverse crest of the upper molars in the type of *H.* (=*P.*) *vulpiceps* as not being represented as sufficently well developed in the plate, but this crest on the last molar is drawn correctly, and on the other two molars it is as nearly as well developed. A comparison of the upper molars in both types of *Hyracotherium* with those of *Euprotogonia*, reveals the fact that the form of the cusps in the former genus has undergone a progressive change; and this is seen especially on the last upper molar which is quadritubercular with a large development of the metaloph, whereas in *Euprotogonia* the last upper molar is tritubercular. Again the third upper premolar in the English types of *Hyracotherium* is tritubercular whereas in *Euprotogonia*, this tooth has only one external cusp.

The species of *H.* (=*Pliolophus*) *vulpiceps* is of importance, as in this specimen we have both upper and lower teeth belonging to thè same individual. I should like to particularly emphasize the point that the last lower premolar in the type of *Pliolophus* is *simpler in structure than the first true molar, the posterointernal cusp being absent.* This character distinguishes this type from some forms of the American Wasatch which have been referred to *Pachynolophus*.

[3] I wish to express here my thanks for the privileges I enjoyed in examining specimens in the British Natural History Museum, and especially to thank Sir W. H. Flower for his kindness. I am also indebted to him for having been able to visit the Royal College of Surgeons. Mr. C. W. Andrews of the Geological Department. I am also much indebted for his many courtesies.

[4] Catalogue of Fossil Mammals in British Museum. p. 11.

The question now arises what is *Pachynolophus*, or in other words what is the exact generic definition of this genus, and does it really occur in the Wasatch of America. It is usually stated that *Pachynolophus* is separated from *Hyracotherium* by the fact, that the last premolar is molariform but as far as I have been able to investigate, this definition will apply to only one European species, namely: *Pachynolophus* (=*Hyracotherium*) *siderolithicus*, and even in this species the last upper premolar exhibits a good deal of variation in its structure. Rütimeyer[5] figures Pictet's species, *P. siderolithicus*, which in this specimen has the last upper premolar molariform, but in the same plate (fig. 21) is given another last upper premolar, which Rütimeyer referred to the same species. This tooth is tritubercular, or simpler in structure than the true molars.

Among the specimens of *Pachynolophus* in the collection of the Jardin des Plantes, Paris, there is a series of loose teeth from the Siderolithic du Mauremont, which are of considerable interest as they were studied by Kowalevsky, and referred by him to *P. siderolithicus*. This series contains at least one last upper premolar, and it has exactly the same character as that figured by Rütimeyer, in other words this is another case in which this tooth in *P. siderolithicus* is simple in structure. In *Pachynolophus duvalii* as figured by Rütimeyer, the last upper premolar is tritubercular, and this tooth has the same structure in *P. desmarestii*. In *Pachynolophus cessarasicus* figured by Filhol[6] from the Middle Eocene of Céssai, the last upper premolar has only one internal cone and the two transverse ridges diverging from it are well developed. The length of the skull in this species is about one-third greater than that of *Hyracotherium venticolum* of the Wasatch.

Numerous species of *Pachynolophus* occur in the different horizons of the Eocene of France, but in nearly all cases, they are represented either by upper or lower molars which were not found together. I believe the English *Hyracotherium leporinum* is the only known form in Europe in which both upper and lower molars were found associated, and actually belong to the same individual.

As is well known it is the last lower premolar which first becomes molariform, consequently if we find forms in which this tooth is simpler in structure than the molars, we can safely conclude that none of the superior premolars are molariform. In a jaw referred to *Pachynolophus* in the collection of the Jardin des Plantes from Phosphorites, containing all the lower premolars, the last tooth of this series is not molari-

[5] Eocäne Sangethiere Welt von Egerkingen, pl. III, figs. 18–21.

[6] Vertébres Fossiles d'Issel Mém. Soc. Geog. de France, 1888, pl. XX, fig. 13.

form. As the Phosphorites are considered to represent the top of the Eocene, we should certainly expect to find in any species of *Pachynolophus* from this formation the last premolar as complex in structure as the molars. In this jaw, however, the last premolar is not molariform and the collection contains a crushed skull of the same species of *Pachynolophus* in which all the superior premolars are simpler in structure than the true molars.

I can find no good generic differences based on tooth structure separating *Propalæotherium* from *Pachynolophus*, and shall consider the former genus as a synonym of the latter in this paper. In *Propalæotherium* the species are much larger than in those of *Pachynolophus*, but certainly size alone can not be considered as of value in generic definitions. In all of the species included in *Propalæotherium*, the premolars are simpler in structure than the true molars.

I believe, however, if we divide the various known species of the *Hyracotheriinæ* into genera according to the complication of the premolars, that we shall be adopting an artificial character, and as shown above. Of all the known forms of *Pachynolophus*, *P. siderolithicus* is the only one in which the last upper premolars is molariform and even this species shows considerable variation in this respect. I conclude then that the only natural classification of these forms is a careful analysis of the form of the molar cusps, and to group the species into genera according to the development of the same; I refer here especially to the European forms of *Hyracotherium* and *Pachynolophus*.

Having thus attempted to show that in nearly all the European species of *Pachynolophus* the last premolar is simpler in structure than any of the true molars, I come to consider what are the generic differences separating *Hyracotherium* from *Pachynolophus*. Kowalevsky studied *Pachynolophus siderolithicus*, and if we compare the molars of this species with those of *Hyracotherium angustidens* from the Wasatch of America, we observe at once that the external cusp of the upper molars in the latter species are nearly round in section, and they are scarcely at all flattened. There is no mesostyle and the height of the crown is very low or strongly brachydont. In *P. siderolithicus* the ectoloph is considerably lengthened from above downwards, and the external cusps are strongly flattened, with a prominent mesostyle. In all the species of *Pachynolophus* which I have studied the molar crowns are higher than those of *Hyracotherium*, and in all the mesostyle is strongly developed. The latter characters demonstrate that the molars of *Pachynolophus* have reached a higher stage of evolution than those of *Hyracotherium*, and this transformation in the form of the molar cusps

points to a still higher differentiation so characteristic of the later genera of the Equine series. The characters above enumerated as distinguishing the molars of *Pachynolophus* from those of *Hyracotherium*, I think should be considered as generic. At least they are the only valid ones which I can discover at present.

Species of Perissodactyle Ungulatus allied to *Hyracotherium* occur in the Wasatch of America, which have been referred to *Pachynolophus*. In these forms the last inferior premolar is truly molariform, but the superior molars are of the primitive type namely; with low crowns and nearly bunodont external cusps. As this a combination of characters, which as far as known does not occur in the true *Pachynolophus* of Europe, I think accordingly that these species should be placed in a different genus from *Pachynolophus*. Prof. E. D. Cope has shown that the generic name *Orotherium* Marsh, has been anticipated by *Orotherium* Aymard, consequently I believe that the name *Orohippus* should be reinstated, and applied to those species of *Hyracotherium* which have the last lower premolar truly molariform in structure. I have already stated that in the type specimen of *Hyracotherium* (=*Pliolophus*) *vulpiceps* the last lower premolar is simpler in structure than in any of the true molars. Accordingly those species with the complex lower premolar represent a true generic stage, and as is already shown they differ from the true *Pachynolophus*.—CHARLES EARLE, *Laboratoire de Palæontologie Jardin des Plantes, Paris, Dec.* 20, 1895.

The Glossopteris Flora in Argentina.—A collection of fossil plants from Bajo de Velis, a league from the entrance to the Cantana valley in Argentina, has enabled Dr. F. Kurtz to establish the age of the fossiliferous shales of that region. The author gives tabulated comparisons of the flora of the Bajo de Velis beds with similar floras found at the Cape of Good Hope (Ekka-Kimberly beds), in peninsular India (Kaharbari beds), in New South Wales (Newcastle beds) and in Tasmania (Mersey Coalfield). From these tables it is seen than the specimens found at Bajo de Velis are most nearly related to those of the lower Gondwanas of India. Dr. Kurtz accordingly corrrelates the fossiliferous shales of Bajo de Velis with the lower Gondwanas of India, and agrees with the paleophytologist, O. Feistmantel, in assigning these beds to the Permian age.

Up to date but three rock formations in Argentina have yielded fossil plants: Retamito in San Juan, which has been shown by Dr. Szajnocha to be Lower Carboniferous; Bajo de Velis which is Permian; and

a series of beds in Mendoza, San Juan and La Rioja determined by Prof. Geinitz as Rhætic.

These data are commented on by the Director of the Geological Survey of India to the effect that one of the chief points of interest in connection with the discovery of Gondwana plants in Argentina lies in the fact that we have an unquestionable lower carboniferous series (Retamito) in the neighborhood of which (and probably unconformably to it) a series of beds is found, which contains well known Lower Gondwana species of plants, thereby limiting the geological range of the lowest beds of it, at all events to upper Carboniferous at most, which is a further confirmation of the views generally adopted by the Geological Survey of India. The genus Glossopteris proper is wanting, but the other genera characteristic of that flora are present. (Rec. Geol. Surv. Ind., Vol. XXVIII, 1895.)

Geological News.—PALEOZOIC —From a petrographic study of the igneous rocks near St. John, N. B., Mr. W. D. Matthew classifies the Pre-Cambrian of that region as follows: A. Laurentian composed of (1) Portland group and (2) Intrusive granite, B. Huronian composed of (3) Coldbrook group, of volcanic rocks, (4) Coastal group, of volcanic and sedimentary rocks, (5) Etcheminian or Basal Series, of sedimentary rocks, and (6) Kingston group, of metamorphosed volcanics. (New York Acad. Sci. XIV, 1895.)

MESOZOIC.—In studying the fossils obtained by M. Gautier from Madagascar, M. Boule comes to the conclusion that the Jurassic deposits of eastern Africa and those of the western slopes of Madagascar appear to have been laid down in a great interior sea, an Ethiopian Mediterranean, which was separated from the Pacific by an Indo-Madagascar peninsula.

Furthermore, that during the Upper Cretaceous there was land communication between the African continent, Madagascar and Hindustan. (Bull. Mus. d'Hist. Nat. Paris, 1895.)

According to R. W. Ells, the whole range of North Mountain, which cuts off the valleys of Cornwallis and Annapolis rivers from the Bay of Fundy, is an overflow of igneus rock which has issued through a line of fissure transversing the red Triassic beds, and is, therefore more recent than the latter. At several places the trap is overlaid by newer sedimentary beds of limestones and shales. No fossils have as yet been found in these sedimentary strata. The author calls attention to their importance and the desirability of a thorough exploration in order to determine their age since they represent the highest group of stratified sedimentary rocks in Eastern Canada. (Trans. Nova Scotian Inst. Sci. Halifax, Vol. I, 1894, p. 416.)

Two new species of Fishes from the Rolling Downs Formation (Lower Cretaceous) of Queensland are described by A. S. Woodward. They represent species of the genera Portheus and Cladocyclus, to which he gives the names *australis* and *sweetii* respectively. This discovery of these fossils is of considerable interest, since with the exception of a few Selachian teeth and vertebræ, and a fine species of Belonostomus, no cretaceous ichthyolites of importance have hitherto been described from this colony. (Ann. Mag. Nat. Hist. (6) XIV, 1894, p. 444.)

Cenozoic.—Fossil Ants are reported from the Bembridge limestone (Eocene) of the Isle of Wight. They are referred by P. B. Brodie to the genera Formica, Myrmica and Camponotus, and some others not yet described. The first two genera have also been found in the Baltic Amber. (Nature, 1895, p. 570.)

The Champlain epoch is correlated by Prof. Hitchcock with the Mecklenburg stage of Geikie. Both have the characteristic marine mollusca fauna, the Arctic flora (*Yoldia* beds of the Baltic) and best illustrate the isobases of De Geer. (Bull. Geol. Soc. Amer., Vol. 7, 1895.)

VEGETABLE PHYSIOLOGY.[1]

Smut Fungi by Oscar Brefeld.—At last we have in two big quartos, with numerous plates, the long promised volumes on the smut fungi. The work which is here completed was begun more than 12 years ago. The earlier experiments were gathered together and published in 1883 in a volume of 220 pages with numerous plates under the title of *Die Brandpilze I*, forming Heft V of Dr. Brefeld's Untersuchungen, the most important and revolutionary portion of this volume being the demonstration that the smut fungi, a goodly number at least and presumably all of them, although previously supposed to be strictly parasitic were capable of growing saprophytically and of multiplying indefinitely in dung in the form of sprout conidia, closely resembling yeasts, if not identical with many forms previously referred to this group. Some years later in an address before the agricultural club of Berlin, Dr. Brefeld communicated the most important results of his

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

magnificent infection experiments, but now for the first time we have full details of all the laboratory and field investigations. In the limits of this review it will be possible to notice only the first of these two volumes. This forms Heft XI of the Untersuchungen and is entitled *Die Brandpilze II.* It deals principally with infection experiments and gives in full the results obtained with *Ustilago Carbo* on oats, *U. cruenta* on sorghum, and *U. maydis* on maize. These experiments were carried on through a period of four years with striking results and in case of corn, with most unexpected ones. Space forbids entering into much detail. Those who wish for details will naturally consult the volume itself. Suffice to say that the infective material consisted of the yeast-like conidia propagated in nutrient solutions made from fresh horse dung.

In case of oats the best results from direct infection were 17 to 20 per cent. of smutty plants, obtained by spraying during the earliest stage of germination. Infections made when the embryo was one cm. long gave only 7 to 10 per cent of smutty plants; when it was 2 cm. long (500 plants), only 2 per cent became smutty. When the plumule had pushed through the enfolding sheath scarcely any of the plants could be infected, 200 seedlings in this stage yielding only 1 per cent of smutty plants and 200 more remaining entirely free. The infections took place through the young axis and also through the sheathing leaf so that both Wolff and Kühn were right, but a majority of the infections were through the young axis. In a second series of experiments garden earth was sprayed with the smut conidia and two days later oats were planted 1 cm. deep and subsequently transplanted to the open field: 300 of these seedlings yielded 5 per cent of smutty plants, and 300 more, 4 per cent., i. e. a much smaller per cent than was anticipated. In a third series fresh horse dung was mixed with garden earth which was then abundantly impregnated with the smut conidia. Three days later oats which had been soaked but were not yet germinated were planted in this soil at a depth of scarcely 1 cm. These seedlings were divided into two lots, 300 were kept for a time in the laboratory at a temperature of over 15°C., and 300 were placed in the cellar where the temperature did not exceed 7°C. Of the 300 kept in the laboratory 27 to 30 per cent finally became smutty; of those kept in the cellar, where germination proceeded more slowly, 40 to 46 per cent became smutty. This shows clearly that fresh horse dung greatly favors the development of smut and that weather which retards germination is also favorable. In the fourth series of experiments the infectious material was derived from conidia cultivated for a long time arti-

ficially, a few of the spores being transferred to a fresh nutrient solution every four days. The first trial (500 seedlings) was with conidia which had been cultivated in this manner for six months. These seedlings yielded from 7 to 10 per cent of smutty plants. The second trial was with conidia which had been cultivated for a year. This experiment was almost wholly negative, 300 of the seedlings yielded no smutty plants and 200 more gave only 1 per cent. The explanation was not far to seek since at the end of this period the conidia had almost wholly lost the ability to send out germtubes and along with it the power to infect the plants. Microscopic examination showed that the germtubes can penetrate into any part of the young seedling but this does not necessarily mean infection. The latter takes place only when the smut hyphæ are able to reach that part of the plant where the smut beds form. In all of these experiments the smut germs penetrated the young seedlings but the smut beds appeared only in the floral organs, some months intervening between the entrance of the fungus and the appearance of the smut in a totally different part of the plant. Those germtubes which enter the plant and fail to reach the incipient ovaries become enclosed in the mature tissues of the host plant and are incapable of further growth and this frequently occurs even in young seedlings.

The infections obtained with the big sorghum plant are even more interesting. Nearly all of the first series of infections were destroyed by a hail storm, but of the 32 plants which escaped 12 became smutty. The seedlings of the second series were infected indoor in March and set out the first of May. The plants grew luxuriantly and by the middle of August had reached a height of 5 to 7 feet. The first smutty panicle appeared August 16 and for some time thereafter it appeared as if all of the plants would be smutty, the infected panicles developing first. Finally sound ones began to appear. In the end there were 158 smutty plants out of 274. A third series of experiments was instituted to determine in what stage of germination the sorghum plant is most susceptible: 252 seedlings sprayed in the earliest stage of germination, gave 180 smutty plants. "The development of the smut in the earliest and strongest plants, which reached a height of 8 feet, was striking. The big panicles were attacked in toto and projected out of the luxuriant green foliage like black brooms." There can be no doubt that infection stimulates the growth of the plant. Older seedlings yielded less striking results: 150 which were infected when the embryo was a centimeter long, gave only 24 smutty plants; 190 infected when the embryo was 1½ cm. long gave 12 smutty panicles; 221 infec-

ted when the plumule had begun to push through the sheath gave 5 smutty plants; finally, 150 infected when the plumule had pushed through the sheath about 1 cm., remained entirely free from smut. Microscopic examinations made a few days after the conidia were sprayed on the seedlings showed that germtube penetrations were very common in that experiment which yielded over 70 per cent of smutty plants, infrequent in those which yielded only a small per cent of smutty plants, and altogether absent in the plants which remained entirely free from smut. As in oats, the smut was confined exclusively to the panicle, and the bulk of the infections took place during the earliest stage of germination, the tissues of the growing seedling very soon becoming immune.

The results with maize were very surprising since they developed three wholly unexpected facts, viz.: (1) The germtubes are capable of penetrating any young rapidly growing part of the plant; (2) The growth of the fungous hypha which has gained entrance into the plant is narrowly localized, the sporebeds developing in situ; (3) There is no period of rest, the smut beds developing immediately, i. e. within two or three weeks of the date of infection. Previous to these experiments it was supposed that corn smut entered the plant when it was a seedling and followed the same law of development as oat smut. In the first series of experiments, which proceeded upon this supposition, the smut conidia were sprayed upon 200 seedlings in the earliest stage of germination; upon 100 which were a little older; upon 100 still further advanced; and, finally, upon 100 when the plumule was pushing through the sheath. This work was done in the laboratory and after 14 days the plants were set out in the garden. Contrary to all expectation, very few penetrations could be found even by the most careful microscopic examinations, and these were confined to the root node, none being found upon the sheath,—everywhere over the surface crept the germtubes without being able to enter. These plants were under daily observation and after 10 to 14 days a few lagged behind the rest in growth, and on being pulled up smut pustules were found on the axis a little above the root node. Of the whole 500 seedlings, only a few became smutty, viz., 4 per cent in the youngest and 1 to 2 per cent in the older seedlings. In all of them the smut pustules appeared on exactly the spot where the germtubes had entered the plant and within three weeks of the date of infection. All the other plants grew to maturity and remained free from smut. Similar results were obtained from an experiment in which soaked, ungerminated kernels of corn were planted in a dunged soil which had been

abundantly infected with smut conidia. Of the 50 plants thus treated one died at the end of 4 weeks from a smut pustule on the axis, and the rest developed without any appearance of smut. Another experiment was undertaken with 150 seedlings still further advanced, the conidia being sprayed upon them, but this also gave negative results. No germtube penetrations could be found and no smut appeared upon any of the plants. These results led to a good deal of speculation and finally to the following experiments: The first of these was with plants a foot high, having a well developed cornucopia-like summit formed by the closely wrapped bases of the large outer leaves. One hundred plants were selected and into these cornucopias a nutrient solution containing smut conidia was injected. They were covered with straw matting five days to keep off rain and then freely exposed. On the tenth day, as growth continued and the infected parts were pushed up into sight, there was a changed appearance. The parts of the leaves touched by the infectious fluid were paler than the upper noninfected parts and suggested chlorosis. This appearance was visible in different degrees on all the infected plants? Already there were slight appearances of pustules and within a day or two they became very distinct, finally covering the whole infected surface with a smutty crust. Scarcely one of the male inflorescences escaped and the axis between the leaves was also smutty in so far as the infective material could reach it. Not one of the hundred plants escaped infection, the youngest suffering most. For the next experiment younger plants were selected, i. e. those about six inches inches high. In many of these the cornucopia was not well developed and allowed the infectious fluid to run out and waste and the infection miscarried. All, however, that were large enough to retain the conidia were killed outright by the development of smut pustules, the plants twisting and curving in all sorts of shapes and frequently wilting before the smut spores were mature. The third experiment was with plants 1½ feet high. Here the cornucopias were wide open and took in large quantities of the infectious fluid, which penetrated deep into the heart of the plant. After three weeks the male inflorescences appeared, but in only six plants out of 50 could any symptoms of smut be found and upon these the pustules were small and scattering. On the leaves there were wrinkled, white spots which, however, did not develop into smut pustules but subsequently became green and nearly normal in appearance. Scattered smut pustules were found on the axis at the base of the internodes in 7 cases, and the effect of the fungus was also visible on some of the upper blossoms which remained white and dried up without developing. Aside from these scattering

symptoms all of the plants remained sound, ripening normal ears. The fourth experiment, with still larger plants, gave wholly negative results. The heart of the plant proved immune, and normal ears developed. In another experiment female inflorescences were infected as soon as there was any indication of a forming ear, the Nahrlösung containing the conidia being injected into the narrow opening between the ligule and the axis. Smut pustules appeared in great numbers within 18 days but only on the parts which were actually reached by the injected fluid. Another experiment was made when the ears were in blossom. All the kernels became smutty and single ears reached the size of a child's head. In another experiment varying amounts of the lower part of the ear were protected from the fungous spray by wrapping them in blotting paper. In this case only the exposed kernels became smutty, showing again conclusively that the infection is purely local. The silk though much exposed to the conidial spray showed not the least trace of injury, having passed out of the meristematic stage. In still another experiment the kernels of the ear were sprayed with the smut conidia when they were more than ½ grown. The result was wholly negative; no smut appeared. Another experiment showed that the adventive aerial roots can also be infected if sprayed in an early stage of their growth. In short, any meristematic part of the maize plant is liable to direct infection and this is made easy by the fact, which is also Dr. Brefeld's discovery, that the corn smut fungus, unlike that of oats and sorghum, is richly provided with *aerial* conidia, which are easily carried or blown from the soil to any part of the plant. The consequent desirability of keeping the soil of corn fields free from smut spores, by removing and burning all smut pustules before they have ripened and shed, must be apparent to all. The corn smut spores seldom germinate in water, as is well known, and infection of the plant probably takes place only when the latter have an opportunity to germinate in the soil and produce the aerial conidia, this germination in the soil being greatly favored by the presence of dung. The volume contains VI, 98 pages of text and 5 lithographic plates, mostly colored.—Erwin F. Smith.

ZOOLOGY.

The Paroccipital of the Squamata and the Affinities of the Mosasauridae once more. A rejoinder to Professor E. D. Cope.—I. *The paroccipital.*—In 1870, Cope[1] designated the occipital externe, Cuvier, paroccipital, Owen with Huxley's name opisthotic, and homologized it with the squamosal of the Lacertilia and Ophidia. This opinion is held up in 1894 and in September, 1895,[2] but for the name opisthotic the name paroccipital is then used. On the other side, it is admitted by everybody else that the paroccipital, Owen (opisthotic, Huxley), which is free in the Testudines, is united with the exoccipital in the Lacertilia; the posterior portion of this bone, which is visible from behind, has been called the paroccipital process; in its anterior portion where it reaches the basioccipital it contains the posterior semicircular canals. I have stated in my last note (AM. NAT., Nov., 1895) that in young Sphenodons the paroccipital is free from the exoccipital exactly as in the Testudines and that Siebenrock has proved without question that the outer portion of the exoccipital of the Lacertilia, which lodges anteriorly the posterior semicircular canals, represents the same element. The paroccipital process of the exoccipital in Sphenodon is, of course, identical with the paroccipital process in the Lacertilia.

To this, Prof. Cope replies: "Baur asserts that the socalled parotic process [I said paroccipital process] of the exoccipital which supports the quadrate in the Squamata is the same element as that termed opisthotic by Huxley. This I deny, and believe that in this it is Baur and not myself who has fallen into error. *Siebenrock, instead of asserting this to be the case, denies it in the following language:*† 'It is not the processus paroticus of the pleuroccipital (exoccipital) which is homologous with the (paroccipital, Owen), opisthothic Huxley, but the portion anterior to the foramen nervi hypoglossi superius which protects the organ of hearing.' Siebenrock here uses the names of Owen and Huxley as refering to the same element, *but he makes the clear*

[1] Cope, E. D. On the Homologies of the Opisthotic Bone, Amer. Asso. Adv. Sc., XIX.

[2] Cope, E. D. On the Homologies of the posterior cranial arches in the Reptilia, Trans. Am. Philos. Soc., Vol. XVII, Apr. 27, 1892; also Am. Nat., May, 1892. The Osteology of the Lacertilia, Proc. Am. Philos. Soc., Vol. XXX, May 10, 1892, pp. 185–211. Amer. Nat., Sept. 1895, p. 855–856.

† Italics are mine.

distinction which is the important point, between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.† What then is the element which articulates with the quadrate in the different orders of the Reptilia?"

The sentence quoted from Siebenrock is misleading. Siebenrock *does not distinguish between the parotic process of the exoccipital and the element which contains the posterior semicircular canal.* He says: *not only the parotic process but the whole portion anterior to the foramen is homologous to the paroccipital.* This whole portion, of course, contains also the parotic process. The sentence of Siebenrock translated by Cope is printed at the end of the paper in a résumé. A full account of the conditions is given on p. 209. "Die bisherige Anschauung, dass am Processus paroticus des Pleuroccipitale (exoccipitale) das Opisthoticum zu finden sei, ist daher absolut unrichtig, *sondern der ganze vordere Theil des Pleuroccipitale, welche die hintere Partie des Gehœres enthält, sammt dem Processus paroticus ist als das eigentliche Paroccipitale aufzufassen.*† Vergleicht man dasselbe mit dem bei den Schildkröten zeitlebens separirten Paroccipitale, so ergiebt sich schon aus der Lage und Function die Homologie der beiden Knochen." And later: "Die gleichen Verhältnisse bestehen bei Hatteria, nur bleibt bei dersellben das Paroccipitale viel laenger vom Pleuroccipitale (exoccipitale) getrennt, als bei Lacerta."

That Prof. Cope has not studied Siebenrock's paper is also evident from the following sentence: "In the Testudinata, and according to Baur, in Sphenodon, the element which extends externally from the exoccipital to the quadrate is continuous with the opisthotic, but the semicircular canal is included in its proximal part only. Here the structure is entirely different from that which characterizes the Squamata, where the opisthotic does not extend distal of the canal and fuses early with the exoccipital." It is still more evident from the following words: "In the Squamata, where the opisthotic is restricted to the region of the canal and does not reach the quadrate, this socalled paroccipital is distinct." Cope thinks the paroccipital + otic portion of the paroccipital or opisthotic in the Testudines is not homologue to the paroccipital + otic portion of the paroccipital or opisthotic of the Squamata, and has the idea that this bone, paroccipital, Owen, opisthotic, Huxley. occipitale externe, Cuvier, consists of two elements, the outer one—the paroccipital—and the auditory portion, the opisthotic. He admits that "the direct evidence for such a primitive division of this element (occipital externe, Cuvier; paroccipital, Owen; opisthotic, Huxley)

† Italics are mine.

PLATE IV.

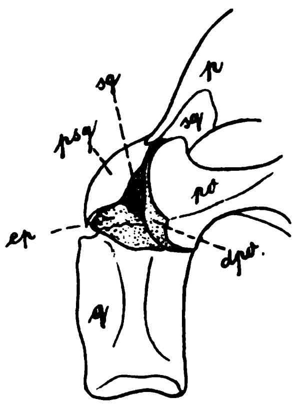


FIG. 1

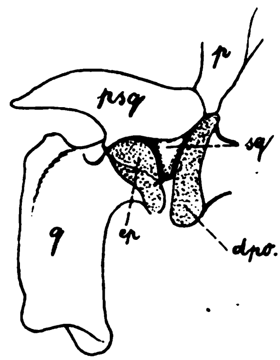


FIG. 2.

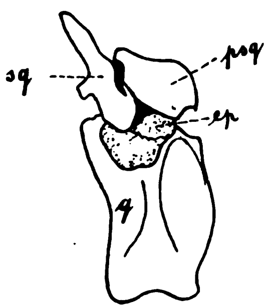


FIG. 3.

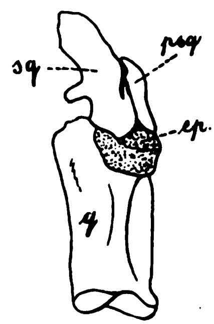


FIG. 4.

Conolophus subcristatus Gray. Quadrate and connections.

in the Testudinata has, however, yet to be produced, and I am entirely willing to give up the view above defended, should it turn out on further investigation to be untenable."

There is no further investigation necessary. The bone in question is a single element, as is shown, not only by comparative anatomy, but also by embryology. This element always is free in the Testudines; it is free in the young Sphenodon; and it is united with the exoccipital in the Squamata. There is not the slightest difficulty in this question.

One word about the squamosal. The squamosal of the Lacertilia and Ophidia is connected with the parietals and stands on the quadrate, outside of this element we have in the Lacertilia with well developed postorbital arch another element, which originally is united with the postorbital and is also connected with the squamosal and quadrate. This bone is the *prosquamosal.*| In Sphenodon the squamosal and prosquamosal are united, but in the Jurassic Saphæosaurus (Sauranodon) these two elements are free as in the Lacertilia. In the Testudines the squamosal represents the squamosal of Sphenodon, i. e., the squamosal + prosquamosal of Saphæosaurus and the Lacertilia. Prof. Cope says: "the squamosal of the Squamata is homologous to the paroccipital (opisthotic, Huxley, occipital externe, Cuvier) of the Testudines. This is impossible, since the paroccipital of the Testudines is the homologue of the paroccipital process of the Lacertilia, which in front contains, exactly as in the Testudines, the posterior semicircular canals. In the Mosasauridæ we have the same conditions as in the generalized Lacertilia. The paroccipital and exoccipital are united; connected with the quadrate we find two elements—the inner one connected by its upper branch with the parietal process; the outer one with the postorbital. These bones are, of course, homologous to the squamosal and prosquamosal of the Lacertilia.

II. *The Affinities of the Mosasauridæ.*—Cope maintains, contrary to my statement, "that in all Lacertilia the exoccipital supports the quadrate, and that in the Pythonomorpha and the Ophidia the exoccipital does not support it or generally touch it." He also maintains "that the paroccipital (squamosal, Baur) does support the quadrate in the Ophidia, while it is only in contact with a very small part of it in the Lacertilia." I have denied in my last note that in all the Lacertilia the exoccipital supports the quadrate, and I repeat it here.

I have before me disarticulated and complete skulls of Iguana, Ctenosaura, Amblyrhynchus and Conolophus. In none of these I find an articular facet on the paroccipital (exoccipital Cope), for the

quadrate. The paroccipital even does not touch the quadrate, but is connected by the anterior and upper portion of its distal process with the inner side of the squamosal; the face of the distal end of the paroccipital is entirely free from any connection and is always visible from the outside. The paroccipital process is placed behind and also above the upper face of the quadrate for these elements. In none of the genera mentioned above I find a face on the paroccipital for the quadrate, but a face for the squamosal. In the Mosasauridæ I find the same. The quadrate is supported by the squamosal and the squamosal is connected by its inner process with the anterior face of the distal end of the paroccipital; the prosquamosal takes also part in the support of the quadrate. We have, therefore, the same conditions as in the genera mentioned. The statement that the Mosasauridæ agree with the Ophidia in the relations of the quadrate, is absolutely incorrect.—G. Baur, University of Chicago.

EXPLANATION OF FIGURES.

Fig. 1.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from behind.

Fig. 2.—*Conolophus subcristatus* Gray. Left quadrate and its relations to the squamosal, prosquamosal and paroccipital, from outside and little behind.

Fig. 3.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind and a little inside.

Fig. 4.—*Conolophus subcristatus* Gray. Right quadrate and its relations to the squamosal and prosquamosal, from behind.

q=Quadrate.

ep=Epiphysis of quadrate.

sq=Squamosal (mastoidien, Cuvier; opisthotic, Cope, 1870; paroccipital, Cope, 1892–95).

psq=Prosquamosal (temporal, Cuvier; squamosal Cope, 1870; supratemporal, Cope, 1892–95). (Baur, G. Anat. Anz. Bd., X, p. 327.)

p=lateral process of parietals.

po=paroccipital (exoccipital, Cope).

dpo=distal end of paroccipital.

ADDITION.—After the manuscript of this note had been sent to the editor, I received the November number of the Annals and Magazine

of Natural History, containing a communication of Mr. G. A. Boulenger; "Remarks on the value of certain cranial characters employed by Prof. Cope for distinguishing Lizards from Snakes." Boulenger shows also that Cope's statement, in regard to the relations of the squamosum to the quadrate of the Lacertilia, is quite incorrect.

Criticism of Dr. Baur's rejoinder on the homologies of the paroccipital bone, etc.—I. *The Paroccipital Bone.*—It seems that I have not yet made clear to Dr. Baur my position as to this element in the Reptilia. The ground for it is paleontological, and when Dr. Baur considers the question from this standpoint, he will probably find some of his very positive assumptions not proveable at present.

In the first place, we agree as to the identity in the Lacertilia, Pythonomorpha and Ophidia of the element which he calls squamosal, and which I call paroccipital. Whatever be the place of this element in the Mammalian skull, it has certainly not been proven to be the squamosal, hence I object to the name which Dr. Baur uses for it, in which position I agree with various authors. It remains to be seen whether the term paroccipital, which I have hitherto used, be appropriate. I must here repeat that at no time since 1871 have I confounded it with the opisthotic of Huxley, not even in those cases (as Testudinata) where I have supposed the two elements to be fused together.

Now the characters of this paroccipital in the Pythonomorpha are such as to suggest strongly that it represents the dismembered distal part of the paroccipito-opisthotic of the Testudinata. This character I pointed out in 1870, and it deserves more attention than it has received from Dr. Baur and other authors. It cannot be seen without taking to pieces the region to which the quadrate is articulated. When this is done it is found that the paroccipital enters as a cone between the exoccipital and petrosal, and extends inwards in Mosasaurus nearly to the region of the semicircular canals. *Nothing like this is to be found in the Lacertilia.* The question now arises, what is the meaning of this structure? As the Pythonomorpha is a cretaceous type, it is evident that it is a survival of some primitive condition, and not a derivative of the condition found in the later order of Lacertilia; where the paroccipital is entirely superficial in its connections. On the contrary, the character of the Lacertilia has been more probably derived from that of the Pythonomorpha by the loss of the proximal part of the paroccipital.

In the Testudinata the paroccipito-opisthotic has not been observed, according to Baur, to consist of two elements distinct at some stage of

embryonic life. This fact does not, however, preclude the possibility that such a division may not have existed among the ancestors of the Testudinata. As this order is very old, these ancestors can only be looked for in the Permian and Triassic periods. Characters which belong to early geologic time, are frequently dropped out of the embryonic record. Now in the Permian Reptilia, some of which are the ancestors of the Testudinata, the quadrate is a short element, and is separated from the exoccipital and the opisthotic by a separate bone which has been called mastoid and mastotympanic by Owen,[3] and which I have considered as part of the "squamosal" in the absence of suture separating it from that element.[4] I think that such an element exists in the Cotylosauria. The periotic bones in Empedias[5] and Chilonyx are far removed from the elements which serve as suspensors of the quadrate bone, and are distinct from them in Chilonyx at least. Owen (l. c.) thinks that a paroccipital has been fused with the exoccipitals in Ptychosiagon (l. c.), and in a position which shows that it could not have been the opisthotic. The homologizing of one or the other of these elements with the paroccipital of the Pythonomorpha is too clearly among the possibilities to be negatived by any evidence to the contrary yet brought forward by Dr. Baur. In fact the origin of the opisthotic element as an ossification about the posterior semicircular canal, renders it a priori probable that an osseous body at a distance from that center, such as the distal part of the paroccipito-opisthotic bones in the Testudinata, was originally distinct, and for this element the name paroccipital is appropriate.

2. *The Exoccipital and Quadrate.*—Dr. Baur again denies that the exoccipital articulates with the quadrate in certain genera of Iguanidæ and gives some figures of that region in the *Conolophus subcristatus* to sustain his allegation. Unfortunately, though he seems to have taken the elements apart, as I suggested that he do, he did not put them together in their original relation when he had them drawn. I now give two drawings traced from the plate of the skull of the same species given by Dr. Steindachner.[6] As these plates represent exactly the characters which I have observed and described in allied genera, I regard them as correct. It will be observed that there is a considerable contact between the exoccipital and the quadrate. There is also con-

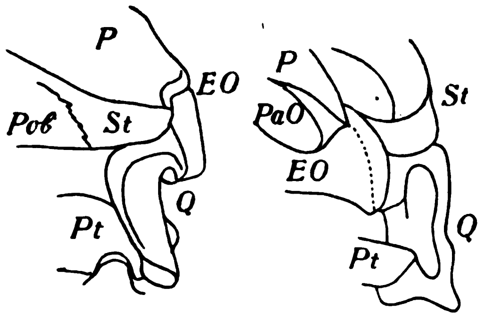
[3] Proceeds. Geolog. Soc. London, 1859, p. 50.

[4] Proceeds. Amer. Assoc. Adv. Sci., 1871, p. 207.

[5] Proceeds. Amer. Philos. Soc., 1885, p. 236; 1895, pl. VIII, fig. 4, otic region of Chilonyx.

[6] Die Schlangen u. Eidechsen der Galapagos Inseln, K. K. Zoolog. Botan. Gess. Wien, 4to. 1876; pl. V.

tact with the supratemporal, and probably with the paroccipital. *The articulation of the quadrate with the exoccipital is universal in the Iguanidæ.* I will, however, here call Dr. Baur's attention to the fact that I nowhere stated that the quadrate of the Lacertilia does not



Conolophus subcristatus Gray. From Steindachner.

touch the supratemporal or the paroccipital. I have simply asserted that the quadrate in the Lacertilia articulates with the exoccipital, and does not do so in the Ophidia. I will notice later some exceptions to the rule, of which I have since obtained information. The articulation with the supratemporal (prosquamosal, Baur) is naturally not to be mentioned in a diagnosis of the order Lacertilia, as there are numerous genera of that suborder in which that element is wanting.—E. D. COPE.

Boulanger on the Difference between Lacertilia and Ophidia; and on the Apoda.—In a recent note,[7] Dr. Boulenger criticizes my definitions of the above suborders and the Pythonomorpha as published in a late number of this journal.[8] He objects to the definition which I gave the Lacertilia, viz.: that the quadrate bone articulates with the exoccipital; and to that of the Ophidia, which asserts that this articulation does not take place. He suggests that I must have examined very few types of Lacertilia or I would not have made such a statement, and he mentions two or three other elements with which the quadrate bone also articulates. Now I must call Dr. Boulanger's attention to the fact that I nowhere state that the quadrate does not touch the other elements he has referred to, but have, on the contrary, stated that they do so touch. The doctor has apparently not read my article carefully, or has injected into his reading some-

[7] Annals and Magaz., Nat. History, XVI, 1895, p. 367.
[8] AMERICAN NATURALIST, 1895, p. 859.

thing which is not there. What he states in his note that bears on my definition is, that the quadrate in Chlamydosaurus does not articulate with the exoccipital, and he gives a figure to substantiate his opinion. I would have preferred to have seen a figure of this structure with the quadrate in place, but I have been able to confirm the observation by the examination of the skull of another Agamid, *Phrynocephalus olivierii.* Here the quadrate articulates with the paroccipital (supratemporal., Boul., squamosal, Baur), and is in contact with the supratemporal (squamosal, Boul.), and little or not at all with the exoccipital. How far the family of the Agamidæ generally present this structure I am unable to say, as but few skeletons of this family are at my disposal. As regards other families I have examined abundant material, as stated in my last communication (l. c., Nov. p. 1004). In review I may say that it is self evident that in general the distinction that I have drawn betweeen Lacertilia and Ophidia in this respect is valid, (1st) because the supratemporal is frequently absent, and therefore not diagnostic; (2d) because the extremity of the paroccipital is insignificant, and affords insufficient support; (3d) because the paroccipital process of the exoccipital is the only remaining element sufficient for the purpose.

Dr. Boulenger next attacks my definition of the Ophidia, alleging that the quadrate articulates with the petrosal (proötic) or with that element and the exoccipital. Here again I am supposed to have stated that the quadrate does not articulate with the (proötic) petrosal, when in fact, I did not mention that element. As to articulation with the exoccipital, I do not consider that this can be regarded as established until the embryology and paleontology are looked into. Because an element cannot be seen in an adult skull, it does not follow that it does not exist. The paroccipital is present in the Tortricidæ, and it is, so far, only an assumption to suppose that it is not represented in the allied Uropeltidæ, and in the less allied Epanodonta and Catodonta.

The decurvature of the parietals and frontals to the basicranial axis in snakes has been cited since Müller, by Huxley,[9] as peculiar to that order, and I know of no exceptions so far as regards the parietals. The optic foramina in some snakes with large eyes are confluent, as I have long been aware, and this foramen is at the expense of the inferior part of the frontals. This, however, does not produce the character of the Lacertilia, and the definition is not invalidated, as Dr. Boulenger alleges; nor is it by the decurvature of this bone and the parietal in the Amphisbænia, where they *do not reach* the sphenoid. I

[9] Anatomy of Vertebrated Animals, p. 203.

did not "admit," as alleged by Boulenger that these Lacertilia "agree with the Ophidia," as they do not. Dr. Boulenger asks "what," under these circumstances, "remains of Prof. Cope's new definition of the suborders of the Squamata?" From what has preceded it is evident, first, that they are not "new" except as to the exoccipital; and second, that they remain intact, so far as any evidence to the contrary has been produced by Dr. Boulenger, except as to the articulation of the quadrate with the exoccipital in two genera of Agamidæ. And it is not necessary to observe that very few groups so closely allied as the Lacertilia and Ophidia can be defined without exceptions.

If we now look at the definitions given by Dr. Boulenger in his volumes of catalogues of lizards and of snakes in the British Museum, the necessity of something better becomes at once apparent. The Lacertilia are thus defined: "Quadrate bone articulated to the skull; parts of the ali- and orbitosphenoid regions fibrocartilaginous; rami of the mandible united by suture; temporal region without or with only one horizontal bar. Anal cleft transverse. Copulatory organs paired. *Gunther.*"

The definition of the Ophidia (dated 1893) is as follows: "Quadrate bone articulated to the skull; brain capsule entirely osseous; rami of the mandible articulated by ligament. Anal cleft transverse. Copulatory organs present paired. *Gunther.*"

In these definitions the first and last two are identical in both. The presence or absence of a horizontal bar is not definitive, and indeed no reference to it is found in the definition of the Ophidia. The only definitions left are those derived from the mode of union of the symphysis mandibuli, and the ossification of the brain case. The former of these characters is not found in several families of Lacertilia, and the latter is the one which Dr.Boulenger has repudiated in the note which gave origin to this reply. I think my attempts at definition do, in point of precision and application, compare very favorably with those which seem to have satisfied Dr. Boulenger in the work cited. In another publication he gives the characters usually employed, which are much better.

In a recent synopsis of the species of Cæciliidæ,[10] Dr. Boulenger makes some observations on the relations of this family to the rest of the Urodela. He remarks: "If the absence of the limbs and reduction of the tail were the only characteristic of the group, I should, of course, not hesitate to unite the Cæcilians with the Urodeles; but, to say nothing of the scales, the Cæcilian skull presents features which are not shared by any of the tailed Batrachians, and the order can be

[10] Proceeds. Zool. Soc., London, 1895, p. 402.

defined by the cranial characters alone. The resemblance of the larval Ichthyophis to Amphiuma is after all superficial, and although, as I believe, the Apoda and Caudata may have evolved from a common stock, Amphiuma is certainly not the connecting form between the two as Prof. Cope would have it, for we cannot well assume the scales, lost in the Urodeles, to have reappeared in the Cæcilians."

The above discussion is interesting but troublesome, because it requires a reply. In the first place, it ought not to be necessary to remark that the presence or absence of scales in the Batrachia is not an ordinal character. On the page following that from which the above is quoted, Boulenger states that six of the sixteen genera of Apoda (Cæciliidæ) *have no scales.* Further, among the extinct Stegocephalia some genera have scales and some have none, so there is reason to suppose that scales may be secondary as well as primary. Moreover, if a a genus of salamanders should be discovered which possesses scales, no one would think of removing it from the Urodela on that account. There is no improbability in the supposition that such a genus may not be found in some of the Mesozoic formations. Second, Prof. Cope has never stated that the genus Amphiuma is the connecting form between the Apoda and Caudata. He has said that the Amphiumoidea probably are, and possibly the Amphiumidæ, but the genus Amphiuma never.[1] He has very rarely alleged that any genus is ancestral to any other genus. There can be no one genus between these two groups, for there is room for several genera. And one may agree with Dr. Boulenger that the Apoda and Caudata have had a common ancestor, and not disagree with the position that the Apoda belong to the Caudata, for there is no reason why that common ancestor may not probably have been one of the Caudata, unless there is more difference in the cranial characters of the two than has been yet pointed out.—E. D. Cope.

ENTOMOLOGY.[2]

Heterocera of the Lesser Antilles.—Reporting on a collection of Geometridæ and allied families from the islands of Grenada, St. Vincent and the Grenadines, Mr. G. F. Hampson[3] says. The Geometridæ

[1] Batrachia of N. America, 1889, pp. 34–222.

[2] Edited by Clarence M. Weed, Durham, N. H.

[3] Ann. and Mag. Nat. Hist., XVI, 329.

are represented by very few species in the Lesser Antilles compared with the large number that exist in other parts of the Neotropical Region both north and south of the isthmus; and almost all the species are identical with those found on the mainland.

The Pyralidæ are represented by a much greater diversity of species; but these, as in other parts of the world, are very wide ranging, most of the species being also found in Brazil and Venezuela, some being identical with forms found in the United States, whilst others range down to Chili; others again being spread throughout nearly the whole tropical zone; whilst, even of the species described as new, several are represented in the British Museum or other collections by specimens from continental localities.

Bot Flies of the Horse.—Prof. H. Garman publishes[1] an interesting account of the habits of oviposition of *Gastrophilus nasalis* and *G. equi*. He enumerates five species of bot flies attacking the horse in America; the adults may be distinguished by the following key:

1 (6) Discoidal cell closed by a cross vein.

2 (3) Wings marked with brown . . . . . *G. equi.*

3 (2) Wings not marked with brown.

4 (5) Anterior basal cell nearly or quite equal to the discoidal cell in length . . . . *G. nasalis.*

5 (4) Anterior basal cell markedly shorter than the discoidal cell [*G. hæmorrhoidalis.*

6 (1) Discoidal cell not closed . . . . *G. pecorum.*

Concerning the habits and life history of *G. equi*, the most abundant species, Professor Garman writes:

This fly buzzes about horses during the hot summer days, occasionally alighting on their bodies, and when an opportunity offers, placing its eggs in the hairs on the inside of the knee, on the shoulders, and sometimes even on the mane. Its mouth-parts are in a rudimentary condition, and it can not, even if it were disposed to, do any injury to horses.

It is probable that the grubs recently hatched from the eggs of this fly are taken into the mouths of horses on the lips or tongue. I am told by a gentleman who has had much experience with horses that he has on many occasions taken the eggs between the moistened palms of his hands, and in a few moments felt the young grubs wriggling about. It appears that moisture accelerates the hatching of the eggs, and it is just possible that many eggs would never hatch at all if the eggshell

[1] 7th Rept. Kentucky, Agr. Exp. Station.

was not moistened in some way. Whether this must be from the horse's tongue or lips in all cases is a question which may be considered not yet settled. Professor H. Osborn, of Iowa, is disposed to believe that the young do not hatch unless moistened by the horse's tongue; that the young grubs generally die in the eggs if left for 35 to 40 days; and that they are not commonly ready to hatch until from 10 to 12 days after the eggs are laid.

Fossil Butterflies.—Fossil butterflies are the greatest of rarities. They occur only in tertiary deposits, and out of the myriads of objects that have been exhumed from these beds in Europe and America less than twenty specimens have been found. The great body of these deposits is of course of marine origin, but at least thirty thousand specimens of insects have been recovered from those beds which are not marine. Over fifty thousand insects from the one small ancient lake of Florissant, high up in the Colorado Parks have passed through my hands, yet I have seen from them but eight butterflies. Each of these belongs to a genus distinct from the others, as is also the case with all or all but one, of the butterflies found at Radoboj, at Aix, and at Rott in the European tertiaries. With two (European) exceptions, each represents an extinct genus, and these two exceptions, Eugonia and Pontia, are genera found to-day both in Europe and America. The species, however, are all extinct.

One would hardly expect that creatures so delicate as butterflies could be preserved in a recognizable state in deposits of hardened mud and clay. Yet not only is this the case, but they are generally preserved in such fair condition that the course of the nervures and the color patterns of the wings can be determined, and even, in one case, the scales may be studied. As a rule they are so well preserved that we may feel nearly as confident concerning their affinities with those now living as if we had pinned specimens to examine; and generally speaking the older they are the better they are preserved.—*S. H. Scudder in Frail Children of the Air.*

Origin of European Butterflies.—Mr. W. H. Bath in discussing[4] the effects produced by the glacial period upon the distribution and diversity of European butterflies says: As the result of his investigations Ernest Hoffmann asserts that of the 290 species of Rhopalocera inhabiting our continent at the present time, no less than 173 were originally derived from Siberia. If this was the case, and it seems very

[4] The Entomologist, XXVIII, 247.

likely to be correct, the majority of them probably immigrated westwards of the commencement of the pleistocene periods, for they must be of great antiquity; moreover it is unreasonable to suppose that many of the species could have existed also in the south of Europe, even at the climax of the glacial period. According to the same authority only 8 species have been derived from Africa, and 39 from Asia south of Siberia. These must have immigrated into the south European province of the palearctic region after the termination of the glacial period as they belong to genera and types of tropical distribution. At the present day they occur in those countries bordering on the Mediterranean Sea.

The glacial species of butterflies—that is the most ancient forms, designated by Weismann "the original stirps"—are in many cases distinguished by their melanic and melanochroic tendencies. We thus find the forms inhabiting the more northern localities and the higher elevations on the mountains often of a darker hue, while their representatives in more southern latitudes and less elevated altitudes exhibit a brighter coloration.

North American Aphelininæ.—As the first of a technical series of bulletins to be issued by the Division of Entomology of the U. S. Department of Agriculture, Mr. L. O. Howard publishes a Revision of the Aphelininæ of North America. Regarding the biology of the group Mr. Howard writes: The insects of this subfamily are all, so far as we know, parasitic either upon the Coccidæ, Aleyrodidæ, or Aphididæ. They are evidently many brooded, and issue from their hosts indifferently throughout the warmer months of the year, and through the winter on the insectary. With the Aleyrodidæ, Aphididæ, and the Diaspinæ among the Coccidæ, but one specimen apparently issues from a single host. Sufficient observations have not been made upon the early stages of the Aphelininæ. Their larvæ feed both upon the body of the scale insect and upon the eggs. They attack both sexes of the host, issuing when full-grown through circular holes, cut through the body walls, and, in the case of the Diaspinæ through the scale. With the scale insects of the genus Pulvinaria, the aphelinine larvæ live within the body of the female and not in the waxy egg mass which she secretes.

News.—A List of Night-flying Moths from Kentucky, is published by Prof. H. Garman in the 7th Report of the Experiment Station of that State.

An extended account of the life-history of *Phryganidia californica* Packard is published by Messrs. V. L. Kellogg and T. J. Jack in the Proceedings California Academy of Sciences (Ser. 2, V, 562–570.)

Prof. J. B. Smith issues as Bulletin 111 of the New Jersey Experiment Station an account of experiments with "Raupenlime" and "Dendrolene," substances useful for applying to tree trunks to keep out borers.

PSYCHOLOGY.[1]

American Psychological Association.—The American Psychological Association held its annual meeting this year at the University of Pennsylvania, in connection with the meetings of the scientific societies affiliated with the American Society of Naturalists. Hitherto the Psychological Association has met independently, but the feeling has been growing that the close relation between the more recent forms of psychology and the biological sciences made it eminently suitable and desirable that their representatives should be brought together. The success which has attended this first step makes it probable that the policy will be continued in future.

No official outline of the proceedings of the Psychological Associaciation is at hand, and any account written from memory will be more or less defective. Consequently the present writer must beg indulgence from those whose words he endeavors to report if he has, in any case, misrepresented them. On the whole, however, he believes he is giving a fair outline of the more important points.

At the first session, on Friday, Dec. 27th, the opening paper, on "Physiology and Psychology," was read by Prof. George S. Fullerton of the University of Pennsylvania. Two years ago, at the New York meeting of the Association, Prof. Fullerton outlined the relation in which psychology as a natural science stands to metaphysic, and concluded that psychology should adopt, as far as possible, the methods and assumptions of the other natural sciences, and should relegate the task of criticising those assumptions to a distinct science—that of metaphysic. The paper read this year was a continuation of the same general line of thought in the investigation of the relations of psychology and physiology. Taking Foster's "Physiology" as a standard, we find, said Prof. Fullerton, that the author is absolutely unable to give any

[1] This department is edited by Dr. Wm. Romaine Newbold, University of Pennsylvania.

account of the functioning of the higher nervous centres without having recourse to sensations, ideas, volitions—in a word, without entering the field that properly belongs to psychology. While it may be not only right, but also necessary, for the physiologist to do this, we must not close our eyes to the fact that the mere fact of its necessity proves the imperfect condition of physiology, and tends to obscure the line dividing physiology from psychology. Prof. Fullerton claimed that the methods employed by the two sciences are distinct, and that it is important to the advancement of knowledge to recognize this distinction.

Dr. Livingston Farrand, of Columbia, submitted a scheme of physical and mental tests which will be used with the students of Columbia to determine, as far as can be done by direct experiment, their capacities in both respects at various stages of their college life. After some discussion, a motion was passed that the President be requested to appoint a committee of five to report upon the advisability of the universities represented taking concerted action in the adoption of some similar scheme.

Dr. Arthur MacDonald, of Washington, D. C., read a paper on "Some Psycho-Neural Data." He reported experiments somewhat similar to those of Dr. Farrand, made upon certain groups in the community, and apparently showing that between definite classes definite physical and mental differences are experimentally discoverable.

Prof. Lightner Witmer, of the University of Pennsylvania, introduced one of his graduate students, Mr. Oliver Cornman, who reported the results of "An Experimental Investigations of the Processes of Ideation." Mr. Cornman's method was that of giving a large number of individuals, usually children, a definite suggestion and requiring them to write for a definite period of time—usually 15 minutes—all the thoughts directly or indirectly suggested by it; he had found that in most of his subjects tne idea trains were, for a short time, largely controlled by the concomitant suggestions of the time and place, and consequently the earlier terms of each series showed a marked similarity. This soon disappeared, and the further development of the idea trains seemed dependent upon the character and previous experience of the individual. We have, therefore, in this, a convenient method of "tapping," as it were, the ideational content of the individual. Mr. Cornman pointed out further, that, to get results at all comparable with one another in the case of different bodies of subjects, the original suggestions must be given in identically the same words without explanations or further suggestions on the part of the experimenter, and, to secure this end, should always be written.

At the afternoon session on Friday, Prof. J. McK. Cattell, of Columbia, read his President's Address. It was, on the whole, a defense of that experimental method of which he is the leading representative in this country, and was, therefore, in a way, a reply to the rather unfavorable estimate of the method and its results which had been expressed by Prof. James of Harvard in his President's Address of the preceding year. The burden of Prof. Cattell's argument was found in the statement, that every science is either genetic or quantitative in its method; that those sciences which have been predominently quantitative will undoubtedly, in time, be formulated in genetic terms, that, conversely, into the genetic sciences also, such as biology and psychology, the quantitative method will ultimately be introduced. This is the aim of experimental psychology in the narrower sense. While expressing the strongest conviction of the importance of this experimental method to the science of psychology, Prof. Cattell displayed such moderation in his estimate of the results thus far achieved by it, and such sympathetic insight into the aims and relative values of other methods, that his address was received with the warmest applause by all, and no one could be found to pass a criticism upon it.

Prof. Chas. A. Strong, of the University of Chicago, read a paper on "Consciousness and Time," of which, on account of its exceedingly abstract character, I could not venture to give an analysis from memory.

The morning of Saturday, December 28th, was occupied by a discussion on "Consciousness and Evolution."

Prof. William James, of Harvard, opened the discussion by outlining the general features of the problem at issue: First, whether consciousness is coextensive with the universe or originated in time; second, whether consciousness is an active force capable of controlling brain movement, or whether it is a mere epiphenomenon, produced by the brain but not capable of affecting the brain; third, whether consciousness has been a factor in the production of adaptation.

Prof. Cope, of the University of Pennsylvania, who had been especially requested to take the leading part in the discussion, attacked the question from the point of view of the paleontologist. He held that natural selection is not sufficient to account for adaptation, that the adaptation of the individual organ is the result of use, and that the effects of use can be inherited. In supporting this position he gave many illustrations, based upon his personal observation. He held further that organic evolution involved combinations and recombinations of matter which not only never could have been produced by the opera-

tion of known physical and chemical forces, but were of a character precisely the opposite of their known effects. To account for this, he thought we must assume in organic matter the existence of an activity distinct from all the other activities of nature. Progressive evolution is the chief outcome of this activity, and therefore he had proposed to term it an anagenetic, or upbuilding activity, as opposed to the katagenetic or destructive activities of physics and chemistry. This anagenetic activity Prof. Cope was inclined to believe due to the presence of sensation, and therefore maintained that consciousness is an active factor in the individual and in evolution.

Prof. Cope was followed by Prof. J. Mark Baldwin, of Princeton, who commented upon several points of Prof. Cope's argument, drawing special attention to the fact that recent investigation into the effect on young children of their surroundings makes it more easy to account for adaptation without reference to inheritance of acquired aptitudes. He also deplored the sharp antithesis between the doctrine of consciousness as a cause and as a epiphenomenon, holding that both views found their reconciliation in monism.

Prof. C. Sedgwick Minot, of Harvard, attacked the neo-Lamarckian doctrine from the neo-Darwinian point of view, supporting his position by evidence drawn from his own work in embryology. He suggested, as a speculation, that consciousness, although not itself a force, might be conceived to possess the property of selecting out of the brain forces that one which it is control conduct.

Prof. G. S. Ladd, of Yale, welcomed Prof. Cope's address as an important contribution from the purely scientific point of view to the support of doctrines held by himself in common with many other metaphysicians, and made a plea for the recognition of the metaphysician on the part of scientists as a coworker in the field of knowledge.

Prof. Fullerton, of the University, called attention to our actual ignorance on all these points, and expressed the opinion that fundamental differences exist which cannot be glossed over by such vague doctrines as that of monism.

Other speakers were: Prof. J. H. Hyslop, of Columbia; Dr. D. S. Miller, of Bryn Mawr, and Dr. Wesley Mills, of McGill University, Montreal.

Prof. Cope then concluded the discussion by adducing a series of arguments in favor of the inheritance of acquired attributes, any one of which, he held, would be sufficient to set the matter at rest.

At the afternoon session, Prof. G. T. W. Patrick, of the University of Iowa, reported an experiment on the effects of loss of sleep. A patient

had been kept awake for 90 consecutive hours, during which time careful experimental tests were made of his physical and mental condition, and the results were reported in detail. Among the more interesting of these results were, continuous increase in weight, relatively slight loss of muscular strength, the production of visual hallucinations, and the sudden disappearance of all symptoms after only 10½ hours of sleep—about 25 per cent. of that which had been lost.

Prof. Wesley Mills, of McGill University, Montreal, announced his intention of contributing at the next meeting of the Association further researches on the psychic development of young animals and its physical correlations.

Prof. Lightner Witmer, of the University of Pennsylvania, read a paper on "Variations in the Patellar Reflex as an Aid in Mental Analysis. Dr. Witmer described the apparatus and the method used to determine, 1st, The extent of the normal jerk; 2d, the increment due to the synergic activity of the cortical processes concerned in sensation, thoughts, etc. His results he regarded as tentative only; they appeared, however, to show (1) that sensation or thought processes which did not directly tend to produce movement had little effect upon the knee jerk; (2) that all processes which tended to produce muscular contraction in any part of the body tended to increase the knee jerk; (3) that this increase was quite as marked in the case of the *thought* of a movement as in that of the movement itself.

Prof. James H. Hyslop, of Columbia, reported a series of experiments on hallucinations induced by a crystal. He did not attempt to give any explanation of the phenomena, but pointed out that in two cases the phantasms possibly indicated some unknown method of acquiring information.

Prof. W. R. Newbold narrated informally three cases vaguely described as "Dream Reasoning," which had occurred in the experience of two of his colleagues. Dr. W. A. Lamberton, Professor of Greek in the University of Pennsylvania, when a young man, after giving up as insoluble a problem in descriptive geometry upon which he had been working for weeks by the analytical method, awoke one morning several days later to find an hallucinatory figure projected upon a blackboard in his room with all the lines necessary to a geometrical solution of the problem clearly drawn. He has never had any other visual hallucination. Dr. H. V. Hilprecht, Professor of Assyriology in the University of Pennsylvania, some years ago dreamed an interpretation of the name Nebuchadnezzar which has since been universally adopted. At a later period he dreamed that an Assyrian priest

gave him information about some inscribed fragments that had puzzled him which was afterwards confirmed in all points now capable of confirmation. Dr. Newbold offered a psychological explanation of these curious cases.

Prof. G. S. Fullerton, of the University of Pennsylvania, was elected President, and Dr. Livingston Farrand, of Columbia, Secretary, for the ensuing year.

Among the members present, besides those already mentioned, were Mr. Henry Rutgers Marshall, of New York; Prof. N. S. Gardiner, of Smith College; Dr. H. C. Warren, of Princeton; Prof. E. S. Sanford, of Clarke University; Prof. E. H. Griffen, of Johns Hopkins; Prof. J. C. Creighton, of Cornell; Prof. James Seth, of Brown, and Dr. Warner Fite, of Williams' College.—W. R. N.

The Cat's Funeral.—Every one has observed instances of affection between those proverbially hostile animals, the dog and the cat, but a case cited by l'Eleveur merits especial attention. A dog and a cat belonging to the same master were the best friends in the world, and spent their time in frolicking together. One day, while playing as usual, the cat died suddenly, falling at the dog's feet. The latter, at first, did not realize what had happened, but continued his play, pulling, pushing and caressing his companion, but with evident astonishment at her inertness. After some time he appeared to understand the situation, and his grief found vent in prolonged howls. Presently he was seized with the idea of burying the cat. He pulled her into the garden, where he soon dug a hole with his paws, and put in it the body of his former companion. He then refilled the hole with dirt, and, stretching himself out on the grave, resumed his mournful howling. The idea of burying the dead cat was extraordinary. Whence came the thought? Could it be imitation, or, which is a better explanation, did the dog have a vague idea of concealing the event which might possibly be imputed to him. But then it would seem unreasonable for him to call attention to the fact, by installing himself on the grave and howling. However, even human criminals are sometimes equally inconsistent. It is difficult to form an exact idea of what gave rise to the dog's conduct in this case. (Revue Scientific Juillet, 1895).—E. D. C.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Academy of Arts and Sciences.—The 11th of December.—The following papers were read: On the temperature of the crust of the earth at great depths. By Messrs. Alexander Agassiz and P. C. F. West. Palestine in the fifteenth century B. C. according to recent discoveries. By Professor Crawford H. Troy.

Boston Society of Natural History.—December 4th.—The following paper was read: Mr. L. S. Griswold, "The San Francisco Mountains and the Grand Canyon."

December 18th.—The following paper was read. Prof. G. Frederick Wright, "The present status of glacial man in America." The subject of Professor Wright's paper was discussed by Prof. F. W. Putman, Prof. H. W. Haynes, and others.

January 1st, 1896.—The following papers were read: Mr. A. W. Grabau, "Lake Bouvé, a glacial lake in the Boston Basin;" Prof. W. O. Crosby, "Glacial lakes in the valleys of the Neponset and Charles Rivers; and the Post-tertiary history of the Nashua Valley.—SAMUEL HENSHAW, *Secretary.*

January 15th.—The following paper was read: Mr. William Brewster, Notes on the Natural History of Trinidad. Stereopticon views were shown.—SAMUEL HENSHAW, *Secretary.*

New York Academy of Sciences, Section of Biology.—December 9, 1895.—The following papers were presented: Prof. C. L. Bristol, "The Classification of Nephelis in the United States." The study of abundant material, collected from Maine to South Dakota, has shown that the color characters cannot be depended upon for specific determination. An examination of the metameral relations of this leech indicate that not more than a single species occurs in this country. Prof. F. H. Osborn, "Titanotheres of the American Museum of Natural History." The complete skeleton of *Titanotherium robustum* is remarkable in possessing but twenty dorso-lumbar vertebræ, a number identical with that typical of the Artiodactyla, but entirely unique among Perissodactyla. It is now appears probable that the development of horns in the Titanotheres became a purely sexual character, and that the genera Titanops, Marsh and Brontops, Marsh, are founded respectively upon male and female individuals of *Titanotherium robustum.* Dr. J. L. Wortman, "The expedition of 1895 of the Amer-

ican Museum of Natural History." The Expedition passed into the Uinta beds of N. E. Utah, then between the Eastern escarpment of the Uinta range and the Green River into the Washakie Beds of S. W. Wyoming, the most important result geologically being that the Brown Park deposit is found to be of much later age then the Uinta.—BASHFORD DEAN, *Recording Secretary.*

American Philosophical Society.—The following communications were read: "The Use of Photography for the Detection of Differences in Chemical Composition, in Age, and in Fluidity of Inks," Prof. S. P. Sharples. "Some Observations on the Forgery of a Mark," and "Detection of a Forgery in the Fraudulent Use of a Signature Stamp," Dr. Persifor Frazer.

Academy of Natural Sciences.—Philadelphia, December 31st.—The following officers were elected: President, Samuel G. Dixon, M. D.; Vice-Presidents, Thomas Meehan, Rev. Henry C. McCook, D. D.; Recording Secretary, Edward J. Nolan, M. D.; Corresponding Secretary, Benjamin Sharp, M. D.; Treasurer, George Vaux, Jr.; Librarian, Edward J. Nolan, M. D.; Curators, Henry A. Pilsbry, Henry C. Chapman, M. D., Arthur Erwin Brown, Samuel G. Dixon, M. D.; Councillors to Serve Three Years, Uselma C. Smith, William Sellers, Charles E. Smith, John Cadwalader; Finance Committee, Charles Morris, Chas. E. Smith, Uselma C. Smith, William Sellers, Charles P. Perot; Council, Isaac J. Wistar.

The American Morphological Society held its annual meeting at the University of Pennsylvania, Dec. 26, 27, and 28, 1895. The stated business of the first session was the Report of the Committee of Affiliation with the American Society of Naturalists. After considering this report the Society voted against affiliation. The following were elected to membership: C. J. Herrick, Denison University, Granville, Ohio; E. G. Conklin, University of Pennsylvania, Philadelphia, Pa.; F. R. Lillie, University of Michigan, Ann Arbor, Mich.; F. C. Kenyon, Clark University, Worcester, Mass.; T. H. Montgomery, Jr., West Chester, Penna.; J. L. Kellogg, Olivet College, Olivet, Mich.; J. I. Peck, Williams College, Williamstown, Mass.; and A. D. Meade, Providence, R. I.

At the second session, December 27, the following papers were read and discussed: "Panplasm," by Prof. C. S. Minot; "The History of the Centrosome in Thalassema," by Mr. B. B. Griffin; "The Centrosome in its Relation to Fixing and Staining Agents," by Prof. E. B. Wilson; The Production of Artificial Archoplasmic Centers," by Prof.

T. H. Morgan; "Cell Size and Body Size," by Prof. E. G. Conklin; "The Development of Isolated Bastomeres of the Egg of Amphioxus," by Prof. T. H. Morgan; and "On the Smallest Part of Stentor Capable of Regeneration," by F. R. Lillie (read by the Secretary). The following officers were elected for the ensuing year: President, Prof E. L. Mark, Harvard University; Vice-President, Prof. H. F. Osborn, Columbia College; Secretary and Treasurer, Dr. G. H. Parker, Harvard University. Members of the Executive Committee elected from the Society at large, Prof. E. G. Conklin, University of Pennsylvania, and Prof. W. Patten, Dartmouth College.

At the third session, December 28, the following papers were read and discussed: "Gastrulation of Teleosts," by Dr. Bashford Dean; "Pigment Changes in the Eye of Palæmonetes," by Dr. G. H. Parker" "Reaction of Metridium to Food and other Substances," by Dr. G. H. Parker; "Some Points in the Anatomy of Anoplocephaline Cestodes," by Dr. C. W. Stiles; and "Development of Cassiopea from Buds," by Dr. R. P. Bigelow. After passing resolutions of thanks to the University of Pennsylvania, the American Philosophical, Society, and the Philadelphia Local Committee, the Society adjourned *sine die*.

The American Society of Naturalists,—Met in the Hall of Department of Arts and Sciences of the University of Pennsylvania, on Thursday December 26th and Friday, December 27th, 1895. *Thursday, Dec. 26th, 2 P. M.* I. Reports of Committees. II. Special Reports. III. Recommendation of new members. IV. Address by the President, E. D. Cope. "The Formulation of the Natural Sciences." V. Special Papers, Prof. B. Wilder on the teaching of Comparative Anatomy. *8 P. M.* Illustrated Lecture at the Hall of the Academy of Natural Sciences, by Professor W. B. Scott, of Princeton University, on "The American Tertiary Lakes and their Mammalian Faunas." *9 P. M.* Reception to all the Societies given by Professor Horace Jayne, at his house on the S. E. corner of 19th and Chestnut Streets. *Friday, December 27th, 9 A. M.* The following new members were elected: Professor C. L. Bristol, Dr. F. C. Kenyon, Dr. W. E. Rotzell, L. O. Howard, Professor John Dewey, G. H. Girtz, Dr. A. D. Mead, Professor G. S. Fullerton, Professor J. McK. Cattell, Professor G. T. Ladd, Reid Hunt, Professor William James, Dr. F. Baker, Dr. G. E. Stone, Professor J. M. Baldwin, Dr. T. S. Palmer, George Lefever,

The following officers were elected for the ensuing year: President, Prof. Wm. B. Scott, of Princeton College; Vice-Presidents, Prof. Wm. G. Farlow, of Harvard; Prof. C. O. Whitman, of Chicago University; Dr. Theodore Gill, of the Smithsonian Institution; Secretary, Dr. H.

C. Bumpus, of Brown University; Treasurer, Prof. John B. Smith, of New Brunswick, N. J.; Executive Committee, Prof. Horace Jayne, of Philadelphia, and Prof. Wm. F. Ganong, of Smith College, Mass.

The following committees were apppointed: On Vivisection; Drs. Patton, Sedgwick and Stiles. On the American table at the Naples Zoological Station; Drs. Conn and Stiles. On Antarctic exploration; Professors Heilprin, Osborn and Goodale. The Society elected Prof. E. D. Cope as its representative on the committee to consult with the American member of the committee of the International Congress of Zoologists on Nomenclature. 10 *A. M.* Discussion. *Subject:* The Origin and Relations of the Floras and Faunas of the Antarctic and Adjacent Regions. Geology. Prof. Angelo Heilprin, Philadelphia Academy Natural Sciences. Paleontology. Prof. W. B. Scott, Princeton University. 2 *P. M.* Continuation of the Discussion. Botany. Prof. N. L. Britton, Columbia College. Zoology. Vertebrata, Dr. Theo. Gill, Smithsonian Institution. 7.30 *P. M.* Annual Dinner of the Affiliated Societies at the Lafayette Hotel, north-west corner of Broad and Sansom streets.

Association of American Anatomists.—This body met in Philadelphia, on Dec. 27th and 28th, at the University of Pennsylvania.—*Friday Morning, December 27th,* 8.30 o'clock.—Meeting of Executive Committee. 9.30 o'clock.—Opening of the session by the President. Report of Secretary and Treasurer. Report of Executive Committee. Report of Delegate to Congress of American Physicians and Surgeons. Report of Committee on Anatomical Nomenclature. Report of Committee on Anatomical Material. Report of Committee on Circular concerning Anatomical Peculiarities of the Negro. Report of Dr. Allen, of the Smithsonian Committee on the Table at Naples. Election of members. Other new business. Reading of Papers and Discussions.—1. "Myology of the Extremities of Lemur bruneus" illustrated by drawings and casts of muscles. Dr. George S. Huntington, N. Y. City; 2. "History of the Ciliary Muscle," Dr. Frank Baker, Washington, D. C.; 3. "Absence of Fibrous Pericardium of left side." Illustrated by specimen, Dr. Addinell Hewson, Philadelphia, Pa. "The Descriptive Anatomy of the Human Heart," Dr. Wm. Keiller, Galveston, Texas. *Friday Afternoon,* 2.30 oclock.—Miscellaneous business. Reading of Papers and Discussions.—5. "Nomenclature of Nerve Cells," Dr. Frank Baker, Washington, D. C.; 6. "The Cerebral Fissures of two Philosophers." Illustrated by specimens and photographs, Dr. B. G. Wilder, Ithaca, N. Y.; 7. "The Human Paroccipital Fissure. Should it be recognized and so Designated." Illustrated by specimens and photographs, Dr.

Wilder; 8. "Practical Histology for large classes" Dr. Chas. S. Minot, Boston, Mass. In the evening a subscription dinner was given by the members of the affiliated societies at the Hotel Lafayette. *Saturday Morning, December 28th.*—Miscellaneous business of minor importance was transacted and these officers elected: Dr. Frank Baker, of Washington, D. C., President; Dr. B. G. Wilder, of Ithaca, N. Y., First Vice-President; Dr. F. J. Shepherd, of Montreal, Canada, Second Vice-President; Dr. D. S. Lamb, of Washington, D. C., Secretary and Treasurer; Delegate to Congress of American Physicians and Surgeons, Dr. Addinnell Hewson, Philadelphia; Alternate, Dr. D. K. Shute, of Washington, D. C. Reading of Papers and Discussions.—9. "Some novel methods of description of the human skull" Dr. Harrison Allen, Philadelphia, Pa.; 10. "Type forms and nomenclature of mammalian teeth." Illustrated by models and diagrams, Prof. Henry F. Osborn, New York City; 11. "The work of the German Anatomical Society in Nomenclature," Dr. Charles Heitmann, New York City. *Sunday Afternoon,* 2.30 o'clock.—Miscellaneous business. Reading of Papers and Discussions; 12. "*Fossa capitis femoris* with observations on the trochanteric fossa." Illustrated by specimens, Dr. F. J. Brockway, New York City; 13. "Note on the appearance of a unilateral tuberosity in place of the trochanteric fossa." Illustrated by specimen, Dr. D. S. Lamb, Washington, D. C.

The American Physiological Society.—The eighth Annual Meeting of the American Physiological Society was held in Philadelphia on December 27th and 28th, 1895. The meeting was preceded by the usual smoke talk upon the evening of December 26th. Three of the four formal sessions of the Society were held at the University of Pennsylvania, the fourth at the Jefferson Medical College, The following communications were presented and discussed:

R. H. Chittenden, The mucin of the white fibrous connective tissue; A. R. Cushny, The distribution of iron in the Invertebrates; J. J. Abel, A preliminary account of the chemical properties of the pigment of the negro's skin (with W. S. Davis); T. B. Aldrich, On the Chemical and physiological properties of the fluid secreted by the anal glands of Mephitis mephitica; G. Lusk, Phloridzin diabetes and the maximum of sugar from proteid; W. T. Porter, Further researches on the coronary arteries; G. N. Stewart, Note on the quantity of blood in the lesser circulation; C. F. Hodge, Histological characters of lymph as distinguished from protoplasm; C. F. Hodge (for J. R. Slonaker), Demonstration of the comparative anatomy of the fovea centralis; G. C. Huber, The ending of the chorda tympani in the sublingual and the

submaxillary glands (with demonstrations); G. W. Fitz, A working model of the eye; J. G. Curtis, A method of recording muscle curves; G. N. Stewart, Demonstration: Measurement of the circulation time of the retina; T. W. Mills, Cortical cerebral localization in certain animals; W. T. Porter, A new method for the study of the intracardiac pressure curve; S. J. Meltzer, On the mode of absorption from the peritoneal cavity in rabbits, (with I. Adler); S. J. Meltzer, On the incorrectness of the often quoted experiments of Starling and Tubby with reference to the mode of absorption from the peritoneal cavity in dogs; F. S. Locke, Of the action of ether on contracture and of positive kathodic polarisation of voluntary muscle; H. G. Beyer, On the influence of exercise on growth; W. H. Howell (for Messrs. Conant and Clark), The existence of a separate inhibitory and accelerator nerve to the crab's heart; Fr. Pfaff, On Toxicodendral and on the socalled toxicodendric acid; H. C. Chapman, Methods of teaching physiology.

The following persons were elected to membership in the Society:

J. G. Adams, Professor of Pathology, McGill University; T. B. Aldrich, Instructor in Physiological Chemistry, Johns Hopkins University; J. M. K. Cattell, Professor of Experimental Psychology, Columbia College; G. P. Clark, Professor of Physiology, Syracuse University; R. H. Cunningham, Assistant Demonstrator of Physiology, Columbia College; G. W. Fitz, Assistant Professor of Physiology and Hygiene, Harvard University; T. Hough, Assistant Professor of Physiology, Massachusetts Institute of Technology; R. Hunt, Fellow in Physiology, Johns Hopkins University; F. S. Locke, Instructor in Physiology, Harvard Medical School.

Professors C. S. Minot and C. F. Hodge were appointed to express to Professor Langley the opinion of the Society that it is highly desirable that the table of the Smithsonian Institution at the zoological station of Naples be continued. Mr. W. B. Saunders entertained the members of the Society at luncheon at the Art Club. The courtesies that were extended to the affiliated societies by the University of Pennsylvania and the Philadelphia Local Committee were also enjoyed. Officers for the year 1895–96 were elected as follows:

Members of the Council: H. P. Bowditch, R. H. Chittenden, W. H. Howell, F. S. Lee, J. W. Warren; President, R. H. Chittenden; Secretary and Treasurer, F. S. Lee.

The President and the Secretary were appointed respectively delegate and alternate to the Congress of American Physicians and Surgeons of 1897.—Frederic S. Lee, *Secretary.*

The Geological Society of America held its eighth Annual Meeting, and the fifteenth meeting of the Society in the Geological Museum of the University of Pennsylvania, December 26th to 28th. The number of Fellows in attendance was sixty. The first session was convened at 2 o'clock on Thursday afternoon with President N. S. Shaler in the chair. The report of the Council, consisting of the detailed reports of the officers for the year 1895, was submitted in print. This report showed a properous condition of the Society; following are some of the items: membership 226, libraries subscribing for the bulletin 59, receipts during the year from the sale of the bulletin $461.50, number of exchanges 85. The library is deposited with the Case Library at Cleveland. Besides printing six volumes of the bulletin, $3000 has been invested as a publication fund.

Announcement was made of the election by transmitted ballots of officers for 1896 as follows:

President, Joseph LeConte; First Vice-President, C. H. Hitchcock; Second Vice-President, Edward Orton; Secretary H. L. Fairchild; Treasurer, I. C. White; Editor, J. Stanley Brown; Councillors, B. K. Emerson, J. M. Safford.

The following Fellows were declared elected: Harry F. Bain, Des Moines, Iowa; William K. Brooks, Baltimore, Md.; Charles R. Eastman, Cambridge, Mass.; Henry B. Kummel, Trenton, N. J.; William H. Norton, Mt. Vernon, Iowa; Frank B. Taylor, Fort Wayne, Ind.; Jay B. Woodworth, Cambridge, Mass.

A memorial of James D. Dana, written by Joseph LeConte, was read by H. S. Williams. This was not only an appreciative sketch of Dana's life, but an admirable discussion of the true character of geology as a science, and of the great influence of Dana in giving geology a commanding position.

Other short memorials of Henry B. Nason, Albert E. Foote and Antonio del Castillo were read.

A message of regard was voted to J. P. Lesley, who was unable to attend the meeting on account of illness.

The Society held a morning and an afternoon session on Friday and a morning session on Saturday. It was announced that the next summer meeting, to be held in August in connection with the American Association for the Advancement of Science, would be devoted chiefly to excursions.

The list of papers read was not as long as at the Baltimore meeting, but the program was of excellent quality. Following are the titles of the papers presented:

George P. Merrill, Disintegration and decomposition of diabase at Medford, Mass.; Charles R. Keyes, The geographic relations of the granites and porphyries in the eastern part of the Ozarks; J. F. Kemp, Illustrations of the dynamic metamorphism of anorthosites and related rocks in the Adirondacks; N. S. Shaler, The importance of volcanic dust and pumice in marine deposits; L. V. Pirsson, A needed term in petrography; John J. Stevenson, The Cerrillos coal field of New Mexico; N. S. Shaler, The relations of geologic science to education. (Presidential address), ; W. M. Davis, Note on the outline of Cape Cod; W. M. Davis, Plains of Marine and subaërial denudation; F. P. Gulliver, Cuspate forelands; M. R. Campbell, Drainage modifications and their interpretation; N. H. Darton, Some fine examples of stream robbing in the Catskill Mountains; Robert Bell, Proofs of the rising of the land around Hudson Bay; C. R. Van Hise, Movements of rocks under deformation; Alfred C. Lane, Possible depth of mining and boring; Harry Fielding Reid, Notes on glaciers; Frank Leverett, The relation between ice lobes, south from the Wisconsin driftless area; Frank Leverett, The loess of western Illinois and southeastern Iowa; G. Frederick Wright, High level terraces of the middle Ohio and its tributaries; H. L. Fairchild, Four great kame areas of western New York; Warren Upham, Preglacial and postglacial channels of the Cuyahoga and Rocky Rivers; C. H. Hitchcock, Paleozoic terranes in the Connecticut Valley; C. Willard Hayes, The Devonian formations of the southern Appalachians; N. H. Darton, Notes on relations of lower members of costal plain series in South Carolina; N. H. Darton, Resumé of general stratigraphic relations in the Atlantic costal plain from New Jersey to South Carolina; T. C. Chamberlin, The Natchez formations; Arthur Keith, Some stages of Appalachian erosion.

The American Psychological Association met at the University of Pennsylvania, Philadelphia, Friday and Saturday December 27 and 28, 1895.

Friday, December 27, 10 *A. M.*—Psychology and Physiology, Professor George S. Fullerton; Description of a Series of Physical and Mental Tests on the Students of Columbia College, Dr. Livington Farrand; Some Psycho-Neural Data, Dr. Arthur MacDonald; An Experimental Investigation of the Processes of Ideation, Mr. Oliver Cornman. (Introduced by Professor Lightner Witmer).

2.30 *P. M.*—Address of the President, Professor J. McKeen Cattell; Consciousness and Time, Professor Charles A. Strong; Some Conditions of Will Development, Brother Chrysostom; A Psychological Interpre-

tation of the Rules of Definition in Logic, Professor Alfred H. Lloyd.

Saturday, December 28, 10 *A. M.*—Discussion on Consciousness and Evolution, Professors William James, E. D. Cope, J. Mark Baldwin and G. S. Ladd.

2.30 *P. M.*—An Experiment on the Effects of Loss of Sleep, Professor G. T. W. Patrick; Further Researches on the Psychic Development of Young Animals, and its Physical Correlation, Professor Wesley Mills; Variations in the Patellar Reflex as an Aid in Mental Analysis, Professor Lightner Witmer; Experiments on Induced Hallucinations, Professor James H. Hyslop; A Case of Dream Reasoning, Professor W. Romaine Newbold.

Informal communications were made at various times during the sessions.

A fuller account of the papers and discussions will be found in our department of Psychology; q. v.

Indiana Academy of Science.—The eleventh Annual Meeting of the Indiana Academy of Science was held at Indianapolis, December 27th and 28th.

The session was of unusual interest and the attendance good. Forty two new members were elected. This indicates the interest that is being aroused in the State in scientific lines.

The address of the retiring President, A. W. Butler, of Brookville, on "Indiana: A Century of Changes in the Aspects of Nature," met with enthusiastic applause.

A poem on the "Naturalist" recited by W. W. Pfrimmer was a novel, yet enjoyable feature.

The report of the biological Survey of Turkey Lake was another new feature of the meeting, and attracted much favorable attention.

The following papers were presented:

Unconscious Mental Cerebration, C. E. Newlin; Human Physiology in its Relation to Biology, Guido Bell; A means of preventing Hog Cholera, D. W. Dennis; The Hopkins Seaside Laboratory at Pacific Grove, Cal., B. M. Davis; Glacial and Eolian Sands of the Iroquois and Tippecanoe River Valleys, A. H. Purdue; The recent earthquakes east of the Rocky Mountains, A. H. Purdue; Some minor processes of Erosion, J. T. Scoville; Kettle Holes at Maxinkuckee, J. T. Scoville; Fossils from sewer trenches in the Glacial Drift, Wm. M. Whitten; Relief map of Arkansas, John F. Newsom; Notes on the Fauna of the black shales of Bartholomew and Jackson Counties, V. F. Marsters; Botanical Literature of the State Library, John S. Wright; Microscope slides of vegetable material for use in Determinative work, John

S. Wright; Embryology of *Hydrastis canadensis*, Geo. W. Martin; Some determinative factors underlying Plant Variation, Geo. W. Martin; Variations in the cleavage of the *Fundulus* Egg, Geo. W. Martin; Hæmoglobin and its Derivatives, A. J. Bigney; Effects of heat upon the Irritability of Muscle, A. J. Bigney; The evolution of sex in *Cymatogaster*, C. H. Eigenmann; The circulation of protoplasm in the manubrium of *Chara fragilis*, D. W. Dennis; A new Subterranean Crustacean from Indiana, W. P. Hay; A peculiar crawfish from southern Indiana, W. P. Hay; A note on the breeding habits of the cave salamander, *Speterpes maculicaudus*, W. P. Hay; Notes on a collection of fishes from Dubois County, Indiana, W. J. Moenkhaus; The geographical variation of *Etheostoma nigrum* and *E. olmstedi*, W. J. Moenkhaus; A revision and synonomy of the *Parrus* group of *Unionidæ*, with 6 plates, R. Ellsworth Call; The fishes of the Missouri River Basin, B. W. Evermann and J. T. Scoville; Recent investigations concerning the Redfish (*Oncorhynchus nerka*) at its spawning grounds in Idaho, B. W. Evermann and J. T. Scoville; Additional notes on Indiana birds, A. W. Butler; A mammal new to Indiana, A. W. Butler; Some beneficial results from the use of Fungicides as a preventive of Corn Smut, Wm. Stuart; Ratio of alcohol to yeast in Fermentation, Katherine E. Golden; Distribution of *Orchidaceæ* in Indiana, Alida M. Cunnigham; A new station for *Pleodorina*, Severance Burrage; Additional notes on Animal Parasites collected in the State, A. W. Bitting; Report upon certain collections presented to State Biological Survey, Stanley Coulter; Infection by Bread, Katherine E. Golden; Certain plants as an index of Soil Character, Stanley Coulter; Forms of *Xanthium canadense* and *X. strumarium*, J. C. Arthur; A new habitat for *Gastrophilus*, A. W. Bitting; Noteworthy Indiana Phanerogams, Stanley Coulter.

The following reports relating to the State Biological Survey were made:

Second contribution to the knowledge of Indiana Mollusca, R. Ellsworth Call; Contributions to the Biological Survey of Wabash County, Albert B. Ulrey; Report of the Biological Survey, Zoölogy, C. H. Eigenmann.

Turkey Lake has been taken as a station for exhaustive study of a limit of environment and the variation of its inhabitants, and the following reports represent the first seasons work:

First Report of the Biological Station, C. H. Eigenmann; Some of the physical features of Turkey Lake, D. C. Ridgley; Hydrographic map of Turkey Lake, J. Juday; Temperatures of Turkey Lake, J. P.

Dolan; Inhabitants of Turkey Lake in general, C. H. Eigenmann; *Hirudinea* of Turkey Lake, Bessie C. Ridgley; *Rotifera* of Turkey Lake, D. C. Kellicott; *Clodocera* of Turkey Lake, E. S. Birge; *Mollusca* of Turkey Lake, R. Ellsworth Call; *Odonata* of Turkey Lake, D. C. Kellicott; Fishes and tailed batrachians of Turkey Lake, C. H. Eigenmann; Tailless batrachians of Turkey Lake, C. Atkinson; Snakes of Turkey Lake, H. G. Reddick; Turtles of Turkey Lake, C. H. Eigenmann; Water birds of Turkey Lake, N. M. Chamberlain; Flora of Turkey Lake, O. H. Meincke; Methods of determining Variations, C. H. Eigenmann; Variation of *Etheostoma* of Turkey and Tippecanoe Lakes, W. J. Moenkhaus.

The officers for the next year are as follows:

President, Stanley Coulter of Purdue University; Vice-President, Thos. C. Gray of Rose Polytechnic; Secretary, John S. Wright of Indianapolis; Assistant Secretary, A. J. Bigney of Moores Hill College; Treasurer, W. P. Shannon of Greensburg.

A. J. Bigney, *Assistant Secretary.*

The Biological Society of Washington.—November 30th, the following communications were read: Edw. L. Greene, Some Fundamentals of Nomenclature; Theo. Holm, Contributions to the flora of the District of Columbia; David White, The Mode of Development of Exogenous Structure in Paleozoic Lycopods, a review of Williamson and Renault.

SCIENTIFIC NEWS.

Notice Concerning the Geological Map of Europe, Published Under the Auspices of the International Congress of Geologists.—At the Third Session of the International Congress of Geologists, held in Berlin in 1885, the committee on a geological map of Europe made a report, in which the following conditions of publication were announced (Berlin Volume, page LXII): "The house of Reimer & Co., undertakes the publication at its own expense on the sole condition that the international committee guarantee the sale of 900 copies at 100 francs per copy, and furnishes the sum in advance.

The subscription price of 100 francs will be augmented to 125 francs in the regular book trade.

The committee has divided this guarantee subscription as follows: Each one of the large countries of Europe (to wit: Great Britain, France, Spain, Italy, Austro-Hungary, Germany, Scandinavia and Russia) agrees to take 100 copies. The six small countries (i. e., Belgium, Holland, Denmark, Switzerland, Portugal, Roumania) will divide among them the remaining 100 copies, etc."

In the Fourth Session of the Congress held in London in 1888, the following note occurs in the report of the proceedings of the committee on the geological map of Europe (London Volume, p. 59).

"The American committee requested of the Directory to be admitted as a subscriber to the map of Europe on the same terms as the great countries of Europe ('sic') i. e.. for at least one hundred copies and at the same price."

Dr. Frazer, the Secretary of the American Committee, obtained the names of American subscribers to the "one hundred copies at the same price" (100 francs), within a short time of the granting of this request, and promptly notified the publication committee in Berlin, Messrs. Beyrich and Hauchecorne, of the fact.

It appears, however, the map is being offered for sale in the German catalogues at the price mentioned in the Berlin resolution as that accorded to original subscribers.

On this account the undersigned advises the survivors of those who so patriotically came forward in 1888 to enable the geologists of the United States to enjoy same privileges as those of the great countries of Europe, to send through their own agents for the geological map of Europe, since there would no longer be any advantage in obtaining them through a single channel.

List of subscribers to the geological map of Europe in the order of their subscriptions, with number of copies:

Williams College, 1; Ohio State Univ., Columbus, 1; Rensselaer Polytechnic Institute, 1; University of Virginia, 1; Am. Inst. of Mining Engineers, 1; Amherst College, 1; Cornell University Library, 1; Provincial Museum, Halifax, 1; Wesleyan University, Middletown, Conn., 1; Lehigh University, Bethlehem, Pa., 1; Academy of Natural Sciences, Philadelphia, 1; Univ. of California, Berkely, Cal., 1; Prof. C. H. Hitchcock, for Dartmouth College, 1; Prof. J. S. Newberry (dead), 1; Indiana University, 1; Smith College, Northampton, Mass., 1; U. S. Geological Survey, Washington, D. C., 3; Rutgers College, New Brunswick, N. J., 1; Yale University Library, 1; American Geographical Society, 1; Peter Redpath Museum, McGill College, Montreal, 1; U. S. Military Academy, West Point, N. Y., 1; Prof. G.

A. König, 1; N. Y. State Library, Capitol, Albany, 2; Eckley B. Coxe, Drifton, Pa. (dead), 2; University of Nebraska, 1; Kansas State Library, 1; B. S. Lyman, 1; Johns Hopkins University, 1; F. W. Matthieson, La Salle, Ill., 1; Lehigh Valley R. R. Co., Philadelphia, 1; E. V. d'Invilliers, Philadelphia, 1; University of Wisconsin, Madison, Wis., 1; Second Geological Survey of Pennsylvania, 2; State Mining Bureau of California, 1; Washington University, 1; Dr. R. W. Raymond, 1; Franklin Institute, Phila., 1; Harvard College Library, 1; University of North Carolina, Chapel Hill, 1; University of the City of New York, 1; Massachusetts Agric. College, Amherst, 1; W. S. Keyes, San Francisco, Cal., 1; R. D. Baker, Philadelphia, 2; S. F. Emmons, U. S. Geological Survey, Washington, D. C., 1; H. M. Sims, Shenandoah, Page Co., Va., 1; American Museum of Natural History, N. Y., I; Prof. Alexander Winchell, Univ. of Mich., Ann Arbor (dead), 1; H. Huber, Argentine, Kansas, 1; Jas. E. Mills, E. Quincy, Cal., 1; Cooper Union, N. Y., 1; Collegiate and Polytechnic Institute, Brooklyn, 1; Cornell University, N. Y., 1; Joseph D. Potts, Philadelphia (dead), 1; Prof. J. C. Fales, Danville, Ky., Centre College, 1; T. H. Aldrich, Blocton, Ala., 1; Chas. Paine, Pittsburg, 1; Colorado School of Mines, Golden, Col., 1; Western Reserve Univ. (d. E. W. Morley), Cleveland, Ohio, 1; F. Klepotoko, Houghton, Michigan, 1; Thos. Macfarlane, Ottawa, Canada, 1; Arkansas Geological Survey, Little Rock, 1; Buchtel College, Akron, Ohio, 1; Mercantile Library, Philadelphia, 1; University of Michigan, Ann Arbor, 1; Alabama Geological Survey, University of Alabama, 1; E. S. Whelen, Philadelphia (dead), 1; Worcester Polytechnic Institute, 1; Julius Bien, N. Y., 1; W. A. Ingham, 1; Dr. Jas. P. Kimball, 109 East 15th St., N. Y. City, 1; Dr. J. S. Newberry, N. Y., Dec. 29, '87, 1; New Harmony Institution, Ind., 1; R. Ellsworth Call, Des Moines, Iowa, 1; Bost. Soc. Nat. Hist., 1; Hastings, Jno. B., Ketchum, Alturas Co., Idaho, 1; Geol. Surv. of Minn., Minneapolis, Minn., 1; Lacoe, R. D., Pittston, Luzerne Co., Penna., 1; Vassar College, Poughkeepsie, N. Y., 1; Mt. Holyoke Seminary, South Hadley, Mass., 1; Colby University, Waterville, Me., 1; Cincinnati Soc. of Nat. History, 1; Packer Collegiate Institution, Brooklyn, N. Y., 1; Emmens, Stephen H., Harrison, N, Y., 1; School of Mines, Rapid City, Dakota Territory. 1; Ohio University, Athens, O., Prof. A. D. Morrill, 1; Proctor, John R., Franklin, Ky., Aug. 19, '88, 1; Rose Polytechnic School, Terre Haute, Ind., Aug. 19, '88, 1; Read, Jas. P.. Calico, San Bernard Co., Aug. 31, '88, 1; Oberlin College, Ohio, Aug. 23, '88, 1; Frazer, Persifor, Philadelphia, 1; Streator Township High School, La Salle Co., Ill., R. Wil-

liam Brice, Sept. 21, '88, 1; State Univ., Athens, Ga., Prof. J. W. Spencer, Nov. 12, 1888, 1; Lowry, Thos., Minneapolis, Minn., Nov. 13, 1888 (N. H. Winchell), Nov. 13, 1.—Total, 100.

Dr. Eugenio Dugès died in Morelia, Mex., Jany. 13th. 1895. He was born in Montpellier, France, but had resided in Mexico since 1865. He was a special student of Coleoptera, and had furnished students in the United States with many specimens.

Dr. Adolf Gerstäcker, Professor of Zoology in the University of Griefswald, died June 20th, 1895. He was born Aug. 30th, 1828, and is widest known from his share in the Zoologie of Carus and Gerstäcker and his contributions to Bronns' Thierleben.

Dr. Th. Ebert has been called as Professor of Paleontology to the Prussian Geological Institute, and Dr. Müller as Professor of Regional Geology in the same institute.

Henry John Carter, well known for his researches on Protozoa, Sponges, etc., died at Rudleigh Salterton, England, May 4th, 1895.

Dr. Wm. H. Flower, of the British Museum, has been elected corresponding member for anatomy of the Paris Academy of Sciences.

Dr. W. I. Nickerson, of the University of Colorado, has been appointed Instructor in Biology in the University of Evanston, Ill.

Prof. A. Sabatier, of Montpellier, has been elected corresponding member for Zoology of the Paris Academy of Sciences.

Dr. F. Schütt, of Kiel, has been appointed Professor of Botany and Director of the Botanical Gardens at Griefswald.

Dr. Joseph G. Norwood, the well-known geologist and paleontologist, died at Columbia, Mo., May 6th, 1895.

Dr. E. Hering, of Prague, becomes Professor of Physiology at Leipzig, as successor to the late Prof. Ludwig.

Dr. René duBois Raymond is assistant in the experimental division of the Physiological Institute in Berlin.

Dr. W. A. Setchell, of Yale College, has been appointed Professor of Botany in the University of California.

Dr. F. Sansoni, Professor of Mineralogy in Pavia, and editor of the Italian Journal of Mineralogy, is dead.

Mr. Charles D. Aldright has been appointed Instructor in Biology at the University of Cincinnati.

C. C. Babington, Professor of Botany in the University of Cambridge, died July 22d, aged 86.

James Mortimer Adye, an entomologist, died at Bournemouth, England, May 30th, 1895, aged 34.

Dr. Jas. E. Humphrey has been appointed Lecturer in Botany in Johns Hopkins University.

Dr. A. Kowalevsky has been elected a foreign associate of the Academy of Sciences of Paris.

Prof. J. G. Agardh has given his magnificent collection of Algæ to the University of Lund.

Pietro Doderlein, Professor of Zoology and Geology in Palermo, died March 28, aged 84.

Dr. Pellegrino Strobel, geologist and Conchologist, died at Parma, Italy, June 9th, 1895.

Dr. R. Hanitsch has gone as Director to the Raffles Museum and Library at Singapore.

The Linnean Society of London has awarded a gold medal to Prof. F. Cohn, of Breslau.

Dr. Gustav von Nordenskiold, ethnographer and crystallographer, of Stockholm, is dead.

Dr. A. D. Mead has been appointed Instructor in Neurology in Brown University.

Dr. Reinitzer, of Prag, has been called as Extraordinarius Professor of Botany to Graz.

Dr. E. Schöbl succeeded Dr. Schiemenz as Librarian of the Naples Zoological Station.

Prof. E. D. Cope has been elected associate member of the Academy of Sciences, Arts and Letters of Belgium.

Dr. R. Bonnet, of Giessen, goes as Professor Ordinarius of Anatomy to Griefswald.

Dr. W. Roux, of Innspruck, has gone as Ordinary Professor of Anatomy to Halle.

Dr. Hans Schinz is appointed Ordinary Professor of Botany in Zürich.

Julien Deby, of London, microscopist and student of diatoms, is dead.

Dr. F. C. Kenyon has gone to Clark University as Fellow in Biology.

Dr. H. Lenk, of Leipzig, has made Extraordinarius of Geology.

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AMERICAN NATURALIST

VOL. XXX. March, 1896. 351

THE HISTORY AND PRINCIPLES OF GEOLOGY, AND ITS AIM.

BY J. C. HARTZELL, JR., M. S.

From the earliest times the structure of the earth has been an object of interest to man, not merely on account of the useful materials he obtained from its rocky formation, but also for the curiosity awakened by strange objects it presented to his notice. The south and west of Asia, and much of the country bordering the Mediterranean, were particularly favorable for directing attention to geological phenomena. Earthquakes were of frequent occurrence, changing the relative positions of sea and land. Volcanoes were seen in eruption, adding layers of molten rock to those of sand and mud filled with the shells of the ocean. The strata in the hills abounded in evidences of similar collections of vegetable and marine life far removed from access of the sea.

The structure of the earth, however, received but little attention previous to the 7th century, B. C. The extent of the surface known was limited, and the changes upon it were not so rapid as to excite special attention. The ancient Hebrews, in the time of Solomon (1015 B. C.), prosecuted their voyages

through the Straits of Babelmandeb into the Indian Ocean, bringing home the produce of the tropical regions; while the ships sent westward to the Atlantic returned with tin, silver, lead and other metalic products of Spain and Great Britian.

The earliest idea formed of the earth seems to have been that it was a flat circular disk, surrounded on all sides by water, and covered with the heavens as with a canopy, even philosophers looked upon the earth as a disk swimming upon the water. Homer (800 B. C.) regarded the earth as a flat circle surrounded by mysterious waters. The nations that were upon its border were called Cimmerians, and were supposed to live in perpetual darkness.

As the ancients slowly gained a knowledge of the country surrounding their provinces through commercial intercourse, wars, and the search for knowledge, they were undoubtedly struck with the differences of the topography and formations. Thus geology is undoubtedly the outgrowth of geographical knowledge.

The 7th and 6th centuries B. C. were remarkable for great advance in the knowledge of the form and extent of the earth.

Their first discoveries were probably made by the Phoenicians. Their investigations were along the shores of the Mediterranean, and passing through the Straits of Gibraltar, they extended their researches into Spain and Africa and the Canaries.

Pythagoras (583 B. C.) observed the phenomena that were then attending the surface of the earth, and proposed theories for explaining the changes that had taken place in geological time. He held that in addition to volcanic action, the changes in the level of the sea and land were due to the retiring of the sea.

Aristotle (384 B. C.) recognized the interchange constantly taking place between land and sea by the action of running water and of earthquakes, and remarked "how little man can perceive in the short space of his life of operations extending through eternity of time."

Geographical knowledge was greatly advanced by the conquest of Alexander the Great ([illegible] B. C.) in making known

Persia, and science was advanced by sending out expeditions to explore and survey the various provinces he had conquered. The Greeks he sent out, and also those who accompanied him, were critical observers and carefully described the products and aspects of the country, and made collections of all that was interesting in regard to the organic and inorganic products.

Ptolemy (323 B. C.) discovered Abyssinia and navigated the Arabian Sea, and Silineus (306 B. C.) ascended the Ganges to Batna and extended his expedition to the Indus.

It was the military genius of the Romans which led to the survey of nearly all Europe, and large tracts of Asia and Africa. In the height of their power they had surveyed and explored all the coast of the Mediterranean, Italy, the Balkan peninsula, Spain, Gaul, West Germany and Britain, and their practical genius led them to the study of the natural resources of every province and state brought under their sway.

Eratosthenes (276 B. C.) considered the world to be a sphere revolving with its surrounding atmosphere on one and the same axis and having one center. His theories were perfected by Hipparchas (160 B. C.). He attempted to catalogue the stars and to fix their relative position, and he applied to the determining of every point on the surface the same rule he introduced in the arrangement of the constellation.

Strabo (60 B. C.) noticed the rise and fall of the tide, and maintained that the *land* changed its level and not the *sea*, and that such changes happened more easily to the land beneath the sea on account of its humidity.

Ptolemy (150 A. D.) was the first scientific geographer. He followed the principles of Hipparchas, which had been neglected during the two centuries and a half since his time, even by Strabo and Pliny. In Ptolemy's work is found for the first time the mathematical principle of the construction of maps, as well as several projections of the earth's surface.

After the great achievements of Ptolemy to the 13th century, the cultivation of the physical sciences was neglected. In the 10th century Avicenna, Almar, and other Arabian writers commented on the works of the Romans, but added little of their own.

From the 13th to the 16th century, astronomy, travels, and commercial interests occupied the attention of the different nations, but geology did not appear as a separate science until in Italy in the 16th century. It began by being a record of observed facts. This was not enough, however, for it did not satisfy the demand as to how the phenomena were produced. High above sea level, and far inland, imbedded in solid rock, were found fossils. At the outset it was unfortunately linked to the belief that they were relics of the Noachean deluge. Some held that they were the result of the formation of a fatty matter, or of terrestrial exhalations or of the influence of the heavenly bodies, or that they were merely concreations, or sports of nature. The abundance of fossils in the strata of the Apennine range could not fail to arrest attention and excite inquiries. Leonardo da Vinci (1519) and Fracostaro, whose attention was engaged by the multitude of curious petrifactions which were brought to light in 1517 on the mountains of Verona in quarrying rock for repairing the city, had sound views, and showed the inadequacy of the terrestrial deluge to collect marine fossils.

Collections were made for museums, that of Canceolarius, at Verona being the most famous. Descriptive catalogues of these collections were published.

Only a few held that they were the remains of animals. Palissy in 1580, was the first who dared to assert in Paris that fossil remains once belonged to marine animals. The question was naturally asked "How came they there?" The result of investigation showed that the rocks must have accumulated around them, and hence could not always have been as they were found and that the arrangement must have changed since they were formed. This brought about the study of the construction of the earth.

Their chief objects were the examination of the materials out of which the solid framework of the earth was built, and the determination of their chemical composition, physical properties, manner of occurrence, and their characteristics. Thus they started out with the idea that rocks were made through secondary causes.

Steno (1669) observed a succession in the strata, and proposed the theory that there were rocks older than the fossiliferous strata in which organic remains occur. He also distinguished between marine and fluvialite formations. He also published his work " De solido intra solidum naturalites contento," in which he proves the identity of the fossil teeth found in Tuscany with those of living sharks.

Scilla, in 1670, published a treatise on the fossils of Calabria, and maintained the organic nature of fossil shells. But both Steno and Scilla referred their occurrence to the Noachean deluge.

In England the diluvialists were busy forming idle theories to give plausibility to their creed, that the Noachean deluge was the cause of all the past changes on the earth's surface. Differing somewhat in detail, they all agreed in the notion of an interior abyss whence the waters rushed, breaking up and bursting through the crust of the earth, to cover the surface, and whither, after the deluge, they returned. Such absurd notions greatly hindered the advance of science.

Leibnitz (1680) proposed the bold theory that the earth was originally in a molten state from heat, and that the primary rocks were formed by the cooling of the surface, which also produced the primeval ocean by condensing the surrounding vapors. The sedimentary strata, he held, resulted from the subsiding of the waters that had been put in motion from the collapse of the crust on the cooling and contracting nucleus.

Burnet (1680) published his " Sacred Theory of the Earth," and it received great applause. It was written in ignorance of the facts of the earth's structure, and was an ingenious speculation. It abounds in sublime and poetical conceptions in language of extraordinary eloquence. In 1692 he published a work which treated of the Mosaic Fall as an allegory.

Lister sent to the Royal Society, in 1683, a proposal for maps of salts and minerals. He was the first to recognize the arrangement of the earth's materials in strata, continuous over large areas, and resembling each other in different countries.

Hooke (1688) and Ray (1690), differing as much from Burnet as from Leibnitz, considered the essential condition of the

globe to be one of change, and that the forces now in action would, if allowed sufficient time, produce changes as great as those of geological time. Hooke published a "Discourse on Earthquakes," which contains the most philosophical view of the time respecting the notions of fossils and the effect of earthquakes in raising up the bed of the sea. Woodward perceived that the lines of outcrops of the strata were parallel with the ranges of mountains. He formed, about the year 1695, a collection of specimens which he systematically arranged and gave to the University of Cambridge.

They were followed in the same direction by Vallismeme (1720), Moro (1740,) Buffon (1749), Lehman (1756), and Fuchsel (1773), each contributing something additional, and advanced the most philosophical views yet presented respecting the fossiliferous strata. The first two made observations throughout Italy and the Alps. Moro endeavored to make the production of strata correspond in time with the account of the creation of the world in six days.

Buffon published his "Natural History," in which he advanced views respecting the formation and modification of mountains and valleys by the action of water.

Geology did not begin to assume the rank of an important science until its application to the practical purposes of mining and agriculture was first pointed out in 1780 by Werner, Prof. of Mineralogy in the School of Mines at Freiberg in Saxony. He greatly advanced the science by establishing the superposition of certain groups, by giving a system and names. He had very crude ideas regarding the origin of the strata. He supposed that the various formations were precipitated over the earth in succession from a chaotic fluid; even the igneous rocks he held to be chemical precipitations from the waters.

Thus we see that the history of geology has been a record of failures, and it was not until Hutton (1788), rejecting all theories as to the beginning of the world returned to the opinions of Pythagoras and Ray. He pointed out that geologists must study the *present* if they would learn of the *past*; and he labored to show that the forces now in operation are capable

of forming rocks and of bringing about the changes that have occurred on the earth. He held that the strata which now compose the continents were once beneath the sea, and were formed out of the waste of preëxisting continents by the action of the same forces which are now destroying even the hardest rocks. Hutton was the kind of man the science had so long been in need of, and by his teaching geologists were at last started on the only path that could possibly lead them to truth. He drove out at once and forever the imaginary agencies which the early geologists had been so ready to have recourse to, and laid down the principle that in geological speculation "no powers are to be employed that are not natural to the globe, no actions to be admitted of except those of which we know the principle, and no extraordinary events to be alleged in order to explain a common appearance." He occupied himself mainly studying the changes that are now taking place on the earth's surface, and the means by which they were brought about, and in demonstrating the fact that the changes that had happened during the past periods of the earth's history were of the same kind and due to the same causes as those now going on.

The determination of the order of the strata, and the grouping of them in chronological order, were begun by Lehman (1756) and carried on by Fuchsel (1773), Pallas (1785) and Werner (1789). Smith made the most important contribution to this subject when, in 1790, he published his *Tabular View of the British Strata.* He showed their superposition and characterized the different groups by their peculiar fossils.

(*To be continued.*)

THE CONSTANCY OF BACTERIAL SPECIES IN NORMAL FORE MILK.[1]

BY H. L. BOLLEY.

It is recognized that aside from actual dirt, as, for example, drippings from the hands of the milker, dirt from his clothing, and hairs and manurial particles from the sides of the animal, that the fore milk constitutes the most productive source of the bacterial flora of milk. Schultz and others have placed quantitative determinations at from fifty to one hundred thousand per cubic centimeter. As the character of the germ content is becoming such a matter of importance in economic labors with milk and its product, it is apparent that a consideration of the types of germ present in the normal udder should command early attention of the bacteriologically inclined dairymen.

The question is of necessity, one of such breadth that it must be approached in separate phases, such, as for example, the study of the presence or absence of physiological groups, constancy of definite species, etc. During the year just closed two such points have been under investigation. The primary object, while being a matter of simple interest, had also the direct aim of determining the relation of normal fore milk to curd inflation in cheese manufactory. The results of the work have in part been reported in a paper read before the General Section of the American Association of Agricultural Colleges and Experimental Stations, July 19, 1895; showing that, in so far as the investigation had been carried, gas generating species such as are accountable for "pinhole formation" or curd inflation are not normal to the fore milk of the healthy udder.[2]

[1] Read before the Section of Botany of the American Association for the Advancement of Science, Springfield Meeting, August 31, 1895. Also published in Centralblatt für Bacteriologie und Parasitenkunde, Ab. II, B and I, No. 22–23.

[2] Bolley and Hall: Cheese curd inflation: Its relation to the bacterial flora of fore milk. Centralb. f. Bact. u. Parasiteuk., II, Ab. I, Bd., No. 22–23.

This conclusion was based upon preliminary cheese curd tests made at Madison, Wisconsin, August, 1894, and duplicated at Fargo in October, and finally upon qualitative analysis made during a period of three winter months, with ten different milch cows under consideration.

The point to be reported upon, at this time, is that of the constancy of species as found: (a) for the same cow for a given length of time; (b) in the same teat of the same cow; and (c) as to whether species are common to different cows or not upon same dates.

In general, the evidence of the work associated with the last named report, was to the effect that there is no evidence that germs are of any certainty common to different animals upon the same date under like conditions; but that a certain inhabitant of the udder of the same animal may remain quite constant. Thus while only one species, number 30, was observed to be present in more than two animals of the original ten animal test upon different dates, several different species were found to occur at several dates in the same udder.

Commencing July 1st, three animals were placed under cultural investigation, number 24 of which was an animal of the original ten, also number 21. Cultures were attempted from each teat upon gelatine and agar, as often as the work could be handled, the same methods of procuring milk being used as in the previous work, except in the different tests of the same animal, the milk tube or trochar, was inserted different depths. Some sixty of these distinct milkings were taken upon fifteen different dates, during which time the cows ran upon a clean pasture during the day, being housed at night. The milk samples were taken sometimes in the morning and sometimes at night. In all, thirty-seven different species of bacteria were separated; and, as in past work, were found to be of various physiological types, gelatine liquifiers, non-liquifiers, solid curd types, peptonizing forms, acid and alkali curdlers, etc., including bacilli, micrococci of various forms, and a streptococcus. Thus it may be said that, in general, forms collected are miscellaneous.

Results: Again, there is no marked evidence that species are common among different animals, but there is strong evidence of constancy of appearance of certain types when once present. This, perhaps, is to be expected, for it is hardly possible that in an ordinary milking all individuals could be excluded from the milk cistern and lower teat passages.

The following table and annotations may help to show the bearings of the work:

Cow No. 24	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
*Expr. No. 1, July 2nd.	Nos. 1	Nos. 1	Nos. 1	Nos. 5
Expr. No. 2, July 3rd.	6	1	9 and 10	5, 100 & 77
Expr. No. 3, July 4th.	16	1	15	(Not taken)
Expr. No. 4, July 6th.	(Not taken)	17 and 1	20	20
Expr. No. 5, July 8th.	(Lost Cul.)	23	10, 61	26, 27, 15, 29
Expr. No. 6, July 10th.	30, 1	(Lost)	31	(Not taken)
Expr. No. 10, July 17th	58, 53, 1	1	61	66, 20, 15, 1
Expr. No. 13, July 23rd.	96, 93, 94	1	96, 97	20, 11, 100, 1
Expr. No. 15, July 28th.	77, 67	(Not taken)	66, 100 & 67	67, 1

*The numbers in each columns—1, 2, 3 and 4 = the laboratory numbers given the different species.

Annotation No. 1, a solid curd, lactic acid forming micrococcus, is seen to be present upon every date, appearing in teat No. 2 upon all possible dates save one.

Nos. 5, 10, 15, 61 and 67 occurred twice each, the intervening days being respectively 2, 8, 7, 4 and 4. It is worthy of note that with the exception of No. 67, each of these was found each time in the same teat.

Cow No. 21	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 8, July 12th.	Nos. 45	Nos. 31	Nos. 27, 31	Nos. 20
Expr. No. 9, July, 15th.	(Lost)	31, 50	29, 53	55, 56, 57, & 31
Expr. No. 12, July 16th.	53, 51, & 56	31, 45	(Not taken)	(Lost)

Annotations:—With this animal it is to be noted that No. 31, a lactic acid forming micrococcus, is constant to all dates, and upon each date was found present in teat No. 2.

Other germs found twice each were Nos. 45, 53 and 56; but each time in a different teat.

Cow No. 26	Species present, per teat, by dates.			
Teats =	No. 1	No. 2	No. 3	No. 4
Expr. No. 7, July 1st.	33, 1	33	39, 61, 67	17, 44, 33
Expr. No. 17, July 17th.	66, 100, 67 17 and 33	33, 15	83	53, 77
Expr. No. 14, July 23rd.	33	67, 83	33	(Lost)

In these three milkings from cow No. 26, the common species to each date are seen to be Nos. 33 and 67. Out of eleven milk samples taken No. 33 occurs in the cultures nine times. The intervening dates being 16, 6 and 22 days apart. No. 33 is a streptococcus and in these distant tests, as to time separation, is a strong argument of constancy of presence being possible to an individual species. In growth characteristics this germ is almost a strict anaerobe.

Studying these tables, we find for each animal the following numbered germs present:

Cow No. 24.—Nos. 1, 5, 6, 9, 10, 100, 77, 15, 16, 17, 20, 23, 61, 26, 27, 29, 30, 31, 58, 53, 66, 96, 93, 94, 97, 11 and 67, a total of twenty-seven distinct forms.

Cow No. 21.—Nos. 45, 31, 27, 20, 50, 29, 53, 55, 56, 57 and 51, a total of eleven.

Cow No. 26.—Nos. 33, 1, 39, 61, 67, 17, 44, 66, 100, 15, 53 and 77, a total of twelve.

The forms common to three animals equal only one, No. 53, while those common to two of them are seen to be Nos. 1, 100, 77, 15, 61, 29, 31, 66, 67, 29 and 20; eleven constant forms.

General Annotations:—From these summaries it is to be noted that cow No. 24 from nine different milkings furnished twenty-seven of the thirty seven germs of the three tests, *cow*

No. 21 six and cow No. 26 four. The numbered germs from the last named animals are representative of but three milking dates each. It is thus a possibility, that further milking dates for these cows might have given others of those common to cow No. 24. While this point last named, is probably a correct consideration, it is nevertheless quite clearly indicated that the great majority of germs are but incidental in a given udder or teat to the date, perhaps, to the environments of the animal. There are, however, certain few germs found which when once present in a teat or udder, remain with marked presistence. For this capability, these are found to possess what are presumably the proper physiological functions or requirements, as for example, capability to properly thrive in or withstand the normal temperature of the animal's body, and anaerobic or semi-anaerobic faculties.

As in the case of the paper previously mentioned, this is given not as final evidence to convince upon the points mentioned or suggested, but rather as a record of preliminary work accomplished.

Again, an interesting fact is the comparatively low number of species per milk sample. In the first work, winter collections, the range was from one to four species, in this it is one to five with a rather high average number. It is also interesting, though perhaps to be expected, that quantitative determinations vary from low to high numbers for different milkings, very much in accord to these last named figures.

North Dakota Experiment Station, Fargo, N. D., August, 20, 1895.

LIFE BEFORE FOSSILS.

By Charles Morris.

The beginning of life upon the earth is one of those mysteries which, to judge from what we now know about it, seems likely never to be solved by ascertained facts. There are mod-

ern facts, indeed, which bear upon it, but few geological ones, and none of absolute force. If we leave out of the question the highly problematical "Eozoon Canadense," we find the first known fossils at a comparatively high level in the rocks; and these, instead of being, as the theory of evolution requires, of very simple organization, are of a degree of development which indicates a very long period of preceding life existence. This primeval fauna, indeed, contains representatives of every branch of animal life except the vertebrate, and these not in their simplest stage, but already divided into their principal orders: the Cœlenterate class, for instance, yielding examples of Actinozoa and Hydrozoa; the Crustacean, of Trilobites and Phyllopods; and the Molluscan, of Gasteropods, Lamellibranchs and Pteropods.

This is the beginning of life as we know it. It is very far from the beginning of life as evolution demands, or as the character of the rock strata indicates. Below the Lower Cambrian beds, which contain these fossils, lie several miles of stratified rocks similar in physical character to those above them, and indicating, as Darwin says, "that during a preceding era as long as, or probably far longer than, the whole interval from the Cambrian age to the present day . . . . the world swarmed with living creatures."

Evidently we are not yet at the origin of life. We are miles away from it probably—miles of rock strata, that is. Between the simplest known microscopic creatures and the much developed Cambrian fossils an immense gap extends. The gap, for example, between a diatom and an oyster is one that represents ages of evolution; yet it is much less in extent than the yawning gap which we find dividing the line of primeval life, and which geologists have sought in vain to fill. Believers in evolution—who represent about all living scientists and the bulk of living thinkers—cannot but stand in some dismay before this strange circumstance, which must be proved away or explained away before their theory can be fully substantiated. Yet proof is not forthcoming, and only attempts at explanation remain.

In April, 1885, I presented certain views on this subject before the Academy of Natural Sciences of Philadelphia, and reinforced my arguments by later communications in 1885 and 1886. In 1894 Professor W. K. Brooks, evidently unaware of the existence of the papers mentioned, advanced a similar hypothesis in the July–August number of the "Journal of Geology," presenting a number of interesting facts, though missing, as it seems to me, much the strongest argument in defence of the hypothesis.

I propose here to repeat my former hypothesis, with additional arguments and illustrations—for some of the latter of which I acknowledge indebtedness to Professor Brooks's able paper.

To begin with, the facts of embryology may be said to point directly to what was probably the primary condition of life. The embryos of ocean animals, as a rule, begin life as swimming forms. Even the oyster—a type of sluggishness in animals—enjoys a brief existence as a swimmer before it acquires a shell and becomes permanently fixed. The same is the case with the sponge, the coral, and other stationary types, and with the various creeping or slow moving forms, such as the echinoderms. Since it has become a settled dogma of science that each stage of development passed through by the embryo represents some mature stage in the ancient ancestry of the animal, the fact stated points almost irresistably to the conclusion that the far off ancestors of the present stationary or crawling animals were swimmers—and, for that matter, naked swimmers, they being as yet destitute of hard skeletal parts.

Yet no swimming stage of existence is indicated by the oldest known fossils, or at least only by the minute pteropods and phyllopods, which were, perhaps, secondary derivatives from crawling ancestors. The trilobite may have had some swimming powers, yet probably made its way only by crawling, and the other known forms were crawlers or burrowers, or were immovably fixed. There are traces of jelly fish, it is true, but these, as they now exist, we know to be derivatives from stationary forms, and the primeval swimmers indicated by embryology have left no trace of their existence in the rocks.

Yet the oceanic waters to-day swarm with swimming life, and in all probability did so then. This life, as now existing, contains many high as well as numerous low forms. Then it must have consisted of low forms only. The wealth of existing minor sea life, as observed by the unassisted eye and revealed by the microscope, is simply boundless. Small jelly fish are met with in vast armies, hundreds of miles in extent, and descending to many feet in depth. Pteropods, both the naked and the shelled forms, occur in prodigious multitudes. The minute copepod crustaceans are found in countless swarms, and, though consumed in myriads daily by herring and other fish, by medusæ, siphonophora and other invertebrates, and even by the whale, they are so productive that their numbers seem undiminished, being found over vast areas of surface and extending through more than a mile in vertical depth. Below these again are hosts of microscopic larvæ and minute animals, and still lower are countless swarms of protozoa, such as radiolarians, globigerinæ, etc.

Here, then, are innumerable swarms of swimming and floating forms, in most part carnivorous, but necessarily requiring a vegetable basis of nutriment. The foundation food supply for such a mighty host must be enormous in quantity. The visible plant life of the ocean, the algæ which grow on the bottom, would not sustain a tithe of such an army. The microscope must again be brought into requisition, and this useful instrument reveals to us an extraordinary profusion of unicellular plants—diatoms, coccospheres, trichodesmiums, and a few other types—which extend from the surface to the lowest level of light penetration, and are so extraordinarily numerous and prolific as to supply food for all the oceanic host. These, and the protozoa which feed upon them, form the basic food supply for the countless myriads of living forms which compose the fauna of modern seas.

Yet, were the conditions of the ocean as they exist to-day to be sought for by some far future geologic delver into the mysteries of the rocks, almost nothing of this profusion of life would be revealed, discovery being nearly or entirely confined to such forms as possess hard skeletons, internal or external, of

which most of these forms are destitute. The same was probably the case with the period which we now have under review, and of whose life we find few forms except those which habitually dwelt upon the bottom. The ocean may have been as full of life then as it is to-day, many of the swimmers of that period, perhaps, representing the ancestral lines out of which the bottom dwellers had evolved, and which are still in a measure preserved for us in modern embryos. These primeval forms may have been even less suitable for fossilization than their counterparts of to-day. The diatoms, the radiolarians, and other minute existing forms have silicious shells capable of preservation. It is quite possible that the early protozoa and protophytes had no such skeletal parts, and that when they died all trace of them departed.

How far back, then, from the earliest age of fossils must we place the actual date of the origin of life? Ages perhaps—epochs—a period as remote from the Cambrian in one direction as we are in the opposite. It may have taken as long, or longer, to develop the trilobite as it since has taken to develop man. During the whole of the immensely long period in which the miles of earlier strata were being deposited, the ocean may have been the seat of an abundant life of the lowest type, and this a very slowly evolving one, the conditions being such that competition and the struggle for existence were not strongly active.

Of the forms of life now existing, the most abundant and the lowest in organization known to us are the bacteria or microbes—omnivorous life specks, feeding alike on animals and plants, and fairly assignable to neither. Possibly life had its origin in forms like these, or in still lower stages of protoplasmic activity, and from this condition developed, after an interminable period, into the simple oceanic protozoa and protophytes typified by the radiolorians and the diatoms, the lowest forms having characters common to both animals and plants, while their descendants divided definitely into plants and animals.

The period here referred to, and that subsequently consumed in the development of the trilobite and its companion forms,

must have been of very great duration; for the conditions were such as to make evolution a slow process. The habitat of these primeval life forms, the oceanic waters, was of the greatest uniformity, even probably in temperature, and possessed no condition likely to provoke rapid variation. There was abundant space and probably abundant food, particularly in view of the minuteness and slight nutritive demands of these early animals, and the struggle for existence could not have been active. Though there were millions devoured hourly, there were trillions provided for the feast, so that no great tendency towards the preservation of favorable variations would have existed.

Yet, though the influences which favor evolution were not very actively present, they could not have been quite absent. The innate tendency to vary which all living forms possess now must have existed then, and the advantage possessed by the more highly over the more lowly organized forms could not have been quite wanting. Consequently, development of varying life forms must have gone on at some rate, and animals must in time have appeared much higher in organization than the simple forms from which they emerged.

And the variations which took place were radical in character. Variation in the higher recent types of life does not penetrate deeply. After ages of change a vertebrate is a vertebrate still. Millions of years of change do not convert a cat into something radically distinct from a cat. But in the primitive period the changes were more profound. Variation went down to the foundation plan of those simple forms and converted them at once into something else. A degree of variation which now would modify the form of a fish's fin may then have converted a monad into a new type of animal. Thus primitive evolution, working on forms destitute of any definite organization, may readily have brought into existence a number of highly different types of life. As the microbe, for instance, may through long variation have given rise to the two organic kingdoms of animals and plants, so the amœba or other low animal form may have varied into the subkingdoms of mollusca, echinodermata, cœlenterata, etc., or rather into simple swimming forms

each of which was the progenitor of one of these great branches of the tree of life.

We are here in a realm of the unknown, through which we are forced to make our way slowly and uncertainly by aid of the clues of embryology, microscopic life conditions, principles of variation and development, and the known conditions of pelagic life. We can only surmise that, as the result of a long era of evolution, the simple primary forms gave rise to a considerable variety of diverse animals, still comparatively minute in size and simple in organization, swimming by means of cilia, and typified to-day by the swimming embryos of invertebrate animals.

As yet—if our hypothesis is well founded—no life existed upon the bottom of the seas, and the swimming forms were destitute of any hard parts capable of fossilization. But why did not some of these forms very early make their way to the bottom and begin life under the new conditions of contact with solid substance? And yet why should they have sought the bottom? Their food supply lay on or near the surface, the bottom of the shallow waters may have been unsuitable through the deposition of soft sediment, and the bottom of the deeper waters very sparse in food. And, more important still, they were quite unadapted to life on the bottom, and needed a radical transformation before they could survive under such conditions. If we look at the remarkable change which the swimming embryo of a star-fish or sea-urchin, for example, goes through before any resemblance to the mature form appears, we may gain some idea of the long series of variations which the primitive ciliated swimmers must have passed through to convert them into crawling or stationary bottom-dwelling forms. Great as was the period needed to produce these type forms of life, another extended period must have been necessary to convert them into well adapted habitants of the solid floor of the seas.

(*To be Continued.*)

BIRDS OF NEW GUINEA (FLY CATCHERS AND OTHERS).

BY G. S. MEAD.

Among the many kinds of Flycatchers (*Muscicapidæ*) inhabiting the Papuan Islands, while there is dissimilarity in so large a number of species, yet there are not those striking differences amounting almost to contrasts which characterize birds of greater size. Many species have been unnoticed by travellers and other writers; many exist only in cabinets and collections, labelled and ticketed, or at most given a few lines of technical summarization in catalogues. With the rank and file of birds anything more than this is impossible. Sometimes a particularly attractive specimen of *Malurus* or *Rhipidura* or *Pratincola* calls attention to itself, or mere accident brings an individual to the notice of the explorer or student.

Thus Mr. Wallace notes pointedly "the abnormal red and black flycatcher," *Peltops blainvillii*, so named by Lesson and Garnot many years since. It is a sprightly, highly colored bird with the predominant hues strongly contrasted and still further accentuated by spots of white on the head and beneath the wings. In flight this active little flycatcher presents in turn these conspicuous markings with striking effect. The red tint is a bright crimson spread over the lower back and tail coverts. The main color is a steely-green black covering with greater or less intensity the seven inches of total length. The genus is represented by this species only.

The same notable expedition to South Eastern New Guinea that secured the two beautiful prizes *Cnemophilus macgregorii* and *Amblyornis musgravianus*, discovered also a new species of flycatcher, viz., *Rhipidura auricularis*. It is described as having the "upper surface smoky gray; head brownish black; tail the same above and below; bill dark brown; legs black." The head is marked by black and white stripes, found upon the wings as well. Upon the chin, throat and breast similar

lines of unequal width are plainly drawn. The under parts are in general buff varied with black and gray. Dots and bars of white appear on the wings and tail. Its total length is about six inches.

Rhipidura leucothorax, the Whitebreasted Fantailed Flycatcher, is much more widely distributed, being met with in different parts of New Guinea. The descriptive name here describes very imperfectly, for the breast is by no means entirely white as might be inferred; black is almost as prominent, alternating with the white which shows in spaces, though lower down it crowds the black into narrow bands or crescents. The general color of the bird above is brown, becoming dark upon the head, still darker over the bill. The wings are black, finished off with white spots. This is the appearance too of the tail feathers as well as of the under side of the wings. There are also white streaks and lines about the sides of the head and throat. Bill black above. Length 8 inches.

The family of Wood Songsters (*Pœcilodryas*) are all small birds rarely exceeding 6.5 inches in total length. The coloration is in general black and white, the former greatly predominating. *Pœcilodryas albinotata* at first sight looks in color not unlike those fine drongos, the Edolias. In this instance, however, leaving the disparity of size out of account, the gray is not nearly so uniform, a dull black and a deep black appearing on the wings, tail and throat including the side face. A patch of white meets the black on the sides of the neck. White again is seen on the abdomen and under tail coverts, becoming discolored along the flanks and sides of the body.

Pœcilodryas papuana comes from the same region of the Arfak Mountains as the foregoing species. It is considerably smaller in size measuring only 4–5 inches inches in length, but of brighter color. This is a yellow, somewhat dull an becoming light brown on the wings and tail. Head and neck are darker than the body. A crescent of orange runs from the bill over the eye.

Pœcilodryas leucops shares the same habitat. It is not unlike the preceding in coloration of the body but that of the head, nape and throat is entirely different. In this case it is

a dark gray, to gray on the neck with darker feathers over the eye. White marks the upper throat and chin and appears as a prominent spot in front of the eye. Total length nearly five inches.

From the Arfak Mountains also comes *Pœcilodryas bimaculata* and from the same general region *Pœcilodryas hypoleuca* and *P. brachyura* and *P. cinerea.* The first is conspicuously black and white, the former color preponderating very largely of course, while the white shows as bands and bars or stripes. It is most apparent on the lower parts where it may be reckoned as the ground color.

P. hypoleuca, the Whitebellied, is a rather larger bird, reaching the length of 6 inches. The general color is dusky above, relieved by white patches on the head. The same color covers the under parts set off by black on breast and throat. The last named—*P. brachyura*, the Shorttailed—is marked similarly with the tones rather deeper and clearer. Length 5.5 inches.

Monachella mulleriana or *saxicolina*, a Chatlike Flycatcher, is a lively little bird found as well in the south of New Guinea along the Fly River, as in the north among the Arfak Mountains. It is of grayish plumage above becoming nearly white on the rump and tail coverts; tail feathers and wings are dark brown. The head is also dark brown with a line of white over the eye. A spot of black lies near the bill. Below the colors are nearly those of the upper parts, that is, the body is a soft white, the wings brown. Bill and feet black. The sexes are alike in markings and size, the length being about six inches. They are both assiduous in the pursuit of insects, generally along streams on level spaces.

Monarcha or *Muscipeta melanopsis*, the Carinated Gray Flycatcher, has a ring of short black feathers about the large full eye, a discriminating characteristic, imparting with the strong prominent bill a singular appearance to this Australian bird. The entire throat and part of the face are also black, crowded upon by the soft slate color which becomes deeper over the rest of the body. The long tail above is dusky; below, as well as under the wings and on the abdomen, the color is a

bright rufous. The female is unmasked about the head and throat. Feet plumbeous. Length 7 inches.

One of the loveliest, certainly the most brilliant of Flycatchers is *Monarcha chrysomela*, the Goldenhooded. Blue-black and gold are the boldly contrasted colors of this bright little creature whose length is 6 inches. The ground color is orangeyellow; this is almost equally rich whenever it is spread. Jet black with a blue gloss covers the entire throat and upper breast, the upper back, the outer wing feathers and tail. An irregular stripe of the same bends round the shoulder. The deepest black is on the throat where the thick plumage is metallic. The crown is roughened into a kind of crest. The bill and feet are black. All besides, as has been said, is a lovely yellow, making the bird a most conspicuous object among the dark trees.

Todopsis cyanocephala of the *Muscicapidæ* is adorned with a blue crown, as its name indicates. This rich color appears besides on the neck, back and wings though of a somewhat different shade. A purpleblack runs down the lower back and covers the tail, excepting the two middle feathers which are of bluish tinge. The under parts are of a dark purple also, becoming black beneath the wings. The bill and feet are dull black. The length of the male bird is rather more than six inches, the female about an inch less. Her coloring is almost as rich, but different. A warm brown takes the place of black above, a light buff of the black below, though along the sides as far as the under tail coverts the brown reappears. Blue colors the head and stripes the neck, showing lighter on the tail where it is much mingled with white.

Malurus albiscapulatus is scarcely 4 inches in length but is not only of rich velvety plumage but of conspicuous appearance also, for its white patches on a black ground color attract attention at once. These patches occur on either side of the body both above and below, those above showing finely when the bird is in flight, those below lining the chest from the bend of the wings. Elsewhere the plumage is a deep black of a bluish cast, soft and lustrous. The home of the species is in Southeast as well as Northwest New Guinea.

Caterpillarcatchers (*Campephaga*) abound in New Guinea of varying degrees of beauty, some being bright of hue, others almost somber. A few individuals not in strict order are considered here.

Campephaga sloetii or *aurulenta*, according to d'Albertis (Vide Journal), is a rare bird in collections but is distributed all over New Guinea. He found it most numerous far up the Fly River, but obtained but one specimen in a native's garden, feeding on the small berries of a tall tree. It is a yellow bird, very vivid on certain parts, duller on the wings where there is more or less black and white as well, and golden yellow on the breast and abdomen. The head, sides of head and throat are marked with gray, black greenglossed, and a band of white. White inclining to yellow lines the under wings. Bill, feet and eyes are black. The bill is short and strong.

Where the male bird is brilliant and positive in color, the female assumes paler shades and neutral tones. She is somewhat longer, measuring nearly 8 inches in total length.

The tail feathers of the male are marked with white, especially the outer ones.

The mountain Cuckoo-shrike, *Campephaga montana* or *Edoliisoma montana* is a fine bird from the Arfak region. The contrasted colors, bluegray above, black below, are so carefully marked as to render their wearer easily distinguished from his kind. The same may be said of the female who is equally conspicuous in unusually clear colors and a perfectly black tail.

The Bluegray Campephaga, *Campephaga strenua* (Schl.), from about the same region is colored mainly as its name indicates, the customary black appearing on the throat and in a line on the head. The bill and feet are also black; some of the tail feathers likewise, but a rusty tinge marks the lower wing coverts. The bill is unuusally powerful for so small a bird.

Campephaga melas or *Edoliisoma nigrum* is found in different parts of Papua. It is a larger bird and with a coloration not at all characteristic of the class to which it bears so similar a name. The male is of a glossy black, reflecting purple along

the wing and tail coverts. The female, longer than her mate in size, is also quite distinct in plumage. A marked reddish dye takes the place of lustrous black. On the head the color is warmer than on the body. The wings are shaded, in some individuals dusky.

Edoliisoma tenuirostris or *Campephaga jardinii* (Gould), the Slenderbilled Cuckoo-shrike, is an Australian bird but found also in New Guinea near Port Moresby. It is about a foot long, of a cloudy blue color, excepting on the side face where it becomes black, and on wings and tail which contain rather more black than blue. The outer tail feathers underneath terminate in white. The bill is black and anything but slender. Feet black, eyes brown.

In 1882 the nest was found by Mr. C. C. L. Talbot in a Eucalyptus tree. It was composed of wiry grasses securely fastened together with cobwebs on the thin forked horizontal branch. The eggs laid in the small shallow depression were ovoid in shape and of a pale bluish gray ground dotted irregularly over with dark brown spots and lines.

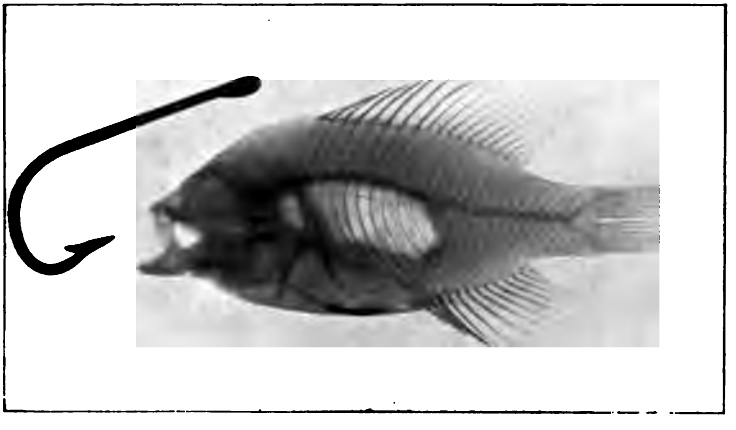
(To be Continued.)

EDITOR'S TABLE.

—WE notice that the project of a National University to be established at Washington has been again brought up before Congress. Washington has many advantages as a location for a university, and the Methodists and Catholics have not been slow to take advantage of them. The Columbian University, a non-sectarian institution, is located there. That it devolves on the nation under any circumstances to establish a university there or anywhere else we fail to perceive. So long as institutions of this kind exist either by virtue of State support or private munificence, there is no necessity for the intervention of the Government in this part of the educational field, but there are strong reasons why it should not do so. The financial basis of all institutions supported by congressional appropriations is always precarious. The subsidies are liberal while they last, but changes in the fiscal policy of the Government produce fluctuations in the revenue, and expenditures are varied accordingly. Then the faculty of such an institution would be under bonds to please the congressional majority,

PLATE V.

1



2

1. Natrix compressicauda. *2. Lepomis sp.*

or the revenues might be reduced or suspended. The teachings on certain subjects might be interfered with or controlled by the appointing power, and the appointments to positions would probably become political perquisites. Nothing more disastrous to the proper conduct of a university can be imagined, and an institution established under such conditions would soon cease to be a credit to the nation. We hope that the project will not prevail, not only for these reasons but for another. This is, that the Government has in connection with its departments various commissions and bureaus, which occupy themselves with original scientific research in connection with the various economic objects of their care. These should be continued and expanded if possible, and not, as is sometimes the case, weakened by insufficient appropriations. If the Government at Washington will support this work it will be doing more for education than any university can do, and will continue to add to its credit among nations in the future as it has done in the past.

—THE X-rays of Roentgen will prove of some utility to some branches of biological research by disclosing the characters of mineral substances enclosed within the walls of animals and plants. A good many characters of the skeleton, for instance, may be detected in specimens which cannot be spared for maceration, and other applications will occur to both botanists and zoologists. We present, as an illustration, a sciagraph of a species of sunfish (Lepomis), made by Messrs Leeds and Stokes, of Queen & Co., of Philadelphia.

—ANOTHER excellent journal, this time a French one, has been led astray by attaching too much importance to the romances of the American newspaper reporter. We refer to the story published some months ago by a San Francisco journal that a physician of that city had succeeded in grafting some snakes together by their tails. The fictitious character of the narrative is demonstrated by the statement that the said physician selected snakes in which the vertebral column does not extend to the end of the tail. If the editor of the journal had referred the question to the professors of the Museum of Paris, he would have learned that snakes of this kind exist only in the imagination of the author of the canard.

—WE published a statement some months ago that Mr. L. O. Howard of U. S. Dept. of Agriculture had discovered that the application of oil to water where mosquitoes breed, destroys both the eggs and the larvæ of those pestilent insects. We are reminded by an exchange that the alleged discovery was made by Mrs. Eugene Aaron in Phila-

delphia. We were probably indiscreet in referring to Prof. Howard's observations as involving more than a modicum of "discovery." On examination we find that the knowledge of this mode of destroying mosquitoes antedates not only his observations, but also those of Mrs. Aaron. The information has, however, not been generally disseminated until recently.

—THE American Society of Naturalists, at its last meeting, adopted a resolution commending to the public the importance of Antarctic exploration. A committee of three was appointed to take measures looking towards sending an expedition to Antarctica in the near future. At about the same time England and Australia joined in supplying the funds necessary for such an exploration of the land lying south of Tasmania within the Antarctic circle. The natural object of an American expedition is, of course, the exploration of Graham's Land, which lies due south of Patagonia. For the advance of knowledge of the physics of the globe, explorations of the polar regions are of the first importance; and the results to the history of its biology in past ages, will be scarcely less important. America has done her full share of Arctic exploration; and in the person of Commodore Wilkes made a beginning in Antarctic work. It is now fully time for us to resume this work, and it is to be hoped that the means of sending the expedition may be speedily obtained.

—THE Huxley Memorial Committee have raised the considerable sum of £1532, and are considering the uses to which it may be put. It has been resolved to erect a statue of Huxley in the British Museum, and to endow the award of a medal for meritorious work in biology. It is now desired that the amount may be increased for the purpose of creating another endowment. Should sufficient subscriptions be obtained in America, it might become appropriate that this new endowment should have its seat in this country. The scientific men of America hold in high esteem the biological work of Huxley, and there are many reasons why a foundation in his memory would be grateful to Americans.

RECENT LITERATURE.

Williams's Manual of Lithology[1] is written for the "beginner in the subject who wishes a thorough knowledge in the presentation of the subject, in a fuller and more compact arrangement than can be obtained in geological text-books. The arrangement is such that those who wish to continue the work in the microscopic analysis of rock forming minerals, as taught in petrography, will have nothing to unlearn."

The latter statement of the author is not quite true, for, in his classification of the rocks discussed, he places among the crystalline schists quartzite, pyroxene rock and olivine rock that present no traces of foliation. In the main, however, the classification is good. The rocks are divided into Primary Rocks and Secondary Rocks, and each of these groups is separated into "Divisions" in accordance with their chemical composition. Of the different families or "divisions" the effusive rocks are first described and then the intrusive ones. The Secondary Rocks embrace the Débris, the Sedimentary and the Metamorphic divisions, the first of which differs from the second in consisting of unconsolidated materials.

Nearly all the rock varieties recognized by petrographers are briefly described, and even many that are no longer recognized as distinct types. The descriptions are all based on macroscopic characters, but they are, in most cases, full enough to enable the user of the book to identify the type.

The terminology made use of in the description is somewhat different from that used in petrographical text-books, but, since it is employed in the description of hand specimens and not of their sections, this is to be expected. All the terms used are clearly defined, and many of the new ones introduced are perhaps needed.

The main faults to be found with the volume are that it attempts to discriminate between too many rock types, and that it contains too many rock names that have long since gone out of use. In spite of these faults, the treatise is a valuable one, and it should meet with success. The typographical work is excellent. The plates are from photographs, and are illustrative of rock structures.—W. S. B.

[1] Manual of Lethology : Treating of the Principles of the Science, with Special Reference to Meguscopic Analysis. By Edward H. Williams. 2d Ed. New York: John Wiley & Sons, 1895. Pp. vi, 418; plates 6. Price, $3.00.

The Corundum Deposits of Georgia.[2]—This preliminary report on the corundum deposits of Georgia, by Francis P. King, has been issued as Bulletin No. 2 by the Geol. Survey of that State. The importance of corundum in the arts, and the high price paid for it, together with the fact that Georgia ranks second in the Union in the production of this mineral, make the report of special interest. The introductory chapters give the history, varieties and associate minerals of corundum, succeeded by a brief account of the geology of the crystalline belt in which the mineral occurs and the distribution of deposits. Several pages are given to the economics, including natural and artificial abrasives. There is also a bibliography of the American literature upon the subject.

The map accompanying the report is well-colored, showing at a glance the different formations. The other illustrations are reproductions from photographs, showing out-crops of the mineral-bearing veins.

Bailey's Plant Breeding.[3]—No man in the country, perhaps, is better prepared to write a book on plant breeding than the accomplished professor of horticulture in Cornell University, and it is a pleasure to find that in the preparation of the work before us he has not disappointed his friends. There is, as the author says in his preface, much misapprehension and imperfect knowledge as to the origination of new forms of plants, and much of what has been written on the subject is misleading. "Horticulturists commonly look upon each novelty as an isolated fact, whilst we ought to regard each one as but an expression of some law of the variation of plants." The author might have included in the foregoing many "botanists" as well as the horticulturists, for, unfortunately, it is true that many who call themselves botanists, and who hold positions in honored institutions, have not yet risen to a biological conception of the science which they profess to cultivate.

Among the topics treated in these lectures are the following, viz.: individuality, fortuitous variation, sex as a factor in the variation of plants, physical environment and variation, struggle for life, division of labor, crossing, etc.

The book should be in every botanist's library, and every teacher of botany will do well to make copious extracts from it in his lectures.

[2] A Preliminary Report on the Corundum Deposits of Georgia. By Francis P. King, Bull. No. 2, Georgia Geological Survey, Atlanta, 1894.

[3] *Plant Breeding*, being five lectures upon the Amelioration of Domestic Plants. By L. H. Bailey. New York: Macmillan & Co., 1895. pp. xii, 293, 12 mo.

The following, from page 135, will show many teachers that much may be learned from this book: "Some two or three years ago, a leading eastern seedsman conceived of a new form of bean-pod which would at once commend itself to his customers. He was so well convinced of the merits of this prospective variety, that he made a descriptive and "taking" name for it. He then wrote to a noted bean-raiser, describing the proposed variety and giving the name. 'Can you make it for me?' he asked. 'Yes, I will make you the bean,' replied the grower. The seedsman then announced in his catalogue that he would soon introduce a new bean, and, in order to hold the name, he published it along with the announcement. Two years later I visited the bean-grower. 'Did you get that bean?' I asked. 'Yes; here it is.' Sure enough, he had it, and it answered the requirements very well."

—CHARLES E. BESSEY.

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General Notes.

PETROGRAPHY.[1]

The Eruptives of Missouri.—Haworth[2] has described in much detail the dykes and acid eruptives in the Pilot Knob region, Missouri. The dyke rocks are typical diabases, diabase-porphyrites, quartz-diabase-

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Mo. Geol. Survey, Vol. VIII, 1895, p. 83-222.

porphyrites and melaphyres. The author unfortunately classes as diabase-porphyrites both glassy and holocrystalline rocks. The acid rocks of the region include granites, granite-porphyries, porphyrites and quartz-porphyries. The first two are characteristically granophyric. Their orthoclases are often enlarged by granophyre material whose feldspar is fresh, while the nucleal feldspar is much altered. The quartzes likewise, are enlarged by the addition of quartz around them. There were two periods of crystallization in these rocks. In the second period the phenocrysts were corroded and the groundmass was produced. In addition to the quartz and orthoclase there are present in these rocks also biotite, hornblende, plagioclase and a number of accessory and secondary components. The porphyries and porphyrites contain the same constituents as the granites, from which they are separated simply on account of differences in structure. The phenocrysts are mainly orthoclose, plagioclase, microcline and quartz, many of which are fractured in consequence of magma motions. The groundmass in which these lie is of the usual components of porphyry groundmasses, and in texture is microgranitic, granophyric, micropegmatitic and spherulitic. Many of the porphyries contain fragments of their material surrounded by a matrix of the same composition in which flowage lines are well exhibited. These rocks are evidently volcanic breccias. The author divides the porphyritic rocks into porphyries and porphyrites, the latter containing plagioclase phenocrysts and the former phenocrysts of quartz, orthoclase and microcline.

Rocks from Eastern Africa.—The volcanic rocks of Shoa and the neighborhood of the Gulf of Aden in Eastern Africa comprise a number of varieties that have been carefully studied by Tenne.[3] The main mass of the mountains of the region consists of biotite-muscovite gneiss. This is cut by nepheline basanites, the freshest specimens of which contain phenocrysts of olivine, augite and feldspar in a groundmass of plagioclase, augite, nepheline and often olivine. Trachytes, phonolites and basalts occur in the Peninsula of Aden. The trachytes include fragments of augite-andesite. Inland granophyres with pseudospherulites in their groundmass, trachytes and feldspathic basalts were met with. The granophyres are much altered. In the fine grained product formed by the decomposition of the groundmass of one occurrence quartz, feldspar, and a blue hornblende with the properties of glaucophane can be detected. All the rocks are briefly described. They present no peculiar features other than those indicated.

[3] Zeits. d. deutsch. geol. Ges., XLV, p. 451.

A Basic Rock derived from Granite.—Associated with the ores in the hematite mines of Jefferson and St. Lawrence Counties, N. Y., is a dark eruptive rock that was called serpentine by Emmons. Smyth[4] (C. H.) has examined it microscopically and has discovered that it consists of a chlorite-like mineral, fragments of quartz and feldspar. By searching carefully he discovered less altered phases of the rock that were identified as granite. The peculiar alteration of an acid granite to a basic chlorite rock is ascribed to chemical agencies. According to the author's notion the pyrite in a neighboring highly pyritiferous gneiss was decomposed, yielding iron sulphates and sulphuric acid. These solutions passed into limestone yielding the ores and then into the granite changing it into chlorite. The altered rock is found only with the ores. The original was probably not always granite. An analysis of the altered rock gave:

SiO_2	Al_2O_3	Fe_2O_3	FeO	MgO	CaO	Na_2O	K_2O	H_2O	Total
29.70	17.03	27.15		10.66	1.68	.56	.10	11.79	=98.67

Cancrinite-Syenite from Finland.—In the southeastern portion of the Parish Kuolajaroe in Finland, Ramsay and Nyholm[5] secured specimens of a nepheline-syenite containing a large quantity of what the authors regard as original cancrinite. The rock is found associated with gneissoid granite at Pyhakurn. The rock is trachytic in structure and is composed of orthoclase, aegerine, cancrinite and nepheline as essential constituents and apatite, sphene and pyrite as accessories. The cancrinite was the last mineral to crystallize. It occupies the spaces between the other components, and yet it often possesses well defined hexagonal forms. It occurs also as little prisms included within the orthoclase. Because of this association and because the nepheline in the rock is perfectly fresh the cancrinite is regarded as original. This mineral comprises 29.04% of the entire rock.

The same authors in the same paper describe a porphyritic melilite rock found as a loose block a few kilometers W. N.-W. of Lake Wuorijarvi. It contains large porphyritic crystals of melilite, pyroxene and biotite in a groundmass composed of labradorite, zeolites and calcite. The pyroxenes are made up of a colorless augite nucleus surrounded by zones of light green aegerine-augite and deep green aegerine. No olivine was detected in any of the thin sections.

[4] Jour. Geology, Vol. 2, p. 667.

[5] Bull. Com. Geol. d. l. Finn., No. 1.

Rocks from the Sweet Grass Hills, Montana.—Weed and Pirsson[6] describe the rocks of the Sweet Grass Hills of Montana as quartz-diorite-porphyrites, quartz-syenite-porphyries and minettes. The first named rock presents no special peculiarities. The quartz-syenite-porphyry contains orthoclase, plagioclase and augite-phenocrysts in a fine groundmass of allotriomorphic feldspar and quartz. The augite is in short thick prisms composed of a pale green diopside core, which passes into a bright green aegerite mantle. The minette also contains aegerine, but otherwise it is typical.

Petrographical News.—Two peculiar phonolitic rocks are described by Pirsson[7] from near Fort Claggett, Montana. One is a leucite-sodalite-tinguaite, with leucite pseudomorphs, and sodalite as phenocrysts in a groundmass composed mainly of a felt of orthoclase and aegerine. The leucite pseudomorphs are now an aggregate of orthoclase and nepheline. In the centers of some of them are small stout prisms of an unknown brown mineral, that is pleochroic in brownish and yellowish tints. The second rock is a quartz-tinguaite porphyry somewhat similar to Brögger's grorudite.[8]

In a few notes on the surface lava flows associated with the Unkar beds of the Grand Cañon series in the Cañon of the Colorado, Ariz., Iddings[9] briefly describes compact and amygdaloidal basalts and fresh looking dolerites that are identical in all respects with modern rocks of the same character.

Laspeyres[10] estimates that the quantity of carbon-dioxide in liquid and gaseous form contained in rocks is sufficient to serve as the source for all that which escapes from the earth's natural fissures as gas, as well as that which escapes in solution with spring water. It may be set loose from the rocks through the action of heat or through the action of dynamic forces.

In a handsomely illustrated brochure Merrill[11] describes the characteristics of the onyx marbles and the processes by which they originate. Differences in temperature, according to the author, are not the controling conditions determining the differences in texture between the onyxes and travertine. He is inclined to the belief that the banded onyxes were formed by deposition from warm solutions under pressure flowing into pools of quiet cold water.

[6] Amer. Jour. Sci., Vol. I, p. 309.
[7] Amer. Journ. Sci., 1895, Nov. p. 394.
[8] AMERICAN NATURALIST, 1895, p. 567.
[9] 14th Ann. Rep. U. S. Geol. Survey, p. 520.
[10] Korrespond. bl. Naturh. Ver. preuss. Rheinl., No. 2, 1894, p. 17.
[11] Rep. U. S. Nat. Mus., 1893, p, 539.

In a preliminary report on the Geology of Essex County, N. Y., Kemp[12] describes the occurrences of the gneisses, limestones, ophicalcites, gabbros, lamprophyres and other igneous rocks of the district, and gives an account of their geological relationships.

GEOLOGY AND PALEONTOLOGY.

Bear River Formation.—The explorations of Mr. Stanton and Mr. Charles White in the Bear River Valley have been the means of correcting a long standing error among geologists concerning the taxonomic position of strata known as the Bear River Formation. A summary of the facts as presented by Mr. White in a late Bulletin of the U. S. Geol. Survey shows that the formation under discussion is not Laramie, to which age it has been hitherto been referred, but belongs to the Upper Cretaceous, at or near the base of that series. That is its position has been determined by Mr. Stanton as beneath the Colorado formation, and above that series of Jurassic strata which occurs within a large part of the interior region of North America generally regarded as of Upper Jurassic age and which in the general section given is called "Dakota?" This accords with the reputed age of a formation in Hungary, whose fauna is more nearly like that of the Bear River series of strata than of any other known.

Mr. White, therefore, defines the Bear River series as a distinct formation stratigraphically, geographically, and paleontologically, and states in detail its taxonomic position. All the known fossils of the formation are described and figured, comparisons are made of its fauna with those of other nonmarine formations of this and other continents, and relevant biological questions are discussed.

In making a general comparison of the Bear River fauna with the other nonmarine fossil faunas of North America, Dr. White calls attention to those features of the Bear Fauna by which it differs conspicuously from all the others. Reference is here especially made to the Auriculidæ and Melaniidæ, because it is members of these two families that give the Bear River Fauna its most distinctive character. In this connection the author remarks "this faunal character is all the more conspicuous because, of the six genera which represent those two families, only two of them are known in any other North American fauna, either fossil or recent."

[12] Report of State Geologist [of New York] for 1893, p. 433.

The similarities and contrasts between the fauna of the Bear River formation and those of the other nonmarine beds of North America leads to a discussion of their causes. The author suggests that certain genetic lines of descent have become diverged from the main lines of succession and destroyed by some of those physical changes which mark successive epochs, and adds "we may reasonably assume that one of those divergent lines terminated in the Bear River fauna; that is, at the close of the Bear River epoch the area which its nonmarine waters had occupied having become overspread by the marine waters in which the Colorado formation was deposited, it is not probable that any fluvial outlet of the former nonmarine waters was perpetuated, and there was, therefore, no provisional habitat in which the Bear River fauna might have been preserved. It was probably in this way that the distinguishing types of that fauna became extinct, together with others of its members which were not so specially characteristic of it." (Bull. U. S. Geol. Surv. 128, Washington, 1895.)

On the Occurrence of Neocene Marine Diatomaceæ near New York.—The rocks which contain Diatomaceæ (or Bacillariaceæ) in America are clayey, that is to say they contain more or less of clay, and they vary in color from a nearly white to a fawn color and to a greenish, greyish-brownish or almost black. They are not older than the Oligocene nor newer than the Plistocene. They can be placed in the Neocene, a period that ranges from the Eocene to the Plistocene, and not in the recent. Those I have to describe in New York are not Miocene, but they belong to a place which may provisionally be classed as Pliocene or Plistocene of the European geologists.

Ever since 1843, the so-called infusorial earth has been known in Virginia and was thought by Rogers the discoverer to be Miocene Tertiary, he classifying it as the European rocks were. Bailey accepted the classification and so did the later geologists. When fresh water fossil Diatomaceæ were found in Massachusetts they were thought to be Miocene also without studying the rocks themselves and seeing how they stood in the geological scale. When they were found in New Hampshire I did not classify them nor did Hitchcock attempt to do so. They were placed in the lacustrine Sedimentary and provisionally in the Recent. But now they can be seen to be older than the Recent and must be placed in a position by themselves. In the Iceberg period, the Champlain, when the ice which covered the country was beginning

to melt, icebergs which formed by the breaking off of the ice on the border were common. The icy water had Bacillariaceæ in it, for they existed, as they do now, when the temperature was at 0° C. This flowed down to the lower regions from the north and northwest.

In California I did not classify the rocks containing the Bacillariaceæ leaving that to the older and more experienced geologists. Blake, who had discovered them at Monterey, supposed them to be Miocene, for he saw as Bailey showed them to be similar to the Virginian ones. In Japan where I discovered them also I failed to classify them for Pumpelly, who had brought them home did not place them likewise. When the infusorial earth was found in Florida, it had also been placed in the Miocene Tertiary by Bailey. And when I had it from that state subsequently at Manatee, I failed to classify it because I had not visited the spot where it came from myself. Now I believe these are older than what is called the Miocene. And I am confirmed in this supposition by what Towney said of the Virginia stratum. I prefer to place them as far back as the Upper Eocene, the Oligocene as it is called. In New Jersey at Asbury Park and Atlantic City the infusorial earth has been found by Woolman and classified by him as Miocene. But further north on the Atlantic side of the continent it has not been seen. I examined the clay that was dug at about two feet down at Foley's, South Beach, Staten Island, N. Y., but although it contained marine Bacillariaceæ it was not what I wanted. I thought it belonged to the Raised Coast period. At Martha's Vineyard, Mass. the clay classed as Miocene by Dall did not contain any Bacillariaceæ.

It was on the 11th of August, 1895, that I visited Rockaway to get rest from the turmoil and heat of the city. Rockaway is a beach or promontory which extends down from a place called Far Rockaway southwards on the coast of Long Island. Long Island is made up of hills of no great height extending down the middle or on the north shore of the island. A low range of country extends down the southern shore where the Atlantic Ocean begins. It is fringed by sandy bars which are mostly islands. These islands extend down the coast from Cape Cod, Mass. to Florida. Key West is the most southern of the islands which are known in Florida as Keys. The country on the Atlantic side of the island is low, sloping down to the coast without any elevation in it.

I knew that I should go down by rail cutting through the hills until I came transversely to the island to the promontory of Rockaway. It is true that I wanted to get out of the cities heat but I had also two other reasons for going. I wanted to study the glacial phenomena which I

knew would present themselves there. At the same time I desired to search for the infusorial earth. At one place we came to a kettle hole, at the Lutheran Cemetery. I was sure it was a kettle hole and knew there was clay, a Lacustrine Sedimentary deposit of Diatomaceæ, at the bottom. I saw the glacial moraine made up of gravel and sand all along the road. The moraine was a gravelly till with boulders scattered through it. On the top it was capped by a layer of about three feet thick of whitish clay. This I knew to be diatomaceous, the same as covers the country in New Jersey and on Manhattan or New York Island. As we approached the station known as Brooklyn Hills we cut through three high hills which I saw then and afterwards were made up of moraine stuff, mostly gravel, with a white clay about three feet thick on top. The clay was the same as we had just passed. It makes the bottom of the glacial clay, the Lacustrine Sedimentary deposits of Diatomaceæ. In this moraine I afterwards got a small distinctly striated boulder and near the bottom of the hill, about twelve feet from the bottom was a grey clay with Hematite nodules in it. Cretaceous clay no doubt.

The country became flat with no rising in it and sloping gradually towards the coast where we came to the station known as Aqueduct. Cretaceous clay underlies the country doubtless covered by glacial till or moraine. At Aqueduct the railroad runs out on tressels to Rockaway. At Rockaway Beach I landed and wandered south on the promontory but found nothing but white siliceous sand, they were not digging anywhere that I could find. I wandered north in the direction of Far Rockaway where the land became higher and was covered by the whitish Iceberg period clay which evidently came from the north-west. At Auvergne they had been digging a ditch to reclaim the land from the sea. This was on the opposite side of Rockaway to the Atlantic Ocean, on Jamaica Bay. The digging was over six feet deep. They had thrown out some of the Iceberg clay and below that some greyish soil without any stones in it. I saw at once that it was different in character from the soil on the marshes and which I had learned belonged to the Raised Coast or Champlain Period. I took some home and examined it and came to the conclusion that I had found what I was in search of, the infusorial earth. It was no doubt what may be termed Pliocene Tertiary and belonged to the Neocene Period.

I cleaned some and found the following Bacillariceæ in it besides some forms of *Dictyota*, which are Radiolaria. So me few usual forms escaped me but will probably be found hereafter.

Achnanthes subsessilis C. G. E.
Actinocyclus ehrenbergii J. R.
Actinoptychus undulatus C. G. E.
Auliscus cœlatus J. W. B.
Auliscus pruinosus J. W. B.
Auliscus radiatus J. W. B.
Aulacodiscus germanicus C. G. E.
Amphora ovalis F. T. K.
Amphiprora elegans W. S.
Amphiprora navicularis C. G. E.
Amphiprora pulchra J. W. B.
Biddulphia aurita A. B.
Biddulphia pulchella G.
Biddulphia rhombus W. S.
Cerataulus radiatus J. R.
Cerataulus smithii W. S.
Cerataulus turgida W. S.
Coscinodiscus asteromphalus C. G. E.
Coscinodiscus excentricus C. G. E.
Coscinodiscus subtilis C. G. E.
Coscinodiscus lineatus C. G. E.
Coscinodiscus nitidus W. G.
Cocconeis scutellum C. G. E.
Cyclotella striata F. T. K.
Dicladia mitra J. W. B.
Doryphora amphiceros F. T. K.
Epithemia turgida F. T. K.
Epithemia musculus F. T. K.
Eunotia monodon C. G. E.
Euotiogramma amphioxys C. G. E.
Fragillaria pacifica A. G.
Grammatophora marina F. T. K.
Hyalodiscus franklinii C. G. E.
Hyalodiscus stelliger J. W. B.
Isthmia enervis C. G. E.
Melosira sulcata C. G. E.
Navicula clavata A. G.
Navicula didyma C. G. E.
Navicula elliptica F. T. K.
Navicula hennedii W. S.
Navicula humerosa A. B.
Navicula lacustris W. S.
Navicula lata A. B.
Navicula peregrina F. T. K.
Navicula permagna J. W. B.
Navicula viridis C. G. E.
Nitzschia acuminata W. S.
Nitzschia balanotis A. G.
Nitzschia sigma F. T. K.
Nitzschia tryblionella H.
Plagiogramma gregoriana R. K. G.
Pleurosigma angulata W. S.
Pleurosigma balticum C. G. E.
Pyxilla ? baltica A. G.
Pyxidicula compressa J. W. B.
Rhabdonema arcuatum F. T. K.
Roicosphenia currata F. T. K.
Scoliopleura tumida L. R.
Schizonema fœtida J. E. S.
Stauroneis aspera C. G. E.
Stauroneis birostris C. G. E.
Stephanopyxis appendiculata C. G. E.
Stephanopyxis turris J. R.
Surirella febigeris F. W. L.
Surirella striatula B. V.
Synedra affinis F. T. K.
Terpsinoe americana J. W. B.
Triceratium alternans J. W. B.
Triceratium favus C. G. E.
Triceratium maculatum F. T. K.
Triceratium punctatum T. B.

These are all the Bacillariaceæ that I have detected up to this time. There are several forms of *Dictyocha*, a genus of Radiolaria, present

also. And what I consider a new genus of Bacillariaceæ, which I have called *Ancile radiata.* It is free and found rarely in the salt water in Jamaica Bay, Rockaway and at Foleys, and South Beach, Staten Island. But of this I shall speak hereafter. Mr. W. A. Terry says he has found broken fragments of *Brunia* but this I myself have not seen, although common in a deposit which I will also describe hereafter taken at fifteen feet from the surface at Hoboken, N. J, I, another day, visited Coney Island, N. Y., and searched for infusorial earth and this time was fortunate enough to find it at Sheephead Bay, which is a village just on the Long Island side of Coney Island Creek. It was a grayish colored clay, one foot underneath the sand taken at low water, about eight feet from the surface of the soil. At Canarsie Landing, which is on Jamaica Bay between Coney Island and Auvergne, I did not find the infusorial earth, but I was there a very short time. I did find glacial phenomena and indication of the elevation of the coast, but of those I shall not speak now as they are not microscopical. But the finding of Bacillariaceæ in the infusorial earth, as belonging to the Upper Neocene period, is thus a fact, and the date of so finding is worthy of record. Perhaps they will be found more inland on Long Island hereafter. I have searched for them as far inland as the city of Jamaica, but without result.

This layer is in the Upper Neocene, or perhaps the Plistocene, but the placing of it definitely is extremely difficult if not impossible at present, for on describing a fossil marine Diatomaceous deposit from St. Augustine, Florida, Mr. Charles S. Boyer says (Bulletin of the Torry Botanical Club, April, 1895, Vol. 22, No. 4, page 172) that it, the St. Augustine deposit, "overlies an Eocene deposit and is beneath the Plistocene" and that the Barbadoes deposit, which corresponds partially with it, "is now claimed to be Pliocene." In fact, as I have already pointed out, the marine fossil layers of Bacillariaceæ, be it from Mors, Denmark; Simbirsk, Russia; Sentz Peter, Austria; Oran, Algiers; Moron, Spain; Argentina; Payta, Peru; New York to Virginia, California and New Zealand, including the Nicobar Islands, are Neocene, be that Miocene or Pliocene.

—ARTHUR M. EDWARDS, M. D., Newark, N. J.

The succession of Glacial changes.—Evidence has been accumulating during the last few years in favor of the periodicity of glacial action. Mr. Geikie recognized in Europe six distinct glacial epochs separated by genial periods, making in all eleven glacial and interglacial stages. For convenience he gives each of these horizons a separate name. The climax of glaciation was reached in the third

stage, that is, the second glacial epoch, after which the cold stage diminished continuously in importance. In like manner, the earliest interglacial epoch seems to have been the most genial, each successive epoch approximating more and more closely to existing conditions.

The American glacial deposits have been classified by Mr. Chamberlin, and an attempt made to correlate them with those of Europe. The following table shows the tentative correlation.

GLACIAL AND INTERGLACIAL STAGES.

European.	American.
XI. Upper Tubarian=Sixth Glacial Period.	
X. Upper Forestian=Fifth Interglacial Period.	
IX. Lower Turbarian=Fifth Glacial Epoch.	
VIII. Lower Forestian=Fourth Interglacial Epoch.	
VII. Mecklenburgian=Fourth Glacial Epoch.	Wisconsin.
VI. Neudeckian=Third Interglacial Epoch.	Toronto.
V. Polandian=Third Glacial Epoch.	Iowan.
IV. Helvetian=Second Interglacial Epoch	Aftonian.
III. Saxonian=Second Glacial Epoch.	Kansas Formation.
II. Norfolkian=First Interglacial Epoch.	
I. Scanian=First Glacial Epoch.	

The complex series subsequent to the Wisconsin formation have not been sufficiently investigated to permit even a tentative correlation, or indeed, to even designate the specific formations. This statement is equally applicable to the formations deposited during the advancing stages of the glacial period in America. (Journ. Geol., Vol. III, 1895.)

Geologic News.—PALEOZOIC.—Haworth proposes to divide the Coal Measures of Kansas into Upper and Lower, the division to be at the top of the Pleasonton shales, which is at the bottom of the Erie limestone. The division is based principally on paleontological evidence. In the author's study of the Kansas Coal Measures he finds that the shales are of submarine origin, while the entire formation appears to have been laid down during a period of gentle oscillations, with the greatest movement to the west, and the least to the east. (Kan. Univ. Quar., Vol. III, 1895.)

An Orthoceras shell of gigantic proportions has been found in the Lower Coal Measures of Iowa, about forty miles from Des Moines. This specimen is three inches in diameter and as it is of the same very slender as the associated forms, it could not have been less than six feet in length, and probably was even longer. The species is *O. fauslerensis.* (Science, Jan., 1896.)

MESOZOIC.—In examining the microscopic structure of the flint nodules found in the Lower Cretaceous of Texas near Austin, Mr. J. A. Merrill found traces of the following organisms: Foraminifera, sponges, molluscs represented by the nacreous tissue of the shells, and fishes represented by their scales. The fact that the delicate spines of the sponge spicules, even to the most minute barb are perfectly preserved, showing no trace of having been subjected to mechanical movement, leads to the conclusion, that these flints result from the continuous growth of sponges *in situ*. Mr. Merrill's study then confirms to this extent the view taken by Prof. Sollas in his study of the nodules of the English flint. (Bull. Harvard, Mus. Comp. Zool., Vol. XXVIII, 1895.)

CENOZOIC.—Mr. G. H. Ashley's studies of the Coast Range Mts. of California lead him to the conclusion that the east and west ranges of Santa Barbara, Ventura and Los Angeles counties were elevated at about the end of the Miocene, while the ranges to the north with a uniform strike of northwest and southeast were elevated at or near the end of the Pliocene. (Geol. Mag., Vol. III, 1895.)

Mr. A. M. Edwards reports Cenozoic clay containing marine forms of diatomaceæ from Rockaway, Long Island. The clay deposit is dark green or grey in color, and is capped by a fresh water deposit of white clay. (Observer, Dec., 1895.)

Prof. H. L. Fairchild enumerates eight reasons for regarding the Pinnacles Hills, near Rochester, N. Y. as a kame series forming a part of a frontal moraine. This is contrary to the views of Upham who considers that they were deposited "in the ice-walled channel of a stream of water," "open to the sky." (Amer. Geol., Vol. XVI, 1895.)

BOTANY.[1]

A recent paper on the relation between the Ascomycetes and Basidiomycetes.[2]—In the October number of the *Revue Mycologique* under the heading "A Fungus simultaneously an Ascomycete and Basidiomycete" appears a résumé by R. Ferry of a portion of

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska, Dec. 21, 1895.

a paper published in *Mémoires couronnés de l'Académie de Belgique,* 1894 by Ch. Bommer. I have not seen the original paper, but as Ferry gives quite a lengthy account of it and quotes the most essential parts there seems to be sufficient basis for some remarks.

The fungi under consideration are *Mylitta australis* Berk. and *Polyporus mylittæ* Cooke and Massee. The former is a large irregularly spherical hypogeous fungous growth found in Australia and Van Diemans' Land and called by the inhabitants "native bread." It was first described by Berkeley in *Ann. and Mag. of Nat. Hist.*, 1839 and referred to *Mylitta*, a doubtful genus established by Fries upon what is now known to be a gall. Berkeley says he found no spores but noticed that the ends of some of the hyphæ were swollen. No one seems to have examined the fungus for some time after Berkeley described it. According to Ferry, Tulasne regarded it as a mycelial formation analagous to *Pietra fungifera* of Battara and older writers, which is now known to be the sclerotium stage of *Polyporus tuberaster* Fr. Later Cooke and Massee[3] referring to the plant incidentally call it a sclerotium and Saccardo[4] who examined it recently, says he observed spores (?) which were globose, smooth, hyaline, plainly nucleate and 14–15μ. in diameter. Such in brief was the knowledge of the plant before the appearance of the paper under discussion.

The latter plant *Polyporus mylittæ* C. & M. (fig. 1) was first described in *Grevillea* l.c. It is a short stipitate plant with a tough pulvinate pileus about 10 cm. broad, found growing on *Mylitta australis* in southern Australia. The authors say in a note; "A most interesting production, undoubtedly the ultimate development of the sclerotium long known as *Mylitta australis* Berk."

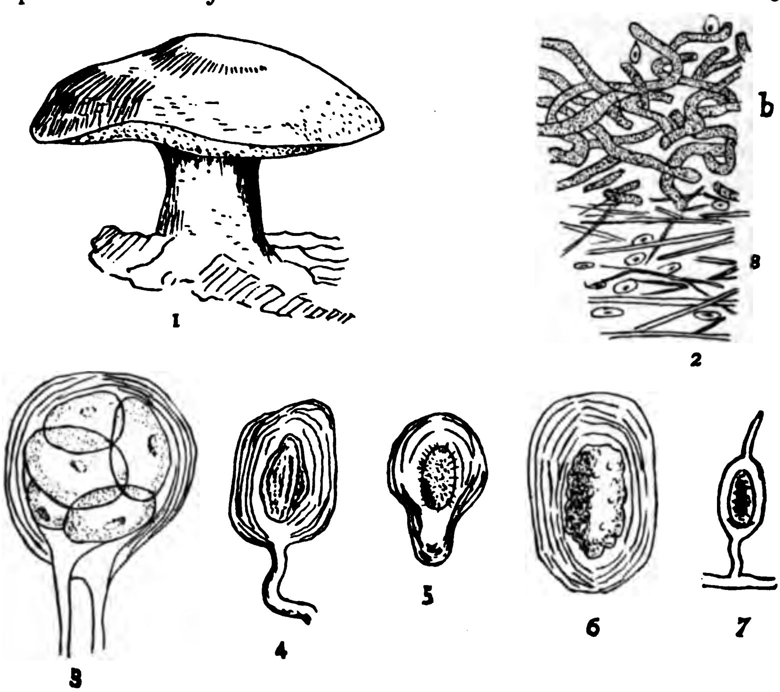
A year later Saccardo (l. c.) published a slightly different form of the same fungus under the same name. After the description he adds: "Growing on *Mylitta australis* from which it appears to originate. The texture of the *Polyporus* and of *Mylitta* are about the same. They are formed of intertwining filaments with frequent globose swellings constituting a soft or suberose white mass. It is very probable, therefore that *Mylitta* is the sclerotium form of the *Polyporus* and probably bears the same relation to the *Polyporus* that *Ceriomyces* bears to *Polyporus biennis* (Bull.) Fr."

Referring now to Bommer's paper we shall give the essential parts of Ferry's summary and translate the important parts of the quotations from the author. Ferry first gives an account of *Mylitta australis* as observed by Bommer.

[3] Cooke and Massee. Grev., **21**: 37. Dec., 1892.

[4] Saccardo. Hedw., **32**: 56. March and April, 1893.

Specimens are compact, very hard and covered with a superficial black crust. In full grown plants the interior is divided into a number of irregular cavities. The walls of these cavities are formed of a white tissue which under the microscope is seen to consist of thick-walled hyphæ which are stained by Bismark brown (fig. 2 b.). These hyphæ are from 4–8μ. in diameter. The cavities soon become filled with a gelatinous substance of a horny consistency in which some thin, hyaline, flexuose hyphæ are found buried. These are not colored by Bismark brown. Some of these hyphæ have ovoid swellings 5–8μ. long near their ends which contain 1, 2 or 3 ovoid bodies with very thin walls. Each body contains a kind of nucleus. Later these swellings (fig. 2 a.), especially those near the periphery of the gelatinous mass increase in size and contain only one ovoid body. This is brown, verrucose, very refringent, presenting all the characters of a spore and is regarded as such by Bommer. Since he finds what he considers asci and spores he refers *Mylitta australis* to the Tuberaceæ. He describes the



mature asci and spores as follows: "The asci (fig. 3) are analagous to those of *Tuber melanosporum*, being ovoid or spherical and 40–50μ. in their greatest diameter. The membrane is thin and encloses a single

elongated hyaline spore 20–30μ. long which is either smooth (fig. 4), verrucose (fig. 6) or echinulate (fig. 5)."

Nearer the centre of the gelatinous mass he says the asci are less plainly differentiated and frequently contain no spores (fig. 7). He submitted the fungus to chemical tests and found a great abundance of cellulose, but no glycogen, a substance usually present in Tubers.

The ordinary structure of the plant is, according to the author, as described above. As to its relation to *Polyporus mylittæ* which is frequently found growing from it, he says: "A specimen from the British Museum removes all doubt. This specimen like many others has a central cavity on one of the walls of which is seen a pulvinate mass formed by the hymenium of *Polyporus mylittæ*. This pulvinus does not possess true pores, but only small hemispherical cavities on its surface and numerous small rounded closed cavities in its interior which are covered by the hymenium. The mass ef hyphæ which forms the base of this hymenium is identical with the opaque white tissue which composes the walls of the cavities of nearly mature examples of *Mylitta*. Notwithstanding the presence of the pores and the thicker and more crowded hyphæ disposed after the manner of palisade tissue so characteristic of the hymenium of ordinary Hymenomycetes, the specimen is unfortunately sterile.

The particular disposition of the hymenium and the continuity and identity which exists between it and the sterile tissue of *Mylitta* establishes the fact that there exists between *Mylitta* and the *Polyporus* an intimate relation of the same nature as that which exists between the different stages in the life history of many fungi. Hence it follows that a carpophore of a Hymenomycete (*Polyporus mylittæ*) is here in reality the conidiophore of an Ascomycete (*Mylitta australis*). If this conclusion is true in the present case, it ought to be admitted that this is the relation which exists in general between the Basidiomycetes and Ascomycetes." ! ! !

Such are the author's preposterous conclusions, and thus is the autonomy of the Basidomycetes calmly disposed of.

One's first impulse is that this is a huge joke, but when you reflect that it emanates from the Belgian Academy of Sciences and is tacitly accepted by Ferry one of the editors of the *Revue Mycologique* the matter takes a serious aspect and it seems necessary to file a protest.

Ferry adds that DeBary long ago expressed the opinion that the Basidiomycetes and Uredineæ may be conidial forms of Ascomycetes. The only statement I find in DeBary[5] touching the point comes far

[5] DeBary, Comp. Morph. and Biol. of Fungi (Eng. trans.), p. 341.

from endorsing such an idea. The reviewer says further that Brefeld admits, theoretically, the existence of such fungi, but does not admit their actual existence because he does not think that two reproductive bodies of such complex development are able to be produced simultaneously. This idea Ferry regards as explaining the imperfect development of the *Polyporus* in Bommer's plant, and he adds, with a profundity equal to that of the author himself, that there are no existing characters which permit the plain separation of conidial bearing basidia (conidiophores) from typical basidia.

As to the author's observations, if accurately made they are of course deserving of consideration. We have not the space nor is it our purpose to discuss them here. Were it not for the fact that the normal form of *Polyporus mylittæ* is frequently found growing on *Mylitta*, and is regarded by several observers as being genetically connected with it, we might possibly disregard the supposed sterile hymenium and accept *Mylitta* as a tuber, though several of the author's observations are at variance with the characters of any known tuber. I am inclined for the present, however, to accept Tulasne's opinion and regard *Mylitta* as a sclerotium or a conidial stage of the *Polyporus*. As to the asci they may be illusions or may belong to some parasitic fungus. This is mere conjecture, however. These fungi are interesting forms and it is hoped that their study may be continued until the author's observations are confirmed or rejected. Accurate observations are always welcomed by botanists, but gratuitous and unfounded conclusions and generalizations should find no place in botanical literature.—C. L. SHEAR.

Polyporaceæ, Hydnaceæ, Helvellaceæ.—The undersigned desires species of the above groups from all parts of North America for the purpose of accumulating materials from which to monograph these families. In sending specimens, good representatives are desired, not mere fragments or abortive specimens. Where possible, indicate the host on which the fungus grows if a lignatile species, and especially in the case of fleshy or semi-fleshy forms, it is desirable to note the characters in a fresh condition. Even the most common species are desired in order to determine geographic distribution. When it is remembered that not a single species of any of these groups has been reported from more than half of our states and territories, it will be seen how great the necessity of coöperation on the part of local botanists and botanical collectors in order that this preliminary monograph may be as fairly representative of our flora as possible.

Before sending large packages, a preliminary correspondence will be desirable in order that the package can be sent the cheapest way. So

far as possible, specimens will be named for contributors and in all cases full credit will be given.—LUCIEN M. UNDERWOOD, Auburn, Ala.

The Smut of Indian-Corn (*Ustilago zeæ-mays*).—It has been found out at the Indiana Experiment Station that the smut does not attack the plant through the seed as has been supposed but like wheat rust it starts in the leaves and stems, wherever the spores are carried by the wind and find lodgment and sufficient moisture to enable them to germinate. The spores will grow as soon as ripe, that is as soon as the mass containing them turns black, and they will also retain their vitality for a year or two in case conditions for growth are not favorable.

It is evident from this that neither the time of planting nor the previous condition or treatment of the seed will have any effect upon the amount of smut in the crop. It is equally evident that meteorological conditions will have decided influence. Two things can be done to decrease smut in corn. The growing crop can be sprayed with a suitable fungicide and the entrance of the smut into the plant prevented. That this can be made effective is shown by experiments at the Indiana station. The other, more convenient but less thorough, method, is to gather and destroy the smut, and thus eventually rid the fields of it.—(*Bull. Ind. Station.*)

Antidromy and Crossfertilization.—I have been much interested in Dr. Macloskie's article on Antidromy in the November number of THE NATURALIST. It reminds me of some observations which I made several years since while investigating the subject of crossfertilization. They will be found recorded in the same journal August, 1880. A suggestion is ventured as to the possible cause of it in the flowers of *Saxifraga sarmentosa* on pages 573 and 574 of that number. In that case it seems to have little or no value in aiding crossfertilization.

Other cases, however, have been noted where it seems probable that it may be of essential value in that direction, viz., in *Solanum rostratum* and *Cassia chamaecrista*. They show lateral asymmetry, by which the pistil is on opposite sides, in successive flowers of a cluster. These plants will be found described also in THE AMERICAN NATURALIST, April, 1882.

It may be an item of general interest, also, that the features of the flowers there pointed out are so remarkable as to have attracted the attention of Darwin. He addressed a letter of congratulation and inquiry to the writer with his characteristic candor and cordiality. It may have been the last letter from that illustrious hand, for he lay cold

in death before the missive had reached its destination. He called attention also to the fact that he had observed similar asymmetry in *Mormodes ignea* and had similarly used the terms "right handed" and "left handed." The fact is published in his "Fertilization of Orchids." —J. E. TODD.

University of South Dakota, Vermillion, S. D., Dec. 2, 1895.

VEGETABLE PHYSIOLOGY.

Water Pores.—Dr. Anton Nestler contributes an interesting "Kritische Untersuchungen über die sogenannten Wasserspalten" (pp. 38, pl. 2) to Band LXIV, No. 3 of *Nova Acta d. Ksl. Leop.-Carol. Deutschen Akad. d. Naturforscher*. The term "water pore" was introduced by DeBary to designate a mechanism supposed to be distinguished from ordinary stomata by (1) Presence of liquid water, at least at times, in the substomatic opening; (2) Rigid guard cells; (3) Often very considerable differences in form and size; (4) Location near the edge of the leaf in the teeth over the end of a vascular bundle. The following subjects are considered in this paper: Previous literature; development of the water pores; structure, number, and size; rigidity of the guard cells; plants destitute of water pores. Dr. Nestler shows that water pores originate from stomatic mother cells in the same way as ordinary stomata (48 species of Ranunculus were examined and also plants of many other families); that while water pores sometimes exceed ordinary stomata in size they are quite as often of the same size or smaller, and frequently show plain transitions into the latter; that rigidity of the guard cells is not always present in the water pores nor always absent in ordinary stomata; that water pores sometimes discharge vapor of water; and, finally, that the ordinary stomata sometimes, and probably often, excrete liquid water (over the whole upper surface of the leaf in *Vicia Faba*).—ERWIN F. SMITH.

Biology of Smut Fungi.—The third part of Dr. Brefeld's Smut Fungi (Heft XII of the *Untersuchungen*) contains 140 pages of quarto text and 267 figures packed into 7 lithographic plates, the crowding together of which makes difficult the comparison of text and figures. All told 13 genera and 64 species are described, of which latter 22 are reckoned as new. The germination of the smut spores is figured for most of the species as well as described. The descriptions are long and include a wealth of biological detail drawn from the behavior of the

various forms in Nährlösung. Two new genera are established: Arthrocoidea, founded on two old species (*Ustilago subinclusa* and *U. carycis*), separated from Ustilago by peculiarities of germination, and Ustilaginoidea, a most peculiar genus, founded on Patouillard's *Tilletia oryzæ* and on a new species found by Möller on *Setaria Crus-Ardeæ* in Brazil. Material for the study of the fungus on rice was obtained from Barclay in India. This fungus which causes a swelling of the ovaries of the rice plant to several times the normal breadth of the grain and which has the external appearance of a smut, has nothing to do with Tilletia, but seems to belong to some other group of fungi. Its principal peculiarities are (1) the production of a large number of smut-like spores on the outer part of the transformed grain, the interior of the same being occupied by a hard mass of nonsporiferous hyphæ suggesting an immature sclerotium; (2) germination in a manner totally different from that of any other smut spores and resembling that of some Ascomycetes, i. e. by the development of a much branched septate mycelium which, in dilute Nährlösung, bears succedaneously on the ends of the hyphæ, small, oval, colorless, nongerminating conidia, and in concentrated Nährlösung omits these conidia and develops in their stead and also anywhere on the walls of the hyphæ, sessile dark greenish-black, echinulate, thickwalled spores one in a place or sometimes two together, one above the other. In the species received from Brazil most of the dark spores had fallen off and the development of the central mass of hyphæ had proceeded a step further, being changed into a true sclerotium with a black rind and an internal thickwalled white pseudoparenchyma. Additional facts are promised as soon as these sclerotia can be induced to germinate. The descriptions are followed by a discussion of the relationship of the smuts to each other and to other fungi. A full account of culture methods and some additional notes on fungi are promised for Heft XIII to appear soon.

Incidentally Dr. Brefeld pays his compliments to the perfunctory grinders out of species: "The accidental circumstance that the all naming Patouillard has given to the fungus on rice the name *Tilletia oryzæ* shows once more how worthless are the namings of a spore material without the developmental history. The latter shows that in Patouillard's supposed *Tilletia oryzæ* we have to do not with a Tilletia and not even with a smut fungus but with a form out of the highest group of fungi." This is quite to the point. The labors of the "all naming" mycologists of the past have filled this part of systematic botany with a mass of rubbish mountain high, and still the brave work goes on, exactly as if it were not known that fungi are exceedingly

variable organisms, or that it is possible by holding on to the old notion of fixity of species to make half a dozen new ones out of the product of a single spore by a little variation of the substratum, or even without the latter device by drawing up separate descriptions of old and young and large, small and medium sized spores. Is it not indeed time we should have a reform and begin to *reduce* the number of species by carefully studying those which have been badly described (by far the larger number), learning their life history and the extent of their variability under ordinary conditions, and throwing out the synonyms? This method carefully applied would unquestionably reduce the number of so-called species of fungi and bacteria nearly or quite one-half. This must necessarily form a large part of the work of the next generation of mycologists, and no one familiar with the ground can doubt that the task of properly classifying these plants would be immensely easier if half the descriptions had never been written.—ERWIN F. SMITH.

Function of Anthocyan.—The following is an abstract of a short paper by Prof. Leopold Kny, of Berlin, Zur physiologische Bedeutung des Anthocyans, published in *Atti del Congresso Botanico internazionale di Genova*, 1892 (pp. 135–144). The name anthocyan has been given to a coloring matter occurring in the vegetative and floral organs of many plants in numerous transitional shades from red through violet to blue. It occurs dissolved in the cell sap and is sensitive to acids and alkalies, changing from one shade of color to another as they are used. It is probable that several different substances have been included under this term, for while in most plants these colors appear only on exposure to light, especially bright sunshine, in others they appear just the same in total darkness, e. g. in the perianth of *Tulipa gesneriana*, *Crocus vernus*, and *Scilla siberica*, the inner tissues of the root of the red beet, and the inner leaves of the red cabbage. In case of the floral organs anthocyan undoubtedly serves to make them conspicuous to insects, etc., but for the most part it can have no such function in the vegetative organs. Its use to these parts of the plant has been explained in three different ways. (1) When young leaves and stems either from seedlings or from buds take on a distinct red or violet color and subsequently lose it wholly or in part, it is but a step to the hypothesis that this color has been developed for the protection of the chlorophyll from injury by light. It is explained in this way by Kerner von Marilaun. On this supposition, it is difficult to understand how many young shoots get along without it, e. g. species of Iris, the young leaves of which are bright green. As proof, Kerner makes

prominent the abundance of anthocyan in many alpine plants as well as the fact that when a species grows on the plains as well as in the mountains it is in the latter locality that the vegetative and floral organs show an inclination to become red with anthocyan. (2) In cases where the cells holding the anthocyan are on the under side of the leaf, the upper side being pure green (*Cyclamen europœum, Hydrocharis Morsus ranæ*) the lightscreen hypothesis naturally falls to the ground. Here there is every reason to believe, according to Kerner, that the light rays which would otherwise pass out of the plant and be lost are converted into heat rays in passing through the cells containing anthocyan. In conformity with this hypothesis we find that the leaves of trees and shrubs which are lifted up from the soil and have other green leaves below to catch the filtered light, are never violet on their under surface, while, in very leafy under shrubs, only the lowermost leaves next the ground are provided with anthocyan. Another indication of the warming influence of anthocyan is its abundance in alpine plants, as already mentioned, and its frequent development in the perennial leaves of other plants during the winter season (*Sempervivum tectorum, Ligustrum vulgare, Hedera helix, Mahonia aquifolium*) the leaves being enabled thereby, in sunny winter days, to break up carbon dioxide even at relatively low temperatures. (3) There are, however, a series of facts going to show that the preceding hypotheses are not sufficient to explain all cases. On full grown shoots of many herbs and woody plants the sunny side of the internodes frequently becomes red while the opposite side remains nearly or quite pure green (*Salix* species, *Polygonum fagopyrum*, and many other plants). The same difference is frequently observed on petioles, the red color being not rarely prolonged into the midrib and its branches. These facts lead to the conclusion that the screen of anthocyan may have some use in connection with the breaking up and translocation of plastic substances through the vascular system. This is also indicated by the fact that when the roots of willows and other plants grow down from a bank into the water and are subject to direct sunlight they become red on the exposed surface. Pick considers the anthocyan screen as a means of bringing about the outward movement of starch in large quantities without seriously disturbing the assimilatory activity of the chlorophyll bodies. Some effort has been made to demonstrate this third view, but so far as known, no one has tried to establish the first two by means of experiment. The following experiments were, therefore, undertaken to fill this gap. (1) *Does anthocyan protect chlorophyll from the destructive action of light?* Owing to the manifest difficulty of dealing directly with the chlorophyll bodies the experiments were made

with an alcoholic solution derived from grass leaves. Two beakers were filled with this green solution and placed in tin chambers with blackened inner walls but having on one side a quadrangular opening with strongly projecting edges for the entrance of light. In front of each opening was placed a parallel walled glass vessel 196 millimeters high, 93.5 mm. wide and 40 mm. thick. Into one of these vessels red beet juice was poured and into the other white beet juice, both filtered and of the same specific gravity. The result was decisive. The light which passed through the anthocyan solution discolored the chlorophyll much less rapidly than that which passed through the colorless solution. (2) *Does anthocyan convert the light rays into heat rays?* Experiments were made with the foliage of green and red leaved varieties of the following species, viz. *Fagus sylvatica, Corylus avellana, Berberis vulgaris, Acer platanoides, Brassica oleracea, Dracæna ferrea, Canna indica;* with decoctions of white and red beets; and with the petals of a white and a red rose. Exactly weighed quantities of the leaves, etc., were placed in the parallel walled glass vessels already mentioned, thermometers were then plunged into the center of the mass, and the vessels were exposed to the action of direct sunlight filtered through a nearly saturated alum water screen 4 cm. thick (to absorb the heat rays). In most of the species (*Dracæna ferrea* and *Canna indica* gave contradictory results) the ability of anthocyan to convert light rays into heat rays seems to have been demonstrated conclusively. In one to two minutes in favorable cases there was a rise of temperature in the vessels containing the red leaves, the maximum difference amounting to as much as 4°C. As soon as the sun was covered by a cloud there was a noticeable fall of temperature in both vessels, and when the cloudiness lasted 10 to 20 minutes the temperature became the same or nearly the same in both vessels. Subsequently an effort was made to determine whether the different light rays of the solar spectrum behaved differently. For this purpose three vessels containing, in turn, red leaves of several species of plants were exposed to direct light under the following conditions; the light entering one vessel was filtered through the alum solution, that entering another was filtered through a screen of sulfuric-copper-oxide-ammonia, that entering the third was passed through a solution of bichromate of potash, it having been determined in advance spectroscopically that the two colored screens divided the spectrum in about the middle of the green. Under these conditions the rise of temperature was less behind the blue screen than behind the orange one, and less behind the latter than behind the alum screen. A consideration of the third supposed function of anthocyan is left by Dr. Kny for a subsequent paper.—Erwin F. Smith.

ZOOLOGY.

A posthumous paper on Myxospridia by M. Prosper Thélohan has recently appeared prefaced with a short account of the authors's scientific career by E. G. Balbiani. The Memoir, intended as a thesis for the degree of Doctor of Science, while complete in the essential parts, lacks the final chapter in which the author intended to indicate the relations of the different genera and families of the Myxosporidiea.

Briefly stated, Myxosporida are parasitic Sporozoa found living in certain fishes, batrachians and reptiles. They have also been observed living in various arthropods, notably spiders and crustaceans. Certain families are limited to vertebrates host: Myxobolidæ and Chloromyxidæ. It is to the latter forms that the author devotes his paper.

It has long been known that the Myxosporida of the vertebrates assume two forms; one, a small ameboid body swimming free in the liquid which contained in certain organs, chiefly the gall and urinary bladders, and a second form which is found distributed in compact tissues, like the connective tissues and the muscles. In either case they may be harmless to the host, or on the other hand, give rise to grave disorders, resulting in the death of the animal which they have invaded.

The free swimming species are variable in form, the most common one being that of an elongated cone the base of which corresponds to the anterior extremity; others are almost spherical. It is, however, difficult to decide upon a definite species form, since each individual exhibits such extraordinary polymorphism. The organisms found in the tissues are generally spherical.

Ordinarily these parasites are colorless, but yellow ones have been seen, and a few green ones are reported.

In dimensions, as in form, there is great diversity. The free swimming species are from 10 or 12μ. in diameter to 5 mm. in diameter.

Reproduction is accomplished by sporulation, and, probably, also by fission. The protoplasmic body of the Myxosporida is plainly differentiated into a peripheral zone, *ectoplasm*, surrounding the central sarcode, *endoplasm*. The former functions as a protection for the latter and, also is capable of putting out pseudopodia which act as organs of locomotion or fixation. These pseudopodia are localized in certain species, in others they appear at random. They take no part in the phenomena of nutrition.

The endoplasm of young individuals appears homogeneous, but in older ones there are found, in some cases, certain products of differentia-

tion, among which the author distinguished, fatty globular masses and rhombohedral crystals of hæmatoidin. In others, there are vacuoles, containing protoplasmic matter which differs from the rest of the endoplasm. It is in the endoplasm also that the nuclear elements are found, often in great number, around which the spores develop. The author traces the development of these spores, describing minutely the various stages of growth. Upon arriving at maturity they remain enclosed in the endoplasm for a varying length of time. When set free it seems to be connected with the destruction of the protoplasm which persists in the mother organism after the formation of the spores. The free-swimming species are expelled from the host either with the fæces or the urine, but the ones imprisoned in the tissues continue where they are until set free by the death and subsequent decay of the tissues of the host. The spores rarely germinate in the old host, never in any exterior medium, but stay dormant until chance provides them a new host.

As to the food habits of the Myxosporida, M. Thélohan observations are to the effect that they imbibe nourishment from the fluids in which they live. In no case did he see food particles ingested.

The following classification of the Myxosporida was proposed by the author in 1892, and his subsequent researches confirms the distinguishing characters.

Spores	form variable	no vacuoles in the plasma.	2 capsules.	I. Myxidiidæ.
			4 capsules.	II. Chloromyxidæ.
		1 vacuole which colors a reddish brown by iodine.		III. Myxobolidæ.
	pyriform, a single polar capsule, not easily seen, with a pointed extremity; a clear vacuole, not colorable with iodine, at the larger end.			IV. Glugeidæ.

Myxidiidæ contains 6 genera with 25 species; Chloromyxidæ has 1 genus, 6 species; Myxobolidæ 2 genera, 14 species; Glugeidæ 3 genera, 16 species.

The Segmentation of the Hexapod Body.—In a recent paper[1] giving the results of work upon the early stages of certain of the Orthoptera, Dr. Heymons gives the whole number of segments in the Hexapod body as twenty-one, of which six form the head; three, the

[1] Anhang. Abh. K. preuss. Akad. Wiss., Berlin, 1895.

thorax; and twelve, the abdomen. At some time during the development of the insect, appendages are present upon all except the first, third and twenty-first segments. The frons, clypeus, labrum and compound eyes are parts of the first segment. The second segment bears the antennæ, the fourth the mandibles, and the fifth and sixth the two pairs of maxillæ. The hypopharynx does not belong in the series of appendages but is formed by a folding of the ventral portions of the fourth, fifth and sixth segments. The cerci, contrary to the views of some authors, are the true appendages of the twentieth (eleventh abdominal) segment. Considerable emphasis is laid upon the similarity between the first and twenty-first segments, in their relations to the openings of the alimentary canal, in being free from appendages, in the lateral position of their ganglia and in the relative changes of the appendages of the adjoining segment. Concerning the position of the genital openings, Heymons reiterates his former opinion that they may belong primitively to the tenth segment, their position in the ninth being a secondary development.—G. M. WINSLOW.

The Coxal Glands of Thelyphonus caudatus.—In a brief note in the Zoologischer Anzeiger,[1] Dr. Theo. Adensamer adds a few facts to complete Sturany's work on the Arachnoidea. The two glands occur between the gastric cœca and the muscles, and extend as unbranched and unlobed sacs to the abdomen. From the anterior end of each extends a simple duct to the coxæ of the first pair of legs through which they open. A thin chitinous intima was distinguished in the ducts. An histologically differentiated portion of the gland corresponding to Lankester's medullary substance and Sturany's Marksubsanz was not found.

The following table shows the location of the openings of the glands in the several groups:

Limulus, openings in the 5th appendages.
Scorpio, openings in the 3d pair of legs = 6th appendages.
Pseudoscorpionidea, openings in the ? = ?
Thelyphonus, openings in the 1st pair of legs = 3d appendages.
Araneida:
a. Tetrapneumous, openings in the 3d pair of legs = 5th appendages.
b. Dipneumous, openings in the 1st pair of legs = 3d appendages.
Phalangida, openings in the 3d pair of legs = 5th appendages.
Acarina, openings in the ? pair of legs = ? —F. C. K.

[1] XVIII, p. 424.

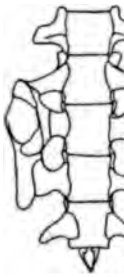
Cross Fertilization and Sexual Rights and Lefts Among Vertebrates.—The November number of this journal, page 1012, under the title "Sexual Rights and Lefts," called attention to sexual peculiarities I had recently discovered in certain Cyprinodonts. At that time no satisfactory explanation of the purpose or origin of the strange conditions offered itself. At present I would like to note in these pages what upon further consideration appears to me the best solution of the problem. Additional study has satisfied me that the sexual conditions in the genus Anableps prevent close "inbreeding," or, in other words, they secure cross fertilization. What in certain plants is attained by means of short stamens with the long ones is in these fishes realized by sinistral and dextral males and females. This is a view in the case of Anableps that brings us in face of probable benefit from the novel features, and of the possible causes of their evolution. As bearing on the inception of the dextral and the sinistral peculiarities we must consider the habit possessed by so many of these fishes of swimming in pairs, side by side, a habit that induced Professor Agassiz to name one of the genera *Zygonectes*, that is yoke swimmers. The acquisition of more or less of a dextral or of a sinistral tendency would not be at all unnatural in each of a pair habitually swimming side by side in the same relative positions to one another. It may be that cross fertilization will afford an explanation of conditions somewhat similar among molluscs.

While writing of matters concerning the publication "The Cyprinodonts," it should be mentioned, as kindly pointed out to me by Dr. A. Smith Woodward of the British Museum, that the name of one of the new genera, *Glaridodon*, was recently preoccupied among fossils, and it may be well here to discard that name (p. 40) for the term *Glaridichthys*.—S. Garman, Cambridge, Mass.

Abnormal Sacrum in an Alligator.—Among a lot of young alligators procured from New Orleans for the University of Chicago one in which the skeleton was prepared, showed a very peculiar variation in the pelvic region there being three instead of two sacral vertebræ.

There are as usual 24 presacral vertebræ. The 25th has the sacral ribs inclined backwards and becoming slender. The 26th has strong thick ribs, and the 27th, the first caudal in normal specimens, has also well developed ribs articulating strongly with the ilium. The 27th is seemingly biconvex. The first chevron is attached between the 28th and 29th and is, therefore, in the normal position as regards the serial number of the vertebræ, but is attached to the first vertebræ the last sacral instead of the second. The whole pelvis has migrated backwards one

vertebræ, the first true sacral tending to become a lumbar and the first caudal has become a sacral. The two side are strikingly symmetrical. The figures giving views from above and below are natural size and include the 24th–28th vertebræ.



I

II.

The other known cases of variation in the sacrum of Crocodilia are, as far as I am aware, as follows: Rheinhardt[1] examined 11 specimens and found 3 abnormal.

1. *Alligator sclerops* Schn.: Last lumbar become a sacral; 23 presacrals.

2. *Crocodilus acutus:* 3 sacrals, 3 plane-convex, 1st caudal concave-convex and bearing a chevron, thus the first caudal has become a sacral, 23 presacrals.

3. *Crocodilus acutus*: First caudal has become a sacral, 24 presacral.

Baur[2] reported two cases.

1. *Gavialis gangeticus:* 25 presacrals. One intercalated between the 9th and 10th.

2. *Alligator mississippiensis*: Last lumbar become a sacral, showing on one side a small sacral rib and which does not reach ilium, 23 presacrals.

Baur[3] reported three cases.

1. *Crocodilus acutus:* A specimen in the museum at Cambridge, Eng. shows on the right side of the 25th vertebra a strong and separate rib, on the left side the rib is smaller and coössified with the centrum. The 26 shows typical sacral ribs. The 27th shows on the left side a

[1] (Anomalier i Krydsvirvlerne hos Krokodelerne, Copenhagen, 1873, and Sur les anomalies des vertèbres sacrées chez les crocodilieus. Jul. de Zoologie T. III, No. 4. Paris, 1874.)

[2] Zoologischer Anzeiger, IX Jahrg., No. 238, 1886. Osteolog. Not. über Reptilen.

[3] (Zoolog. Anz. XII, Jahrg., No. 306, 1889. Revision meiner Mittheilungen in Zoologisher Anzeiger, mit Nachträgen.)

strong free rib and on the right side a weaker rib but free. The 28th biconvex.

2. *Crocodilus acutus*: Two specimens in the Royal museum at Leiden have only 23 presacrals.—E. C. CASE.

The Polar Hares of Eastern North America, with Descriptions of New Forms.—In 1819 Captain John Ross, in the fourth Appendix of the second (octavo) edition of his "Voyage of Discovery" in Baffin's Bay, described a hare which he procured in Baffin Land, in latitude 73° 37′.

To this animal he gave the name "*Lepus arcticus* Leach," stating at the end of his description that "Dr. Leach thinks it to be very distinct from the common White Hare of Scotland (*Lepus albus* Brisson) and equally so from the *Lepus variabilis*, Pallas." Ross then makes a reference to "Appendix No. V," of the same volume, which he evidently supposed would contain Leach's description of the same animal. Leach's chapter on the "New Species of Animals" obtained by Ross, however, does not come in appendix number five but is part of the same appendix in which Ross' description appears. It is on page 170, while Ross' description is on page 151. Leach evidently described the same specimen which Ross had in hand, but gave it the name *Lepus glacialis*. Owing to its precedence in paging, Dr. J. A. Allen[1] rightly adopts the name *arcticus* for the American Polar Hare, *glacialis* of Leach becoming a synonym.

The question has been raised by my friend, Mr. Outram Bangs, whether Ross, and not Leach, should have credit for the name *arcticus*. We may justly infer from Ross' description that he intended that Leach should have this credit and that he published it with such intention. He must have consulted with Leach about its relations to the European and Scottish Hares and quotes Leach in his diagnosis, using, without doubt, the specific name then suggested by Leach. The fact that Leach gave it another name does not affect the status of the one given by Ross, nor weaken Leach's claim to it. From the present custom, not definitely formulated in our American Ornithologist's Union's canons of nomenclature, I see, however, no alternative but to call the Baffin Land Hare, *Lepus arcticus* Ross.[2]

[1] Mon. N. Amer. Rod., 1877, p. 288.

[2] Some authorities prefer that sole credit for the name of a species be given to the person to whom the original publisher of that name ascribes the origin of the name, writing it in this case *Lepus arcticus* Leach. The A. O. U., with one (or two ?) exceptions, adopts the reverse rule in their check list of birds, and would make it read *Lepus arcticus* Ross. Neither method does justice either to the public or to

Dr. J. A. Allen (l. c.) concludes that the American Polar Hare is not specifically separable from the European *L. timidus* (=*variabilis* Auct.), and the deficient material which he had for examination at that time probably justified such a verdict as the safest one, especially when we consider the standard of species and varieties adopted at that date by American mammalogists. Through the kind liberality of Messrs. G. Brown Goode and F. W. True of the Smithsonian Institution, and of Mr. Outram Bangs of Boston, I have been favored to examine, in connection with the specimens in the Academy of Natural Sciences of Philadelphia, an unusually large series of skins and skulls of the Polar Hares of America and northwestern Europe. The results of this study, so far as they relate to the Polar Hares of eastern North America, and Scandinavia may be summed thus briefly.—

1. Lepus timidus L. Scandinavian Polar Hare.

Type locality (hypothetically restricted), Southern Sweden.

Nasals nearly or quite reaching to anterior vertical plane of premaxillaries. Posterior frontal swelling on a plane with the postorbital processes. Upper incisor with transverse sectional diameter greater than the longitudinal diameter; the chord of the arc of its exposed surface (with skull, minus mandibles, resting on a plane horizontal surface) is vertical; the radius of the arc described by the incisors is one-eighth ($\frac{12}{100}$) of the basilar length of skull; their inner faces indented by a deep broad sulcus and they are rooted on the premaxillaries at or slightly anterior to the inferior maxillo-premaxillary sutures. Roots of lower incisors extending to base of $\overline{\text{pm. 1}}$.

Summer pelage; above blackish brown, sprinkled with gray; ears darker, but not black, tail white, dark above.

2. Lepus arcticus " Leach," Ross. Baffin Land Polar Hare.

Type locality, lat. 73° 37′, northern Baffin Land, southeast of Cape Bowen.

Size larger (?) than *timidus*, with relatively smaller and wider skull and shorter ears. Skull of the same type as *timidus*, with the following differences: Nasals, rostrum and incisive foramina relatively

those personally interested. I suggested (Proc. Acad. Nat. Sci., Phila, 1895, p. 395), that both the publishing and the manuscript or verbal authority for such names should be indicated. My friend, Witmer Stone, has suggested an improvement on my formula which I heartily endorse, viz.. that instead of " *Rana clamitans* Bosc., Mss., Sonn., Latr." (l. c.), it should read *Rana clamitans* " Bosc.," Sonn. & Latr., and the Baffin Land Hare would read *Lepus arcticus* " Leach," Ross. This comports far better with our motto that, " Zoological nomenclature is a means, not an end, in zoological science."

shorter and broader, the incisive foramina never reaching to middle of pm. 1. Palatal bridge longer than width of postpalatal fossa. Supra orbital processes of frontals deeply notched anteriorly, upraised and widely flaring. Frontals, at their posterior constriction, remarkably tumid, their anterior plane greatly depressed.

Summer pelage (fide Ross and Leach (l. c.) and Sabine[3]), white, "The back and top of the head are sprinkled with blackish brown hair which is banded with white, the sides of the neck are covered with hairs of the same color, interspersed with white. The extreme tips of the ears are tipped with black."—Leach. "In some of the full-grown specimens, killed in the height of summer, the hair was a grayish brown towards the points but the mass of fur beneath still remained white, the face and front of the ears were a deeper gray."—Sabine.

In south Baffin Land, as evidenced by a specimen from Cumberland Gulf, the type form intergrades into the following subspecies:

3. Lepus arcticus bangsii Rhoads, subsp. nov. Newfoundland Polar Hare. Type locality, Codry, Newfoundland. (Diagnosis as given below.)

4. Lepus grœnlandicus Rhoads, sp. nov. Greenland Polar Hare. Type locality, Robinson's Bay, Greenland. (Diagnosis as given below.)

Lepus arcticus bangsii,[4] subsp. nov. Newfoundland Polar Hare.

Type, Ad. ♀, No. 3752; Col. of E. A. & O. Bangs. Collected by Ernest Doane at Codry, Newfoundland, Aug. 3d, 1895.

Description.—Size equal to *L. timidus* L., of Southern Sweden, with shorter ears, shorter and broader skull, nasal bones and incisive foramina, weaker dentition and narrower frontal breadth anterior to the supraorbital processes.

Adult summer pelage: entire back and upper sides, including neck, shoulders and outer surfaces of thighs, uniform dark grizzled gray, faintly suffused with tawny. A pinch of hairs from near middle of back shows the following color pattern: under-fur fine, tawny-white basally, becoming tawny at distal end; over-fur white or black at base in about equal proportions, the coarser black-based hairs black throughout, the finer white-based hairs with terminal half black, interrupted by a subterminal band of white or pale tawny. Lower head

[3] Suppl. Appx., Parry's Voy., 1824, pp. 187–188.

[4] Named for Mr. Outram Bangs, who has done so much in making known the mammal fauna of Newfoundland, and who has there collected the finest study-series of Polar hares that can be found in this country.

(including chin), lower neck, nape, forebreast to forelegs, lower sides, edges of thighs and rump, dark plumbeous gray, flecked with very long, slender, white hairs. Lower breast, belly, vent and tail white, bordered by a nearly clear plumbeous edging which separates the ventral from the abdominal regions and joins the dark rump along the inside of thighs. Inner anterior border of hams, sides of hind feet and toes, and lower surfaces of forelegs, white, thinly intermixed with leaden hairs. Outer surfaces of fore and hind legs and superior surfaces of the feet, tawny gray. Ears and space between them, black, becoming grayish at base and with a narrow whitish outer posterior margin from near base to tip. Upper head, including cheeks and nose, grizzled buffy gray, appreciably lighter than the gray shades of the back. Eyelids whitish, edged with black. Whiskers weak and sparse, white and black in equal proportions, the longer black hairs tipped with white.

Winter pelage (No. 1187, Ad. ♀, Col. of E. A. & O. Bangs. Bay St. George, Newfoundland, Mar. 1, 1895): Entire pelage, exclusive of ears, white. Extreme tips of ears black, the median anterior borders of ears grayish.

Measurements (of type).—Total length, 626 millimeters; tail vertebræ, 63; hind foot, 160; ear (from crown), 85. Skull: total length, 97; basilar length, 76; greatest breadth, 48.2; anterior frontal constriction, 23; length of nasal (longest diagonal), 40; greatest breadth of nasals, 22; alveolar breadth of upper incisors, 9; greatest length of mandible, 76; greatest width of mandible, 47.

The above measurements both of body and skull are a very fair average of the dimensions of five adults taken for Mr. Bangs in Newfoundland by the same collector, Mr. Ernest Doane. Summer specimens from northern Labrador are inseparable from those taken in the same month in Newfoundland. A summer series from Great Slave Lake may show the existence of another race of *arcticus* in that region.

LEPUS GRŒNLANDICUS sp. nov. Greenland Polar Hare.

Tpye, No. 1486 ad. ♂. Col. of Acad. Nat. Sciences, Philadelphia. Collected by Chas. E. Hite at Robinson's Bay, North Greenland, Aug. 2, 1892, for the Peary Relief Expedition.

Description.—Size larger than *L. timidus* L. of Sweden, with radically distinct coloration and incisor dentition.

Adult summer pelage, white, suffused with light tawny and sparingly sprinkled with gray on upper head and ears. Back with scattering black and gray hairs. Tip of ears black. Tail, sides and lower sur-

faces pure white. Half grown young in July and August like adult, but darker, owing to greater abundance of colored hairs and the leaden under fur. Appearances of young and old at a distance at all seasons, white.

Winter pelage, pure white throughout, except the tips of ears, which are black.

Skull with rostral portion anterior to $\underline{\text{pm. 1}}$, relatively much longer and more attenuate, owing to the outward prolongation of the premaxillaries and the small calibre of incisors. Upper and lower incisors very long, produced and slender, their transverse diameter being less than the longitudinal. Upper incisors describe the arc of a circle whose radius is one-fifth ($\frac{20}{100}$) the basilar length of the skull. The chord of their exposed arcs (with cranium, minus mandibles, resting on a plane horizontal surface) forms an angle of 45° to the horizontal plane. Face of upper incisors multistriate, the normal sulcus peculiar to all other members of the genus being so filled with dentine in adult *grœnlandicus* as to obliterate the depression, presenting an even, rounded, enameled contour marked with three minute striæ.

Roots of upper incisors based on the maxillaries and reaching back nearly half way from inferior maxillo-premaxillary sutures to $\underline{\text{pm. 1}}$. Roots of lower incisors extending to the base of $\overline{\text{pm. 2}}$.

Measurements (of type taken from dry mounted skin, relaxed): ear, from crown, 100 millimeters; hind foot, 145; tail vertebræ (dry), 50 ?

Skull: total length, 100; basilar length, 84.5; greatest breadth, 50; anterior frontal constriction, 22.5; length of nasals (longest diagonal), 41; greatest breadth of nasals, 20.5; alveolar breadth of upper incisors, 8.5; greatest length of mandible, 76; greatest width of mandible, 48.

Five skins, seven skulls, and one skeleton, all from North Greenland, comprise the Academy series of Greenland Hares, and all confirm the peculiar characters of this species as above given. I regret that more complete body measurements are not available. Average adult measurements of ear and hind foot are 100 millimeters for the former and 145 for the latter. The total length of an adult skeleton (ligamentous) is 519 millimeters, measured as in the flesh, from tip of nose to end of tail vertebræ.

It is possible that Spitzbergen and Iceland Hares are of the same type as those of Greenland. None of these have come into my hands. The Bavarian, Swiss, Scottish, Irish and Siberian representatives of *timidus* are also likely to prove separable, at least into definable races, already named. From what is known of Linnæus at the time of writ-

ing his tenth edition of the System, it is most fitting that the Polar Hare of Southern Scandinavia should be made the type of the *timidus* group, the Swedish Hares being those which would most naturally embody and form the source of his original diagnosis.

The writer is now preparing a more compendious revision, with illustrations, of the New World representatives of the *Lepus timidus* group, which will probably appear in a future number of the Proceedings of the Academy of Natural Sciences of Philadelphia.

—SAMUEL N. RHOADS.

ENTOMOLOGY.[1]

On Certain Geophilidæ Described by Meinert.—The Chilopoda of the Museum of Comparative Zoology were studied by Dr. Meinert, and the results published in a paper entitled "*Myriapoda Musei Cantabrigensis.*"[2] Many new species were described, but as no figures were given, identification is not in all cases easy, although the descriptions are of considerable length. With reference to the Geophilidæ, at least, there are certain misleading statements and unfortunate omissions. During a recent visit to Cambridge I had the pleasure of a very brief examination of the types of several of Dr. Meinert's species, and some long-standing curiosity was satisfied.

Geophilus georgianus Meinert.

According to Dr. Meinert this species has but a single pleural pore. For some years past I have had specimens from the South which agreed well with the description of this species, but had two pores. As this character is a very constant one, my determination was not made with confidence. The type of *georgianus* has, however, two large pores on each side concealed under the last ventral plate, so that the anomaly is disposed of. The pores are similar in structure and location to those of *G. rubens*.

Geophilus cephalicus Wood.

The specimens described by Meinert, and previously by Wood as *cephalicus* belong to *G. rubens* Say. I have examined the type in the British Museum. It is the most common geophilid in the northeastern states.

Geophilus urbicus Meinert.

No ventral pores could be made out. The sterna are uneven and the whole animal is very hairy. The form of the body, the armature

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] Proc. Am. Phil. Soc. XXI, pp. 161–233 (1885).

of the prehensorial legs and other strong similarities leave little doubt that this is a member of the genus Escaryus, as was conjectured when that genus was erected.[3] The anal legs were also strongly curved under as has been the case with all the specimens of Escaryus yet observed. That the differences enumerated between *E. phyllophilus* and *E. urbicus* can be maintained, is doubtful, for the Cambridge specimen is in rather poor condition, so that some of the characters ascribed by Dr. Meinert may easily prove to have been accidental.

Scolioplanes robustus Meinert.

The locality of this species was not known. I have collected what is evidently the same in central New York and southern Pennsylvania, and am unable to separate it from Sager's *Strigamia fulva*, the probable type of which I have seen in the Museum of the Academy of Natural Sciences at Philadelphia. The only difference between it and *bothriopus* and *robustus* seems to be that of size. The large specimens always show evidences of good living. The creatures are also constructed so as to be capable of considerable distention, besides being variable in size and number of legs, even in the same localities.

Scolioplanes parviceps Meinert.

The label in this bottle, probably in Meinert's handwriting is "*Scolioplanes parviceps* n. sp." The bottle also contains a label marked "*Strigamia bidens* Wood, N. A. loc.?" It is evidently to this label that Dr. Meinert refers when he says, (p. 226). "A specimen, which was said to be a type of Dr. Wood, was labeled '*Strigamia bidens* Wood.'" To thus rename a type specimen seems a remarkable proceeding, especially when the new name proposed has already been used in the same genus. Yet this is probably what Dr. Meinert proposed to do, for Mr. Henshaw kindly showed me a list of the collection, carefully made out in Dr. Meinert's handwriting, and in this the species is again given as new. That it did not so appear when the paper was printed, may have been the work of some American editor who knew of Wood's species and naturally supposed that the same was intended by Meinert.

Wood's *parviceps* is a Californian species, while *bidens* is found in the East. I have collected it in the vicinity of Washington. I had a specimen of *parviceps* at Cambridge with me to compare, but the difference was evident. There was no other specimen of *bidens* at hand, but the size, form of the body and other characters agree well with the eastern species.

[3] Proc. U. S. Nat. Museum XIII, p. 394 (1890).

Scolioplanes longicornis Meinert.

This species was looked upon by its author as the probable type of a new genus. The prehensorial claws are very long and slender, and the basal tooth very small. That it represents a new genus is well-nigh certain, but it would be idle to name it until drawings can be made.

Scolioplanes exul Meinert.

This is a large specimen with a strong general resemblance to large males of *fulvus* (*robustus*). The last pleuræ are without pores except close under the edge of the last ventral plate, where there is a large porose cavity. Anal legs with the claw minute, almost rudimentary, in this offering a strong contrast to the other American species known to me. The anal legs are also very robust, much stouter than a Californian specimen of *parviceps*.

Mecistocephalus breviceps Meinert.

The type specimen is minus the cephalic lamina and antennæ. There is another specimen labeled *breviceps*, but with no locality given. If the type was really collected at Nantucket the species must be very rare or local, for it seems not to have been found elsewhere.

Mecistocephalus heros Meinert.

It has been conjectured by Mr. Pocock that this species should be added to the long list of synonyms of *punctifrons*. I have never examined carefully authentic specimens of *punctifrons*, but the form of the prehensorial legs in the Cambridge specimen, especially the armature of the coxa is different from that of *Haase's* diagram of *punctifrons*. There is no distinct tooth, only a rounded prominence at the distal corner.

Himantarium indicum Meinert.

This specimen is in poor condition and has evidently been allowed to dry at some time in its history. The antennæ are distinctly attenuate. The ventral pores are in a posterior, transverse, subreniform area three or four times as broad as long. This area is scarcely depressed, but is quite definite. Pleural pores are not visible.

Himantarium tæniopse (Wood).

Ventral pores in a small, round, impressed, posterior area. No pleural pores visible, but they may be concealed under the very broad last ventral plate, as is the case in the following species.

Himantarium laticeps (Wood).

The ventral plates appear to be unusually long. The pores are located about two-thirds back, in broad, short, transverse areas. Three

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large pleural pores, subconcealed. There seemed to be no specimen of *Himantarium insigne* Meinert in the collection.—O. F. Cook.

Life-history of Scale Insects.—In an excellent account of the Scale Insects affecting deciduous fruit trees Mr. L. O. Howard discusses[4] the life-history of the Coccidæ as follows: In respect to life history, the family Coccidæ, which includes all of the so-called scale insects, is very abnormal. The eggs are laid by the adult female either immediately beneath her own body or at its posterior extremity. Certain species do not lay eggs, but give birth to living young, as do the plant lice. This abnormal habit is not characteristic of any particular group of forms, but is found with individual species in one or more genera. The young on hatching from the eggs are active, six-legged, mite-like creatures which crawl rapidly away from the body of the mother, wander out upon the new and tender growth of the tree, and there settle, pushing their beaks through the outer tissue of the leaf or twig and feeding upon the sap. Even in this early stage the male insect can be distinguished from the female by certain differences in structure. As a general thing, the female casts its skin from three to five times before reaching the adult condition and beginning to lay eggs or give birth to young. With each successive molt the insect increases in size and becomes usually more convex in form. Its legs and antennæ become proportionately reduced, and its eyes become smaller and are finally lost. As a general thing, it is incapable of moving itself from the spot where it has fixed itself after the second molt, although certain species crawl throughout life. The adult female insect, then, is a motionless, degraded, wingless, and, for all practical purposes, legless and eyeless creature. In the armored scales she is absolutely legless and eyeless. The mouth parts, through which she derives nourishment, remain functional, and have enlarged from molt to molt. Her body becomes swollen with eggs or young, and as soon as these are laid or born she dies.

The life of the male differs radically from that of the female. Up to the second molt the life history is practically parallel in both sexes, but after this period the male larva transforms to a pupa, in which the organs of the perfectly developed, fledged insect become apparent. This change may be undergone within a cocoon or under a male scale. The adult male, which emerges from the pupa at about the time when the female becomes full grown, is an active and rather highly organized creature, with two broad, functional wings and long, vibrating antennæ.

[4] Yearbook U. S. Dept. Agr., 1894.

The legs are also long and stout. The hind wings are absent, and are replaced by rather long tubercles, to the end of each of which is articulated a strong bristle, hooked at the tip, the tip fitting into a pocket on the hind border of the wings. The eyes of the male insect are very large and strongly faceted. The mouth parts are entirely absent, their place being taken by supplementary eye spots. The function of the male insect is simply to fertilize the female, and it then dies. The number of generations annually among bark lice differs so widely with different forms that no general statement can be made.

EMBRYOLOGY.[1]

The development of Isopods.—Last Winter when M. Louis Roule published a long paper in French on the development of an Isopod, *Porcellio scaber* Leach, it seemed advisable to present a rather full abstract in this magazine, for the benefit of those readers who would not see the original or who did not read French. That abstract appeared in February and contained, besides the descriptive account of the embryology, some interesting conclusions based on these results.

In the May number of the Journal of Morphology Dr. J. Playfair McMurrich publishes a long paper, illustrated with excellent figures, which is not at all reconcilable with M. Roule's views. It must be remembered, in comparing the two papers, that M. Roule studied a single species of Isopod, that he gives rather diagrammatic figures, and that his description of the segmentation, on which apparently the whole fabric rests, is of a very general nature.

Dr. McMurrich took up the work in 1890, hoping to make out the cytogeny of a Crustacean as Whitman had done for Clepsine, and as E. B. Wilson has later done for Nereis and other forms. This author's results rest then on a thorough study of the segmentation, and as he did not confine his attention to one form, but observed and figured the segmentation and early differentation in a number of Isopods, the paper is of especial interest.

The forms studied were *Jæra marina* Möbius (1873); *Asellus communis* Say; *Porcellio scaber; Armadillidium vulgare;* with some observations on *Cymothoa* and *Ligia*.

The segmentation is centrolecithal. The nucleus of the unsegmented ovum lies in a central mass of protoplasm surrounded by yolk, and

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

from the central protoplasm a network extends out through the yolk to a peripheral layer. It is possible to determine the second plane of division as that of the long axis of the embryo as has been shown to be the case in Nereis, Crepidula, and Umbrella. In discussing the segmentation, the author uses the term cell as a convenient one for the nucleated bodies of protoplasm which appear on division in the yolk, but he insists on the syncitial nature of the ovum to a late period.

The cleavage is apparently a spiral one, and results in what may be spoken of as a blastula stage, in which considerable differentiation has taken place. From the eight cell stage, especially in Jæra, it is possible to trace the history of different areas of this blastula to certain well marked cells. For instance, in Jæra, a cell at the posterior pole gives rise to the future Vitellophags, three cells immediately encircling it are the ancestors of the mesendoderm, while the ectoderm arises from the remaining four anterior cells. There are some interesting variations in this history in the forms studied, though the end result is practically the same. The author concludes with E. B. Wilson that, "cells having precisely the same origin in the cleavage, occupying the same position in the embryo, and placed under the same mechanical conditions may nevertheless differ fundamentally in morphological significance."

In connection with the segmentation Dr. McMurrich thinks that the existence of a syncitium up to so late a period in differentiation is of special interest in relation to the current disscusion of the cell-theory. The question is asked, "are we to believe that there is no continuity in Lucifer, between the blastomeres, notwithstanding that in all probability there was continuity in the ova of its ancestors?" In *Peripatus capensis* there is an approach to holoblastic cleavage associated with less yolk and still a syncitium results. This is regarded as supporting "the supposition that, even in such cases as Lucifer, there may be also a continuity of protoplasm, the separation into distinct spherules being only apparent."

It should be remembered that, however, plausible this argument is, of course the fact of continuity between the blastomeres of Lucifer or of other holoblastic ova still remains to be proven by direct observation.

The conclusion that "the existence of a syncitium is no bar to a certain amount of differentiation," certainly seems justified from the facts described for Jæra. Continuing this subject, such a syncitium is compared with the differentiation in certain protozoa, and a peculiar phenomenon in Porcellio is considered, where there is a precocious segregation of a portion of the cytoplasm which is to take part in the

formation of the blastoderm. This segregation is said to take place, "not in accordance with any previous location of a nucleus, but independently." Dr. McMurrich thinks that "this phenomenon seems to demonstrate that cytoplasmic differentiation may occur *independently of definite nuclear influence.*" He immediately adds, however, that "he, of course does not mean to assert that the nuclei may not possess a *coördinating or even a trophic action upon the cytoplasm,* but that they are directly responsible for the segregation or concentration seems to him an unwarranted assumption."

It is difficult to understand just what is meant by these statements. The remarkable concentration of the peripheral protoplasm of the ovum of Porcellio toward the definitive ventral side, independently of any previous location of nuclei, is notewothy. Does it, however, "demonstrate cytoplasmic *differentiation* independent of definite nulcear influence?" Can this *movement* of protoplasm, even toward a definite point, be correctly spoken of as *differentiation* and compared with the specialization in the cytoplasm of certain protozoa? Having in mind the condition of the ovum when this phenomenon takes place, is it not possible that the movement may be the result of nuclear influences from the center, acting through the central network on the periphal protoplasm?

Again, if the phenomenon demonstrates cytoplasmic differentiation *independent* of definite nuclear influence, why does the author add that, "he does not mean to assert that the nuclei may not possess a *coördinating* action upon the cytoplasm?" There seems to be a contradiction in these two statements, which may destroy the force of the argument. It should also be remembered that in Jæra, and in the other Isopods studied, there is a contraction of the blastoderm cells toward the ventral surface. Here the nuclei, as well as the cytoplasm, of the blastoderm are evidently directly involved. Perhaps the precocious segregation of cytoplasm ventrally in Porcellis is but an early appearance of this process. If it be admitted that the nuclei possess coördinating influences on the cytoplasm, how can it be claimed that in the case of the highly differentiated protozoa such influences were not active during the differentiation?

Another point discussed is the extent of external influences and their action, in holoblastic and in centrolecithal ova like those of Jæra. The conclusion reached from a review of the facts of segmentation is that "the cleavage form of Jæra is determined entirely by intrinsic conditions." The phenomena of segmentation "leave us no choice but to refer the vis essentialis which determines the direction of the Karyokinetic

spindle, and therefore, the cleavage form of Jæra, to the constitutional peculiarity of the ovum." "Holoblastic ova, the author believes, can not be excluded from the action of external forces, but the presumption is allowable, for several reasons, that even in these intrinsic forces are important." The assumption must consequently be made "that intrinsic forces reside in all ova, though they may be overshadowed by external influences in some cases."

It is important to examine the assumption which forms the foundation of this argument. The author quotes E. B. Wilson's conclusion that "cleavage forms are not determined by mechanical conditions alone," and assumes that by "mechanical conditions," Wilson means conditions extrinsic to the ovum. This can hardly be so, for it is necessary to include among the "mechanical conditions" influencing the cleavage of an ovum like that of Jæra, the presence in the cytoplasm of a great accumulation of food-yolk, (excessive in quantity when compared with that in holoblastic or meroblastic ova). It is true that this mass is within the ovum, and in so far "intrinsic", but its action is usually looked on as that of a foreign body, so to speak, which modifies and obscures the primitive phenomona of cleavage and differentiation as seen in holoblastic ova. Hence it is important to remember that Dr. McMurrich, in maintaining that "the cleavage form of Jæra is determined entirely by intrinsic conditions," must include the action of the nutritive mass. This would seem to weaken materially the position that extrinsic influences, (in the generally accepted sense as extrinsic to active cytoplasm), are excluded from action on the spindles of centrolecithal ova. The confusion seems to lie in the use of the word intrinsic to include, in the case of the ovum of Jæra, both inherent properties of the protoplasm, and secondary forces due to the presence of a body of nutritive material which is morphologically not a part of the protoplasm. Dr. McMurrich's conclusion, that "instrinsic forces reside in all ova", or preferably, as E. B. Wilson has just it, "cleavage forms are not determined by mechanical conditions alone," will probably be accepted as truth by most observers. However, I can not see that he has shown that "in Jæra we have practically a demonstration of the correctness of this view" of a more convincing character than is exhibited by holoblastic ova.

"The cleavage form of Jæra, is said to be, determined entirely by intrinsic conditions." A conclusion from which Dr. McMurrich sees no escape, after a review of the changes of position in the yolk assumed by the nuclei during segmentation. The Karyokinetic spindles then are regarded as entirely beyond the influence of forces external to the

ovum in such eggs as those of Jæra, and their direction, with consequently the cleavage form, is due without other alternative entirely to the constitutional peculiarity of the ovum. After carefully considering the evidence presented by Jæra and similar centrolecithal eggs, the assumption does not seem warranted, that they are any more removed from the influences of external forces, than are holoblastic ova. It may be true that it is difficult to *understand* how forces external to a centrolecithal ovum may affect the spindles within it, but many will find the same difficulty in the case of holoblastic ova. Does the great increase of yolk in a centrolecithal ovum remove the spindles from the action of the external world? I, for one, can not see that this necessarily follows, and hence do not see that the condition of segmentation in Jæra leaves us no escape from the conclusion that its cleavage form is determined entirely by intrinsic conditions.

Returning to the description of the embryo, it will be remembered that the germ-layers are already distinguishable in a blastula stage on the surface of the yolk in Jæra, and somewhat less distinctly in the other forms. Now the blastoderm cells gradually concentrate towards the ventral surface of the egg. This results in the mesendoderm and vitellophag cells being crowded beneath the surface in the form of a solid plug, and in the ectoderm of the ventral surface marking out a somewhat triangular area, the base of which lies anteriorly while the apex is posterior. This area is the Nauplius region. The rudiments of the eyes are placed anteriorly at the angles of the base, the appendages appear later along the sides, while the blastoporic plug of mesendodermal cells lies just under the posterior apical end. In a most interesting discussion of the formation of the germs layers in the Crustacea, the author concludes that the primitive Crustacea probably passed through a blastula stage which was filled with yolk, and in which a plug of cells migrated into the yolk to be later differentiated into mesoderm and endoderm. This is the condition exhibited by the Phyllopods (Samassa, 1893 and Bauer, 1892). Jæra, the Decapods and especially Lucifer are examples of precocious differentiation of the germ layers. The entire mesendoderm of Crustacea has a blastoporic origin, and is not (except in Decapods, where there are secondary phenomena) formed by delamination of extra-blastoporic region. The under layer of the latter regions is formed by a migration of cells from the blastoporic plug. In Armadillidium this is especially well made out. An interesting question is raised in regard to the mesenteron of Astacus. Dr. McMurrich suggests the probability that the yolk-pyramids do not form it, but eventually form mesodermic tissues, while

the mesenteron is really formed by cells of the entodermic plates. This interpretation would be more in line with what is known of other Crustacea. Some of the most interesting observations and conclusions of the paper are those concerning the development of the metanaupliar regions of the embryo. It is a remarkable fact that the Naupliar and more posterior metanaupliar regions are very sharply distinguished by differents methods of growth. Dr. Patten was the first to call attention to the fact of teloblastic growth in the ectoderm and mesoderm of Cymothoa. Dr. McMurrich has gone further, and in his comparative study, has made out in detail the character and limits of this method of growth in Isopods. While the Naupliar is formed as described, the metanaupliar regions, are the result of teloblastic growth in ectoderm and mesoderm, just as the metatrochophoral regions of Polygordius are due to a similar process. The author is inclined to regard these two instances of teloblastic growth as acquired independently. He thinks that in the Isopod "the development points back to a period where a free-swimming Nauplius occurred in the development of the ancestors of the group, the egg embryo being a nauplius." At such a time the metanaupliar regions were developed after hatching. Now, however, this posterior region is developed in Isopods before hatching, but it still retains the peculiar teloblastic method of development, and is sharply distinguishable from the Naupliar area.

There is unfortunately not space to describe this remarkable process. It is interesting to note, however, that, while the ectoderm of the metanaupliar regions arises from the successive divisions of a row of ectodermal teloblasts, the rythm of these divisions is not the same as that of the row of mesodermal teloblasts which lies beneath. The mesodermal teloblasts divide just 16 times giving rise to 16 transverse rows of mesoderm cells, "each of which rows is equivalent to a segment," as is proved on the appearance of appendages. The ectodermal teloblasts divide twice as many times.

Though these are the main points of the paper, a number of important observations and conclusions have been necessarily crowded out of this review. For instance, I have not touched on the processes of impregnation, the formation of membranes, the details of segmentation and differentiation, the formation of the digestive tract, the history of the vitellophages, or the development of certain organs.

—H. McE. Knower.

PSYCHOLOGY.

Consciousness and Evolution.—The quotation by Professor Cattell in SCIENCE, July 26, of Professor Cope's table (from the *Monist*, July, 1895) shows that he was equally struck by it with myself. Prof. Cope gives in this table certain positions on points of development, in two contrasted columns, as he conceives them to be held by the two camps of naturalists divided in regard to inheritance into Preformists and the advocates of Epigenesis. The peculiarity of the Epigenesis column is that it includes certain positions regarding consciousness, while the Preformist column has nothing to say about consciousness. Being struck with this I wrote to Professor Cope—the more because the position ascribed to consciousness seemed to be the same, in the main, as that which I myself have recently developed from a psychological point of view in my work on *Mental Development* (Macmillan & Co.). I learn from him that the table[1] is not new; but was published in the the 'annual volume of the Brooklyn Ethical Society in 1891;' and the view which it embodies is given in the chapter on 'Consciousness in Evolution;' in his *Origin of the Fittest* (Appletons, 1887).

Apart from the questions of novelty in Professor Cope's positions—and that Mr. Cattell and I should both have supposed them so can only show that we had before read hastily; I myself never looked into Professor Cope's book until now—I wish to point out that the placing of consciousness, as a factor in the evolution process, exclusively in the Epigenesis column, appears quite unjustified. It is not a question, as Mr. Cattell seems to intimate in his note referred to in SCIENCE, July 26, of a causal interchange between body and mind. I do not suppose that any naturalist would hold to an injection of energy in any form into the natural processes by consciousness; though, of course, Professor Cope himself can say whether such a construction is true in his case. The psychologists are, as Mr. Cattell remarks, about done with a view like that. The question at issue when we ask whether consciousness has had a part in the evolutionary process is, I think, as to whether we say that the presence of consciousness—say in the shape of sensations of pleasure and pain—with its nervous or organic correlative processes, has been an essential factor in evolution; and if so, further,

[1] This table is given in the issue of SCIENCE for July 26, p. 100. The three points from it which are taken up now are cited below.

whether its importance is because it is through the consciousness aspect of it that the organic aspect gets in its work. Or, to take a higher form of consciousness, does the memory of an object as having given pleasure help an organism to get that object a second time? This may be true, although it is only the physical basis of memory in the brain that has a causal relation to the other organic processes of the animal.

Conceiving of the function of consciousness, therefore, as in any case not a *deus ex machina*, the question I wish to raise is whether it can have an essential place in the development process as the Preformists construe that process. Professor Cope believes not. His reasons are to appear fully in his proposed book. I believe that the place of consciousness may be the same—and may be the essential place that Mr. Cope gives it in his left-hand column and which I give it in my *Mental Development*—on the Preformist view. I have argued briefly for this indifference to the particular theory one holds of heredity, in my book (Chap. VII.), reserving for a further occasion certain arguments in detail based upon the theory of the individual's personal relation to his social environment. The main point involved, however, may be briefly indicated now, although, for the details of the social influences appealed to, I must again refer to my book (Chaps. on 'Suggestion' and 'Emotion').

I have there traced out in some detail what other writers also have lately set in evidence, *i. e.*, that in the child's personal development, his ontogenesis, his life history, he makes a very faithful reproduction of his social conditions. He is, from childhood up, excessively receptive to social suggestion; his entire learning is a process of conforming to social patterns. The essential to this, in his heredity, is excessive instability, cerebral balance and equilibrium, a readiness to overflow into the new channels which his social environment dictates. He has to learn everything for himself, and in order to do this he must begin in a state of great plasticity and mobility. Now, my point, but briefly, is that these social lessons which he learns for himself take the place largely of the heredity of particular paternal acquisitions. The father must have been plastic to learn, and this plasticity is, as far as evidence goes, the nervous condition of acute consciousness; the father then learned, through his consciousness, from his social environment. The child does the same. What he inherits is nervous plasticity and the consciousness. He learns particular acts for himself; and what he learns is, in its main line, what his father learned. So he is just as well off, the child of Preformism, as if he had been the heir of the particular lessons of his father's past. I have called this process 'Social Hered-

ity,' since the child really inherits the details; but he inherits them from society by this process of social growth, rather than by direct natural inheritance.

To show this in a sketchy way, I may take the last three points which Professor Cope makes under the Epigenesis column, the points which involve consciousness, and show how I think they may still be true to the Preformist if he avail himself of the resource offered by 'Social Heredity.'

I do this rather for convenience than with any wish to controvert Professor Cope; and it may well be that his later statements may show that even this amount of reference to him is not justified.

1. (5 of Cope's table). "Movements of the organism are caused or directed by sensation and other conscious states."

The point at issue here between the advocate of Epigenesis and the Preformist would be whether it is necessary that the child should inherit any of the particular conscious states, or their special nervous dispositions, which the parent learned in his lifetime, in order to secure through them the performance of the same actions by the child. I should say, no; and for the reason—additional to the usual arguments of the Preformists—that 'Social Heredity' will secure the same result. All we have to have in the child is the high consciousness represented by the tendency to imitate the parent or to absorb social copies, and the general law now recognized by psychologists under the name of Dynamogenesis—*i. e.*, that the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement.[2] Given these two elements of endowment in the child, and he can learn anything that his father did, without inheriting any particular acts learned by the parent. And we must in any case give the child this much; for the principle of Dynamogenesis is a fundamental law in all organisms, and the tendency to take in external 'copies' by imitation, etc., is present in all social animals, as a matter of fact.

The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Epigenesis argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn any thing new, just because their nervous systems are not in the mobile

[2] Both of these requirements are worked out in detail in my book.

condition represented by high consciousness. They have instinct and little else. Now, I think the Preformist can account for instinct also, but that is beside the point; what I wish to say now is that, if Epigenesis were true, we should all be, to the extent to which both parents do the same acts (as, for example, speech) in the condition of the creatures who do only certain things and do them by instinct. I should like to ask of the Neo-Lamarckian: What is it that is peculiar about the strain of heredity of certain creatures that they should be so remarkably endowed with instincts? Must he not say in some form that the nervous substance of these creatures has been 'set' in the creatures' ancestors? But the question of instinct is touched upon under the next point.

2. (6 of Cope's table). "Habitual movements are derived from conscious experience." This may mean movements habitual to the individual or to the species in question. If it refers to the individual it may be true on either doctrine, provided we once get the child started on the movement—the point discussed under the preceding head. If, on the other hand, habitual movements mean race movements, we raise the question of race habits, best typified in instinct. I agree with Mr. Cope that most race habits are due to conscious function in the first place; and making that our supposition, again we ask: Can one who believes it still be a Preformist? I should again say that he could. The problem set to the Preformist would not in this case differ from that which he has to solve in accounting for development generally: it would not be altered by the postulate that consciousness is present in the individual. He can say that consciousness is a variation, and what the individual does by it is 'preformed' in this variation. And then what later generations do through their consciousness is all preformed in the variations which they constitute on the earlier variations. In other words, I do not see that the case is made any harder for the Preformist by our postulate that consciousness with its nervous correlate is a real agent. And I think we may go further and say that the case is easier for him when we take into account the phenomena of Social Heredity. In children, for example, there are variations in their mobility, plasticity, etc.; in short, in the ease of operation of Social Heredity as seen in the acquisition of particular functions. Children are notoriously different in their aptitudes for acquiring speech, for example; some learn faster, better, and more. Let us say that this is true in animal communities generally; then these most plastic individuals will be preserved to do the advantageous things for which their variations show them to be the most fit. And the next generation will

show an emphasis of just this direction in its variations. So the fact of Social Heredity—the fact of acute use of consciousness in ontogeny —becomes an element in phylogeny, also, even on the Preformist theory.

Besides, when we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits of others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Wherever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected. But consciousness is, of course, the prime variation through which coöperation is secured. All of which means, if I am right, that the rise of consciousness is of direct help to the Preformist in accounting for race habits—notably those known as gregarious, coöperative, social.

3. (7 of Cope's table). "The rational mind is developed by experience, through memory and classification." This, too, I accept, provided the term 'classification' has a meaning that psychologists agree to. So the question is again: Can the higher mental functions be evolved from the lower without calling in Epigenesis? I think so. Here it seems to me that the fact of Social Heredity is the main and controlling consideration. It is notorious how meagre the evidence is that a son inherits or has the peculiar mental traits of parents beyond those traits contained in the parents' own heredity. Galton has shown how rare a thing it is for artistic, literary or other marked talent to descend to the second generation. Instead, we find such exhibitions showing themselves in many individuals at about the same time, in the same communities, and under the same social conditions, etc. Groups of artists, musicians, literary men, appear, as it were, as social outbursts. The presuppositions of genius—dark as the subject is—seem to be great power ef learning or absorbing, marked gifts or proclivities of a personal kind which are not directly inherited but fall under the head of sports or variations, and then a social environment of high level in the direction of these sports. The details of the individual development, inside of the general proclivity which he has, are determined by his social environment, not by his natural heredity. And I think the phylogenetic origin of the higher mental functions, thought, self-consciousness, etc., must have been similar. I have devoted space to a detailed account of the social factors involved in the evolution of these higher faculties in my book.

I fail to see any great amount of truth in the claims of Mr. Spencer that intellectual progress in the race requires the Epigenesis view. The level of culture in a community seems to be about as fixed a thing as moral qualities are capable of being; much more so than the level of individual endowment. This latter seems to be capricious or variable, while the former moves by a regular movement and with a massive front. It would seem, therefore, that intellectual and moral progress is gradual improvement, through improved relationships on the part of the individuals to one another; a matter of social accommodation, rather than of natural inheritance alone, on the part of individuals. It is only a rare individual whose heredity enables him to break through the lines of social tissue and imprint his personality upon the social movement. And in that case the only explanation of him is that he is a variation, not that he inherited his intellectual or moral power Furthermore, I think the actual growth of the individual in intellectual stature and moral attainment can be traced in the main to certain of the elements of his social *milieu*, allowing always a balance of variation in the direction in which he finally excels.

So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of the social life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood; the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit? Whether we admit Epigenesis or confine ourselves to Preformism, I suppose we have to accept Mr. Galton's law of Regression and Weismann's principle of Panmixia in some shape. Now when social life begins we find the beginning of the artificial selection of the unfit; and so these negative principles begin to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress tn two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and seecond, by putting a direct premium on variations which are socially available.

Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great

plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of helplessness, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view.

And to finish where we began, all this is through that wonderful engine of development, consciousness. For consciousness is the avenue of all social influences.—J. MARK BALDWIN, Princeton.

The preceding communication from Prof. Baldwin is copied from Science of August 23, 1895. It is reprinted in order to render intelligible a review of it which I propose to publish in the next number of the NATURALIST.—E. D. COPE.

ANTHROPOLOGY.[1]

Mercer's Cave Explorations in Yucatan.[2]—This a handsomely illustrated volume which describes in detail the researches made by the Corwith Expedition to Yucatan, under the direction of Mr. H. C. Mercer of the University of Pennsylvania. The object of the expedition was to search for the remains of prehistoric man in the cave deposits, and to learn who were the predecessors or ancestors of the peoples whose civilization is attested by the remarkable ruins which are such a conspicuous feature of that country. Explorations of this kind made in Europe have achieved such important results to archeology, that every research in America must be watched with great interest. As a summary of his work, Mr. Mercer remarks:

"The intervening two months seemed a long time; nor was it easy to realize that, after all, the area gone over had not exceeded one hundred miles in length by ten in breadth. Twenty-nine caves had been visited in sixty days, of which ten had been excavated. Thirteen had archeological significance. Six had yielded valuable, and three, decisive results.

"We had seen but little of the ruins. We had not passed southward over the boundary line into the great wilderness, whence fables of lost cities reach the traveller's ear. Our continued study of an un-

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

[2] The Hill Caves of Yucatan: A Search for the Evidence of Man's Antiquity in Central America; being an account of the Corwith Expedition of the Department of Archeology and Paleontology of the University of Pennsylvania, by Henry C. Mercer. J. B. Lippincott & Co. Philadelphia, 1896. 8vo., pp. 183.

derground layer of human refuse substantially the same in all the caves, instructive as it was, had taught us but little of details. Evidently a wide range of tools and implements had not been left, lost or broken in the subterranean rooms. We did not find, and did not expect to find, that the water producing underground chambers had been used as burying places. Neither were they dwellings, but rather temporary halting spots, which, but for the water supply, would probably have shown fewer human traces than do the caves of the United States. Human bones scattered in the rubbish indicated that the old inhabitants of Yucatan practiced cannibalism. Beyond that, the traces of pre-Columbian cookery at the underground sites referred to an ancient cave visitor, who was rather an agriculturist than a hunter, and who (unless the dog found at Sabaka be an exception) possessed no domestic animals.

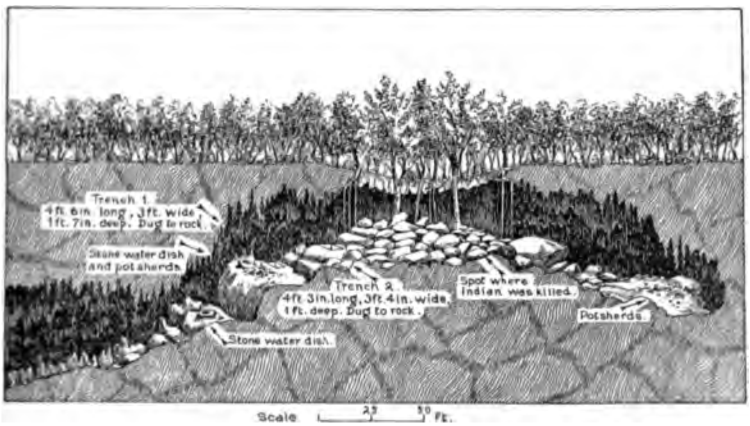
"We had learned little of stone chipping, and had found in the scanty list of stone blades but one imperfect point that might have served for an arrowhead. The secret of stone carving we had failed to discover, and though the whole mystery had seemed within our grasp at Oxkintok, we had to rest content with proving that the chiselling of the ruins could not have been done with chips of the parent block or round hammer stones. We had found no copper, or gold, or silver, no jade, no gums, no preserved grains, no cloth, no apparatus for weaving, and had discovered no pipe, and learned nothing of pre-Columbian smoking or tobacco.

"A close examination of the potsherds showed a ware mixed with powdered limestone that reacted strongly under acid on the fractures. A smooth red make, strong, wellbaked, and symmetrical, and whose dull polished surface resisted the action of nitric acid, was abundant, while a very few fragments were decorated with brightly colored designs, though their polish, after the manner of varnish, yielded readily to the acid test. Many, though better baked than the ware of the Delaware Indians, were coarse. A very common hard variety had been striped with brown lines on a white or bluish background. But there was nothing brilliant or striking about these fragments of dishes, cooking pots, or water jars. Few were ornamented, and only two or three highly so. None were marked with hieroglyphs. Nevertheless, a variety of tones, colors, and polish struck the eye when many sherds were laid side by side and brushed.

"But results more important than these had rewarded our close examination of the position and contents of the human rubbish heap everywhere present in the caves. Though this layer was the only cul-

PLATE VI.

I



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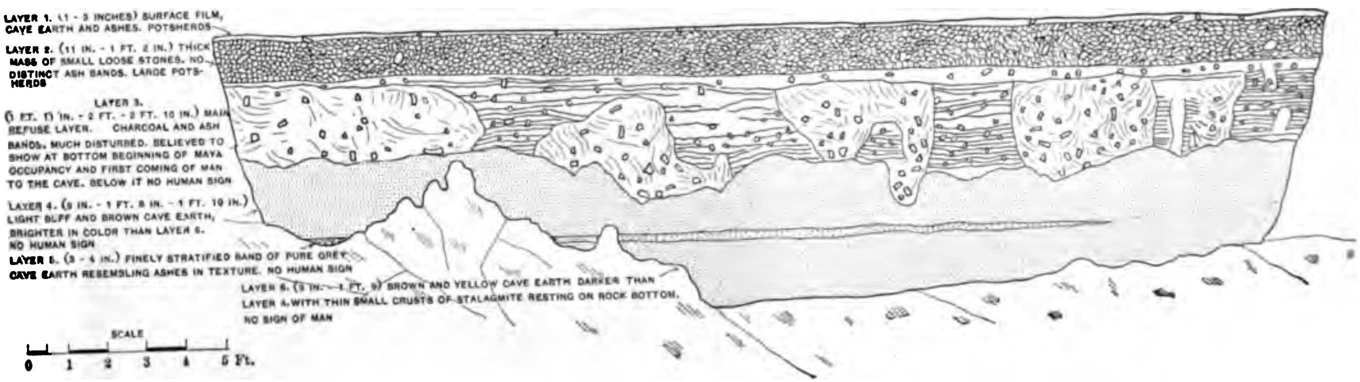
1. Cave of Sayab Actun, interior. 2. Section of Cave of Actun Xmak.

PLATE VII.



Section of Cave of Loltun.

ture layer, our digging had fairly proved at Oxkintok, Loltun and Sabaka, and though we had often failed to reach rock bottom at other caverns, there was nowhere ground for supposing that deeper digging or blasting would have upset our inference. An earlier people visiting Yucatan under its present topographical conditions must needs have left their trace in the caves, and because the undisturbed earth beneath the culture layer discovered, always failed to show trace of any deeper, older or more primitive human visitor, the conclusion was that no such earlier people had seen the region while its stony hills, its torrid plain, and its damp caves were as they now are."

The evidence secured by Mr. Mercer justifies this conclusion so far as it goes. To prove that a human population existed in Yucatan prior to that whose remains were actually found, it will be necessary to discover another series of deposits inside or out of an older type of caves. No such caves were found, and while it cannot be asserted that such will not be found, it is evident that they must be very rare if existing in the region explored. The case of Yucatan may prove to be similar to that of the United States, where I have shown on paleontologic grounds,[1] that cave deposits of two different ages exist. The remains of vertebrate life found in the caves of Yucatan explored by Mr. Mercer, are those of the existing fauna of the country, and the deposits correspond, therefore, with those of the second (postchamplain) age of the northern caves. Caves of prechamplain age are rare in the United States, as shown by Mr. Mercer's earlier researches, having been probably removed by the action of water during the Champlain submergence. That such a submergence may have also taken place in Yucatan is indicated by the recent researches of Spencer; but if so, a cleaner sweep of them was made than was the case in North America.

Among the remains of animals which were discovered, those of the horse occurred in two caves, and the dog in one. It is probable they both belong to the domesticated species.

I append some examples of the very admirable illustrations with which the book abounds.

Apart from its scientific value, this book will interest the general reader for various reasons. It is written in a pleasant style, and many side lights are thrown on the characters of the country and people. That the exploration was not without the element of danger is shown by the tragic death of one of the natives; while the sufferings of the

[1] American Naturalist, 1895, p. 598.

party from heat and insects show that none but hardy explorers could undertake such labor. We recommend the book as an admirable ex-

Interior of grand rotunda of Cave of Actun Benado.

ample of the combination of utility with adventure which characterizes scientific research in the wilds.—E. D. COPE.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Academy of Science of St. Louis.—President Gray in the chair and twenty-two other persons present, Mr. Trelease exhibited several specimens, about three feet square, of a curious silk tapestry, taken from the ceiling of a corn-storing loft in San Luis Potosi, Mexico, by Dr. Francis Eschauzier, stating that he was informed that the larger specimen had been cut from a continuous sheet over twenty yards wide and about four times as long. The specimens, of a nearly white color, and of much the appearance and feeling of a soft tanned piece of sheepskin, were shown to be composed of myriads of fine silken threads, crossing and recrossing at every conceivable angle, and so producing a seemingly homogeneous texture. Although specimens of the creatures by which they are produced had not been secured, it was stated that there

was no doubt that these tapestries are the work of lepidopterous larvæ which feed upon grain, the presumption being that they are made by the larvæ of what has been called the Mediterranean Grain or Flour Moth (*Ephestia kühniella*). The speaker briefly reviewed the history of this insect and its injuriousness in various parts of the world, and quoted from a report of Dr. Bryce, showing that in Canada, where it became established in 1889, "a large warehouse, some 25 feet wide, 75 feet long, and four stories high, became literally alive with moths in the short course of six months."—WILLIAM TRELEASE.

Boston Society of Natural History.—February 5th.—The following paper was read: Mr. Herbert Lyon Jones, "Biological adaptations of desert plants to their surroundings.—SAMUEL HENSHAW, *Secretary*.

Nova Scotian Institute of Science.—13th of January.—The following papers were read: "Notes on the Superficial Geology of Kings County, Nova Scotia," by Prof. A. E. Coldwell, M. A., Acadia College. "A Note on Newton's Third Law of Motion," by Prof. Mac Gregor, D. Sc., F. R. SS. E. & C., Dalhousie College.—HARRY PIER, *Secretary*.

New York Academy of Science, Biological Section.—January 13th, 1896.—The papers presented were: G. S. Huntington on "*The Visceral Anatomy of the Edentates.*" The characters of the brain, alimentary, respiratory and genito-urinary tracts were especially considered. The following forms were discussed: *Myrmecophaga jubata, Tamandua bivittata, Arctopithecus didactylus, Dasypus sexcinctus, Tatusia novemcincta, Manis longicaudata.* In the brain characters the following features were considered;—the transverse frontal sulcus, the great longitudinal fissure, and the absence of a distinct Sylvian fissure. In the alimentary tract the Sloths are to be sharply separated from the remaining groups, the stomach structure with its pyloric gizzard notably aberrant: the ileo-colic junction is traced throughout the edentates in a well marked series of transitional forms.

O. S. Strong, "*On the Use of Formalin in Injecting Media.*" The paper made especial note of the advantages possessed by this preservative in injecting the brain *in situ*. Formalin (40 per cent formaldehyde) diluted with an equal volume of water is injected into the cephalic vessels until it runs from the cut jugulars. After a few minutes the same quantity is again injected and once or twice again after an elapse of fifteen to twenty minutes. The brain is then removed and will be found to be completely fixed throughout. The swelling usually

noticed in formalin hardened brains does not appear to take place when this method is employed. Besides the many general advantage of fixing brains by injection, formalin has the especially merit of giving them the best consistency for macroscopic work, and further such brains are available subsequently for the Golgi and Weigert methods as well as, possibly, for cytological methods. Formalin has also the advantage that it can be used, as above, stronger than is necessary for fixation and thus allowance made for its dilution when permeating the tissue. When only the Golgi method is to be used, an equal volume of a 10 per cent solution of potassium bichromate may be added to the formalin instead of water. Pieces can be subsequently removed, hardened further in formalin-bichromate and impregnated with silver.

Bashford Dean, "*On the Supposed Kinship of the Paleospondylus.*" A favorably preserved specimen of this interesting fossil, received by the writer from Wm. T. Kinnear of Forss, Scotland, appears to warrant the belief that this lamprey-like form was possessed fins, a character decidedly adverse to the now widely accepted view of Marsipobranchian affinities. The structure referred to consists of a series of transversely directed rays, arising from the region of the postoccipital plates of Traquair. From this peculiar character, as well as from many unlamprey-like features of the fossil, it would appear accordingly that the kinship of the Paleospondylus is as yet by no means definitely determined.—C. L. BRISTOL, *Secretary.*

Nebraska Academy of Sciences.—The following program of papers was presented. *First Session—Thursday, Jan.* 2, 1896. "America the Primitive Home of Civilization," H. S. Clason; "The Home of the Buffalo Grass," Dr. C. E. Bessey; "Early Rainfall Records in Nebraska," G. D. Swezey; "The Volcanic Ashes of Nebraska," Dr. E. H. Barbour. *Second Session—Friday, Jan.* 3.—"The Relative Importance of Economic Fungi, East and West," F. W. Card; "Animal Parasites of Nebraska," Dr. H. B. Ward; "Diatomaceous Deposits of Nebraska," Dr. E. H. Barbour; "Some Fossil Diatoms from Nebraska," C. J. Elmore; "Wind Velocities in Nebraska," G. A. Loveland; "Report of Progress on the Study of Dæmonelix," Dr. E. H. Barbour; "Origin of the Present Flora of Nebraska," Dr. C. E. Bessey.

SCIENTIFIC NEWS.

Huxley Memorial.—Since the first meeting of the General Committee on November 27, which was fully reported by the Press, two meetings of the Executive Committee have been held.

At the first of these, at which Lord Shand accepted the office of Chairman, it was reported that a number of foreigners of eminence had expressed a wish to be associated with the proposal to commemorate Mr. Huxley's distinguished services to humanity. It was resolved, in the first instance, to invite subscriptions from the members of the General Committee.

At the second meeting, held on December 18, it was reported that the subscriptions, which at the General Meeting had amounted to £557, had been increased to about £1,400, and it was resolved that a wider appeal for subscriptions should now be made to the friends and admirers of Mr. Huxley amongst the general public. The sum subscribed now exceeds £1,500.

The Honorary Secretary stated that in America Committees were in the course of being formed to promote the realization of an adequate fund.

The Committee resolved to communicate, by means of a sub-committee of their number, with Mr. Onslow Ford, R. A., who had the advantage of being well acquainted with Mr. Huxley, in reference to the statue, which it is proposed should be erected beside those of Darwin and Owen in the Natural History Museum, South Kensington.

The extent to which the Committee may be able to carry out the other intended objects of founding exhibitions, scholarships, and medals for biological research and lectureships, and possibly in assisting the republication of Mr. Huxley's scientific works, will, of course, depend on the subscriptions which may now be received.

Meehans' Monthly is a magazine for the lovers of gardening; and covers the whole field of general intelligence in so far as it may have the remotest bearing on the chief topics it sets out to advance. For instance, a beautiful Prang colored plate of some wild flower is given every month, with a description which illustrates the whole ground of classical history that has any bearing on the topic. Information on the most diversified topics abound. Corn from Indian mounds will not grow—swamps that are real swamps are among the healthiest of localities. There is no sickness in the great dismal swamp of Virginia.

Elderberry root is found to be a deadly poison. Foul water is pronounced to be a self-purifier, because bacteria eat out vegetable matter and then die of starvation. The hickory and the chestnut are proven cousin—Germans. Weeds are useful, by forcing the cultivator to work to aerate the soil. Illustrations of a curious maze, formed of yew hedges at Hampton Court, pruning and keeping trees from insects, chrysanthemum culture, and practical information on fruits and flowers are among the topics treated. Sample copies may be had of the publishers, Thomas Meehan & Sons, Germantown, Philadelphia.

In the January *Monist*, of importance to students of evolution will be the article on *Germinal Selection*, by the famous German biologist, Prof. August Weismann, of Freiburg. In the theory of germinal selection, Prof. Weismann propounds a doctrine which rounds off and perfects, as he claims, the theories of Darwin and Wallace, and which consists essentially in applying the principle of the struggle for life to the minutest parts of organization, viz., to the germinal and determinant particles generally. Weismann's article is a complete summary of the present status of the discussions in evolutionary theory, and will itself doubtless constitute one of the most important recent acquisitions to biological science.

Abnormal pleasures and pains are treated by Prof. Th. Ribot, who applies to their explanation the pathological method, using diseases as a means of analysis. His results as regards the pleasure which some people take in pain are highly interesting.

The fourth annual meeting of University Extension and other students will be held in the four weeks beginning July 6, 1896, in the buildings of the University of Pennsylvania. The Summer Meeting combines the advantages of an ordinary summer school with the co-operative feature which distinguishes conventions, or associations, in which there are representatives of many universities and colleges.

Professor E. Selenka, of Erlangen, has resigned his position in order that he may make a scientific journey. He has been appointed Honorary Professor of Zoology in Munich. His place at Erlangen is temporarily filled by Dr. Albert Fleischmann.

The Paris Academy of Science has recently elected the following corresponding members: Dr. G. Retzius, of Stockholm, as successor to Carl Vogt; and Prof. R. Bergh, of Copenhagen, as successor to Huxley.

Dr. F. Miescher, Professor of Physiology in the University of Basel, died at Davos, Switzerland, Aug. 26, 1895, aged 51 years. Dr. Rudolf Metzner, of Freiburg i B, has been appointed his successor.

Dr. Felix Hoppe-Seyler, Professor of Physiological Chemistry in the University of Strassburg, died in Wassenburg, on the Lake of Constance, Aug. 11, 1895, aged 70 years.

The Australian Association for the Advancement of Science, will hold its annual meeting at Sydney, Jan. 3 to 10, 1896. Professor A. Liversedge is the President.

The Berlin Academy of Science has elected Professors K. W. von-Gümbel, K. A. von Zittel, A. Cossa, and Mr. Alexander Agassiz as corresponding members.

Dr. R. Krause, who formerly had charge of the Anthropological Section of the Museum Godfroy in Hamburg, died in Schwerin, Mecklinburg, July 25, 1895.

Dr. A. Schaper, of Zürich, has been appointed instructor in Histology and Embryology in the Harvard Medical School.

Ernst Baumann, formerly head of the station of Misahöhe, West Africa, died in Cologne, Sept. 4, 1895, aged 24 years.

Professor Hellriegel, botanist and Director of the Agricultural Experiment Station in Bernburg, died Sept. 24, 1895.

F. Nies, Professor of Mineralogy and Geology in the Agricultural School of Hohenheim, is dead at the age of 56.

Dr. Herman Credner has been advanced to the Ordinary Professorship of Geology in the University of Leipzig.

Dr. V. Rohon has been appointed Extraordinary Professor of Histology in the Bohemian University in Prag.

Dr. Valentin Häcker has been advanced to Extraordinary Professor of Zoology in the University of Freiburg.

Dr. Emil Yung is the successor of the late Carl Vogt as Professor of Zoology in the University of Geneva.

Dr. Moritz Willkomm, formerly Professor of Botany in Prag, died Aug. 26, in Wortenburg, Bohemia.

Dr. Kallies of Göttingen, has been promoted to Extraordinary Professor of Anatomy in Tübingen.

Joseph Thompson, African explorer and geologist, died in London, Aug. 2. 1895, aged 37 years.

Dr. F. Reinitzer, of Prag, has been appointed Extraordinary Professor of Botany in Graz.

Mr. R. Trimen has resigned his position as Director of the Cape Town (Africa) Museum.

L. Perry Arcas, entomologist, died in Requena, Spain, Sept. 24 1895, aged 70 years.

Dr. D. Brandza, Professor of Botany in Bucharest, died August 15, 1895, aged 48 years.

Dr. A. S. Dogiel, of Tomsk, goes to St. Petersburg as Professor of Histology.

Dr. H. Lenk, of Leipzig, has been called to the chair of Geology in Erlangen.

F. Kitton, the student of diatoms, died at Norwich, England, July 22, 1895.

E. J. Chapman, Professor of Geology in Toronto, has resigned his position.

Dr. F. Czapek is now Privat-docent in Botany in the University of Vienna.

Professor Sven Loven, of Stockholm, died Sept. 4, 1895, aged 86 years.

Dr. H. Strahl has been appointed Professor of Anatomy in Giessen.

Dr. P. H. Macgillivray, the student of Australian Polyzoa, is dead.

Dr. M. Miyoshi has been called to the chair of Botany in Tokyo.

Dr. A. Senoner, geologist, died in Vienna, Aug. 30, 1895.

Dr. J. Vesque, botanist, of Vincennes, France, is dead.

Dr. F. Müller, herpetologist, died at Basel in May.

THE

AMERICAN NATURALIST

VOL. XXX. April, 1896. 352

THE BEARING OF THE ORIGIN AND DIFFERENTIATION OF THE SEX CELLS IN CYMATOGASTER ON THE IDEA OF THE CONTINUITY OF THE GERM PLASM.[1]

CARL H. EIGENMANN.

At the meeting of the American Microscopical Society last August I read a paper on the Evolution of Sex in Cymatogaster, of which the present paper is a part. It is not, and was not intended as a full discussion of heredity, but contains observations and conclusions forced upon me while tracing the sex cells from one generation to the next in *Cymatogaster aggregatus* Gibbons, one of the viviparous perches of California.

Since writing it, I have received from Dr. Minot his article "Ueber die Vererbung und Verjüngung," which is just being republished in the NATURALIST. I have thought best to present my results as read at the Ithaca meeting, with a note written after the receipt of Dr. Minot's article, although the details of the observations on which the conclusions are based may not appear for some time.

[1] Contributions from the Zoological Laboratory of the Indiana University, No. 12.

The origin of the heredity cells may be explained in one of three ways:[2]

I. The sex cell is the product of the whole organism, and is in this apart from the other tissues. This is the *Pangenesis* of Darwin.

II. The sex cell is an unchanged but increased part of the sex cell of the previous generation, and something apart from the rest of the body. This is *Jaegerism*, or, more popularly, *Weismannism*, and, according to it, the body has no influence over the hereditary cells and changes arising during the life of one individual cannot be transmitted to the next generation.

III. The sex cell is the product of histogenesis and of precisely the same significance and origin as any other cell in the body. This view is held by Morgan, Minot and myself.

As a corollary of the last two is the fact that "in the ancestry of the individual cells of which our body is composed there has never been a death."

The first two theories are not based on observation. They have been evolved from the attempts to explain the heredity power of the sex cells.

The idea of the cellular continuity of successive generations first suggested by Nussbaum in 1880, is now generally accepted. Indeed, there is, perhaps, now no one who would contend that the reproductive cells are new formations in the individual. The reproductive cells are known to be of the same origin as the retinal or any other series of cells. There is but little less unanimity over the idea of the continuity of the unchanged germ plasm, although the number of observations bearing on this point have, necessarily, been very limited.[3] So often is the idea restated without actual examination of the data, the whole subject has become hackneyed. I have taken up this subject because it seems to me the conditions observed in Cymatogaster warrant a conclusion differing from the one generally accepted.

[2] See Osborn, Am. Nat., 1892. Morgan, Animal Life and Intelligence, 1891, p. 131.

[3] Boveri, Befruchtung in Ergebnisse der Anatomie und Entwicklungsgesch, I, 1892, records an apparent case of unchanged transmission.

There is no doubt concerning the continuity of the reproductive cells in Cymatogaster; they may be followed from very early conditions till sexual maturity without once losing their identity. No somatic cells are transformed into reproductive cells, and the comparative constancy of the number of the latter present in any embryo up to 7 mm. long makes it probable that none[4] are ever changed into any other structure. These statements apply with equal force to other tissues.

The difference between the reproductive and the somatic cells is that the latter, after development has begun, continue to develop, divide, grow and adapt themselves to their new duties without intermission. The sex cells, on the other hand, stop dividing at a certain point and remain at apparent rest for a long period. Owing to this arrest in division the sex cells soon stand out prominently as large cells among the smaller somatic cells. Such an arrest in segmentation has been observed in a number of other animals in which the reproductive cells are early segregated, and it cannot be without meaning. It has been supposed that during such periods of apparent rest the cells remain dormant, retaining their embryonic character unchanged. I do not think this is the true reason for the difference of development between the soma and the reproductive cells. The reason seems to me to lie in the fact that the sexual organs are the last to become functional, and their development is consequently retarded. The sex cells, when first segregated—that is, when they first lag behind in segmentation—are not exactly like the ovum from which they have been derived, and there is just as true histogenesis in their development into the reproductive tissues as in the case of any other embryonic cells into their corresponding tissue. Even during the long period of rest from segmentation, the process of tissue differentiation produces a visible and measurable change. But the difference between embryonic cells and undifferentiated reproductive cells being small, the histogenic changes in them during early stages is correspondingly small. This small change has been supposed to amount to no change, and has given rise to that fascinating " myth ", the

[4] For possible exceptions see Eigenmann, Journ. Morph., V, No. 3, 1891.

hypothesis of the continuity of unchanged germ cells, and later, when observation in other animals had made this theory untenable, to the theory of the continuity of unchanged germ plasm which is beyond the ken of direct observation.

If the sex cells are the result of histogenesis, it will be necessary to explain their peculiar power. They seem to me to be due to the same processes that have given the retinal cells their peculiar properties.

Assimilation, reproduction and the closely allied hereditary power are the diagnostic characters of protoplasm. These, with numerous other powers, such as contractility, conductivity and irritability, are the properties of every protozoan cell. Even here we find that certain of these functions are more or less restricted to definite parts of the cell. In the higher animals this differentiation has gone so far that definite functions predominate in highly specialized cells to almost the exclusion of the other powers.

With this division of labor and the consequent histogenic differentiation of definite cells in the metazoan corm for purposes of contraction, conduction and irritation, we have also the differentiation for heredity, and it would be surprising if we did not.

In lower forms, where the cells of the body often perform many duties, where the division of labor and histogenesis has not been carried to the extreme, many of these cells also retain the hereditary power to a great extent as shown in the power of budding or regeneration.

There seems to he no necessity to conjure up a substance and processes in the genesis of the reproductive tissues different from those obtaining in the muscular tissues.

During the long ages of the rise of animals those possessing sufficiently differentiated contractile tissue to move the corm to food or from danger have survived, and in precisely the same way those corms containing cells capable of developing into other similar corms have survived. Similar causes have operated in producing each tissue.

The sex cells are proven to influence the formation of the sex ridge. The peritoneal cells rise to form the ridge only

when sex cells are present without regard to whether this position is normal or not.[5] If the sex cells thus influence the surrounding tissue, may we not safely assume a reciprocal influence of the surrounding tissues on the reproductive cells?

Sexuality can first be distinguished not by the difference in the sex cells, but by the character of the peritoneal covering. While this difference in the peritoneal covering may be the expression of an invisible difference existing in the reproductive cells, it is quite possible that sex is determined by the body. In frogs, butterflies, etc. the sex determining power of the soma has been experimentally demonstrated. Later it is well known that the character of the sex cells influences the remotest parts of the organism, although we are not at all familiar with the processes by which this is accomplished.

Changes in the sex cells introduced by the body which do not become apparent until the development of the cells into young, seem, therefore, to be not impossible, although we are entirely unable to tell just how such a change might be accomplished.

Since writing the above, I have received, through the kindness of the author, Dr. Minot's " Ueber die Vererbung und Verjüngung." While the views expressed are not identical with those given in the present chapter, there is considerable agreement. Dr. Minot recognizes that the problem of the origin of the reproductive cells is also the problem of the origin of the tissue cells (p. 580), and that " a germ plasm in the Weismannian sense does not exist." So far we agree. According to him all parts inherit from the germ and possess, as well as the reproductive cells, the power of multiplying and morphogenesis, but this power cannot manifest itself on the part of the somatic cells because the conditions of the body prevent it. The conditions are the increased amount of protoplasm and the specialization of the tissues. According to my views it is not so much a high state of tissue differentiation which *holds captive* the morphogenic power in muscle cells for instance as it

[5] In one interesting larva a few of the sex cells were belated in their migration and situated in front of the normal position. Sex ridges (germinal bands) formed about these sex cells entirely independent of and separated from the sex ridges occurring in the normal place.

is the process of tissue differentiation which *emphasizes* the contractile power in the muscle cell, at the same time *limiting* and finally *eliminating* the morphogenic power, and which gives the sex cells morphogenic power in such marked degree while it deprives them largely of contractile power. In a former paper,[6] I stated this view thus: "The segmentation nucleus of metazoa contains, as in the infusoria, both micro and macro nuclear elements, but these are retained in varying proportions in its descendants, *i. e.*, in the cells of the adult organism. Through a process of division of labor the power of rejuvenescence becomes restricted to comparatively few of the cells derived from the segmentation nucleus."

While Minot's views are in part borne out by the conditions in Cymatogaster, the italicised part of the quotation below finds no support, and is negatived by all the observations made in Cymatogaster. His conclusion, as translated by me, is: "Somatic cells are simply cells in which the activity of heredity is prevented by senescence, viz.: tissue differentiation, *but the somatic cells can, under favorable conditions, be translated into the rejuvenated stage and then develop the most complete or, at least, more complete, hereditary power.*"

ABSTRACT OF OBSERVATIONS ON WHICH THE ABOVE CONCLUSIONS ARE BASED.

The sex cells originally segregated retain their individuality, but undergo a measureable change between the time of their segregation and 7 mm. long larvæ. Soon after the larva has reached a length of 7 mm., the sex cells begin to divide. In the meanwhile they have migrated laterad and lie, for the most part, in a longitudinal groove formed by a duplication of the peritoneum into which a few peritoneal cells have also migrated. *In one such case an extra sex ridge was formed much further forward than usual, in connection with a few sex cells which were accidentally belated in their migration.* The peritoneal cells which have migrated into the sex ridge give rise to the entire stroma of the future sex glands, and together with the sex cells form a core quite distinct from the covering

[6] Bull. U. S. Fish Comm., XII, 442, 1894.

of peritoneum. Posteriorly the sex ridges of the two sides are united into a single ridge. There is considerable variation in the rate of segmentation in larvæ of the same size, but the following table will give an idea of the segmentation and the number of cells in successive stages:

Size of larva.	No. of sex cells.	No. of generations from fertilization.
45–5 mm.	9–15	5
8	22	6
10	28–183	6–9
12	39–143	7–9
15–17	638–2280	11–13 sexes distinct.
16–25	2200–8000	13–15

The sexes can first be distinguished not by the differences in the sex cells, but in the tunic of peritoneal cells. A small groove on the outer ventral part of the sex ridge is the first indication of the ovarian cavity and the surest criterion of the female. In the male the sex gland remains much more circular in cross section and no groove is developed. Much later histological differences in the sex cells themselves can be made out. The long slender chromatin threads of the female cell just before dividing are represented in the male by short, thick bars.

THE HISTORY AND PRINCIPLES OF GEOLOGY, AND ITS AIM.

By J. C. Hartzell, Jr., M. S.

(*Continued from page 183.*)

Lamarck and Defrance earnestly engaged in study of fossil shells, and the former, in 1802, reconstructed the system of conchology and introduced into it the new species collected by the latter from the strata underlying the city of Paris and quarried for the construction of its buildings. Six years previous to this Cuvier had established the different specific character of fossil and living elephants and he devoted himself to researches throughout the remainder of his life. Jameson, in 1808, pointed out the nature of all the rocks and the mode in which they were formed, and made use of the observations

of Desmorest, who, in 1768, traced the origin of basalt to the crater of volcanoes.

In 1807 the Geological Society of London was established with the professed object of encouraging the collection of data and the making of observations. In 1819 the Society published a map of England by the aid of Greenough. About the same time Buch prepared a similar map of a large part of Germany. A geological survey of France was ordered in 1822 by the French government, and as a result a geological map was published in 1841. Conybeare and Phillips published a treatise on the "Geology of England and Wales," in 1821. In 1814 Aiken published his work on mineralogy, which had a large circulation at home and in this country. Previous to this Sowerby published a work on "British Mineralogy, illustrated with colored plates," but the date of which I do not know. The publication of the Geological Map of England, in 1815, by Smith, may be said to form an epoch in the history of geology.

In 1809 Maclure published an article on "Observations on the Geology of the U. S., explanatory of a Geological Map," and he is rightly called the father of American geology. He visited all parts of the Union and all the principleming ni districts of Europe. In 1817 he presented a report to the "Philosophical Society of Philadelphia" of his work, and accompanied it with a colored map. In 1816 and 1817 he visited the Antilles and published a paper on their geology. In 1810 Bruce, of New York, published the first purely scientific journal supported by original American contributions. His journal was devoted principally to mineralogy and geology. Science was also promoted by the collections in the colleges and societies, and by those made by scientific men. In 1816 Cleveland published a treatise on mineralogy. In 1818 Dana published a detailed report on the mineralogy and geology of Boston and vicinity. In the same year the American Journal of Science was first published. The first geological survey made by State authority was that of North Carolina in 1824.

In 1830 the Principles of Geology, by Lyell, appeared and has most powerfully influenced the direction of scientific

thought in the 19th century. It broke down the belief in the necessity of stupendous convulsions in past times. He adopted and improved the views of Hutton, eliminating the baseless theories mingled with them. He rendered great service in elucidating North American geology, and published his travels on this continent in 1845 and 1849. His "Geological Evidences of the Antiquity of Man," published in 1863, startled the public by its advocacy of Darwin's theory in the "Origin of Species."

And so the science has advanced with rapid strides and is solving the problems that are constantly arising in regard to our planet, and upon its fixed data are based many of the fundamental principles of philosophy.

Having considered the history of the progress of geology, let us now consider its aim and the fundamental principles upon which the geologist bases his work.

In the broadest sense, geology is the science whose province is the planet upon which we live, its history from the beginning to the present, including changes which have occurred in regard to the condition at different periods, its several physiographic features, its' atmosphere, temperatures, and aqueous bodies, and its life at different stages. In a nutshell, the evolutionary progress of the earth.

The narrow or commonly accepted view does not consider the changes that have occurred, other than those that occurred to the visible portion of the earth. Back of what is supposed to be the earliest formation, it does not attempt to go.

The latter view is sufficient for the ordinary geologist or for the geologist who does not care to speculate on hypotheses which refer to the origin of the earth; but to the geologist who is anxious to grapple with problems which require a drawing upon the imagination for solution, this is not enough. Chemists are not satisfied to study a drop of water, but they are anxious to know its origin; its composition is not sufficient for them. Botanists and zoologists desire to know the origin of plants and animals, not merely their structural and physiological features.

Geologists who study the earth, not merely to satisfy their own curiosity as to the *present* condition of things, but for the purpose of advancing the science, and unraveling the mysteries of the past, in order to produce a history of the planet as accurately as human knowledge in its present condition will permit, are only satisfied with the broad and comprehensive view.

Geology, by the aid of astronomy and physics, therefore, begins with a great nebulous mass, of which all celestial bodies were component parts. It traces the evolution of each body, and that of the earth in particular. Starting when the earth was thrown off as a ring of cloudy or gaseous elements, it traces it through its transformation into a sphere of molten matter surrounded with gases, through which the parent body, the sun, could not penetrate. We learn of the war that existed between the congealing surface and the liquid interior in which the former came off victorious, and formed a crust through which the latter seldom broke. Then began the war between the condensing vapors and the heated crust, in which the latter succumbed to the overpowering element that fell upon it and fairly covered it.

Geology tells us of the life that existed in this mighty ocean after it became sufficiently cooled, and in the powerful internal movements that resulted in the upheaval of masses of rock that were to be the nuclei of the present continents, the history and the formation of which is traced with great minuteness, and the life of each is described with great care, from the lowest forms to the highest, and also the period in which each form lived.

There are several principles by which the geologist is guided in answering the questions that continually arise as he studies the earth with its many characteristics.

1 In the first place, he understands that *geology is an inductive science.* That is, it is a process of demonstration in which a general truth is gathered from an examination of a self-evident truth. Let me illustrate: From the study of modern glaciers he learns certain facts in regard to conditions necessary for their formation, their modes of action, and the results of those actions.

Now, whenever a geologist sees the results of some great force and those results are similar to the phenomena produced by glaciers, he concludes that at some previous time the conditions were such as to make it possible for glaciers to exist in the locality in which his observations were made, for no other force could produce them.

2 He reasons that *all affects must be referred to secondary causes.* In other words, law governs all phenomena, and forces are so balanced as to produce all *known* and *unknown phenomena.* All events that have transpired in the development and configuration of the earth have been brought about by law. In the formation of glaciers certain laws are obeyed, and those laws are always obeyed unless an equilibrium is sustained between them and some other laws are overbalanced.

When the conditions are favorable for the action of *glacial laws* glaciers will be found. The same principle holds good in the distribution of life.

3. *The forces in existence to-day are capable of producing all phenomena that have and may occur.* Therefore, the geologist must study the methods by which they are producing changes at present, and thereby be able to judge of what took place ages ago, and the manner in which great events transpired. In other words, *the past is understood by the present* and to some extent *the future may also be understood.* No new law is, nor has been, necessary for the explanation of phenomena and, therefore, there have been no accidental happenings. There may be laws that man has not as yet learned the nature of, and they may be so balanced as to be beyond man's comprehension, but that there are being or have been created new laws, and that there are accidents, the geologist does not admit.

4. *The earth is undergoing and therefore has undergone changes.* He sees this in studying the phenomena of denudation and disintegration. He sees that the mountains are being destroyed by chemical and physical agencies, and that they are being gradually carried into the valleys, and then into the sea. This, he reasons, must have been going on ever since the first continent made its apppearance.

5. Finally, from a consideration of the above principles, the geologist realizes that his *work must be systematic*, and that the bulk of it *must be done in the field. Field investigation is indispensable. Laboratary work holds a subordinate position.*

It is safe to say that geology has advanced more rapidly than any other science, and the number of those who are making a specialty is steadily growing. New periodicals devoted to the science are continually appearing, and its literature is quite comprehensive. Very little attention was paid to it in our colleges at no late date, but to-day it occupies a prominent position.

The great advance which has been made is due to systematic field work, followed by laboratory work, and the latter is of but little value from a geological standpoint unless it is based upon accurate field investigation. It is necessary to reduce to a practical formula the data secured in the field, and to have a definite method of procedure, for without such, much time is wasted, and many results that otherwise would have been valuable are entirely lost. Mere conjecture must not be indulged in, but " work persistently back from the seen and known to the unseen and unknown," should be the maxim. Conclusions must not be arrived at too hastily.

Professor Dana once said, " I think it better to doubt until you know. Too many people assert, and then let others doubt." Hence, in drawing conclusions from the results of field and laboratory work, be sure you are right, before giving publicity to them, and if a doubt exists, state it, and be willing to change your theory. Dana says, " I always like to change when I can make a change for the better."

It is obvious, from what I have said, that geology is a field science. Different characteristics of the earth's surface cannot always be taken into the laboratory for study at leisure, and it is necessary to see the objects under study if we would arrive at correct conclusions and fix them indelibly in our minds. Facts then become real, and we acquire a correct understanding in regard to the forces that have been at work preparing this planet for man.

It is necessary to have a knowledge of other sciences if one would make practical use of geology, that is, to understand the many phenomena that are presented to him.

Natural philosophy and chemistry are necessary in order to determine the composition of rocks and to understand how they were formed and changed. Botany is necessary to understand paleobotany, zoölogy is necessary to understand paleozoölogy, astronomy figures very prominently in the determination of the relations of this planet to other heavenly bodies. Anything that the telescope and the spectroscope reveal is of geological importance, and bears upon the past and future condition of the earth. Mathematics is constantly in use, and without that science little or nothing could be accomplished.

The foundation work of a geologist, therefore, should be a knowledge of the natural sciences, for without them he will be materially hampered in his work.

Geology is practical as well as literary in nature. Every agriculturalist would become more scientific, and would reap better "crops" if he had a knowledge of the science, for it gives a knowledge of soils and fertilizers. To the engineer it is of great importance, for thereby he understands drainage and the best methods for excavating. It is of great importance to the manufacturer, for he can better understand clays, ores, fuels, etc., and in mining it is of great value for it enables the miner to understand the nature of the rock in which the metals occur and assists him in "prospecting."

This use of the science is termed "Economic Geology" and is of inestimable value and importance in developing systematically the resources of a state or of a nation.

The United States government has realized the importance of thorough and accurate investigation of this vast country of ours from an economic standpoint, and established the U. S. Geo. Survey in 1879 for this purpose. Most of the states have their surveys and work for the same ends, but on a smaller scale, and assist, and are assisted by, the government survey, and so work in harmony with each other.

Individuals are at work gathering information in regard to particular formations, correcting mistakes, advancing new theories, devising new plans for more thorough and accurate works and imbuing students with the grandeur of the science.

What is there more sublime than a science that reveals the universe in all its beauty and grandeur and as the result of the balancing of forces which emanate from a creative will? Geology reviews the history of the planet from the earliest known formation to the present. Back of this it goes by retrograde calculation, and hence we have a complete resumé from the time "the earth was without form and void," to the phenomena observed to-day. It tells us of periods of time of immeasurable duration, during which was being molded that upon which it would be possible for life to exist, and over which mind should rule.

There is no science which presents so many problems to be studied, nor in which so much of interest can be taken. It carries one over plains, up the rugged mountains and down into valleys. On every hand is found something new upon which to concentrate the mind, and which demands a satisfactory explanation. How came these plains, these mountains, these valleys? How came those masses of rock, thousands of feet high? Why is sandstone here, limestone there, and granite yonder? What mean those remains of animals and plants that are not in existence to-day? Why are those masses of rock in every conceivable position? Whence came the waters and the land? The plants and animals? Is there a reason for all we see? Are these things accidental, or was there a purpose in their formation?

And so questions crowd upon us, and fill us with wonder and admiration, and with a determination not to be satisfied until they are answered. We see that law is at work, fashioning the universe, and we have brought very forcibly to our minds the fact that there was a purpose involved in the creation of the universe, and that from this realized grand conception is being evolved a divine purpose. That which at first appeared to be outside the domain of law, is seen to be the result of the balancing of forces; and we come to realize the fact

that law pervades the universe, and although we do not know as yet the way in which these laws are balanced to produce all phenomena, that they are so balanced as to produce harmony, and that in proportion as the human mind develops it will be capable of grappling with problems that are not now within its reach.

LIFE BEFORE FOSSILS.

By Charles Morris.

(*Continued from page* 188.)

Such a new stage of existence may have been essayed frequently. The dwellers in the early seas, in their descents below the surface, must often have come into contact with the bottom, and at times temporarily rested upon it. This contact with hard substance doubtless produced some effect upon them, and certain variations in structure may have proved of advantage in these new circumstances and been retained and further developed. Particularly if food was found there, and habitation on or near the bottom was thus encouraged, would such favoring variations tend to be preserved.

But, as has been said, myriads of years may have passed in the slow development of swimming pelagic animals before this phase of evolution was completed. And, perhaps, not until this was fully accomplished did contact with the bottom set in train a new series of changes, and in time give rise to the greatly transformed bottom-dwellers. The change, indeed, was a great one, if we may judge by the wide diversity in character between the swimming embryos and the mature forms of oceanic invertebrates, and must have needed a long period of contact with the bottom for its completion. Yet it was probably much more rapid than had been the preceding pelagic development. Contact with solid substance was a decided change in condition, and may have greatly increased

the preservation of favorable variations. And the area of habitation on the single plane of the sea bottom is so restricted as compared with that within the many planes of oceanic waters, that the struggle for place and food must have been greatly increased, and the development and preservation of newly adapted forms have been more rapid in consequence.

This may seem to bring us to the very verge of the kingdom of life as it is known to us from the oldest fossils yet discovered. Yet in truth we are probably still remote from it. We are still dealing with soft bodied animals, not with those possessed of the hard external skeletons from which fossils are produced. There is no good reason to believe that mere contact with the earth induced the previously naked swimmers to clothe themselves in solid shells. In truth, the earliest bottom-dwellers may have long continued soft bodied, the hard case or shell being only slowly evolved. The mantle of the mollusk, for instance, with its shell-secreting glands, is not likely to have been a primary accessory of molluscan organization. The same may be said of the chitin-forming glands of the crustacea, and the analogous glandular organs of other types. Such conditions must have developed slowly, and their appearance was probably due to an exigency of equally slow unfoldment.

For now we come to another highly important problem, that of the true disposing cause of the development of dermal skeletons, on which there exists some basis for speculation. In truth the fossils preserved for us in the Cambrian rocks have an interesting tale to tell which has a strong bearing upon the story of animal evolution. And this is, that all these bottom-dwellers, with the exception of the burrowing annelids, became covered with what was probably defensive armor. They all seem to have sought protection in one way or other, and in so doing became in a measure degenerated forms of life, their former ease of motion being now partly or wholly lost.

All this represents an interesting stage in the process of evolution, and indicates some special exigency in life conditions which the animals of that age could only meet by rendering themselves heavy and sluggish with a weight of inclosing

armor. This new phase of evolution may have proceeded very rapidly, many forms of early life disappearing, while those that quickly became armored survived.

What was this exigency? Protection, apparently, as is above stated. But protection from what? Against what destructive foe did these ancient animals need such strong defence? Which among them was the rapacious creature whose ravages imperilled the existence of all the others? Certainly not the sponge or the cœlenterate; they feed on smaller prey. The mollusk or the echinoderm, in their agile unclad state, may have been actively predatory, but they were among those forced to seek protection. Of the known forms the trilobite seems most likely to have been the aggressive foe in question. It was the largest, the most abundant, and, perhaps, the most active of them all, its size and numbers indicating an abundance of easily obtained food, while its great variety of species points to the existence of varied conditions of food or methods in food getting.

To all appearances the trilobite was then the lord of life, the Napoleon of that early empire. Awkward and clumsy as such a creature would appear now, it was then superior in size, strength, and probable agility to all other known animals, while its numbers and variety indicate that it was widely distributed and exposed to all the varying conditions of existence at that time. What a hurrying and scurrying there must have been among those small soft creatures to escape this terrible enemy, from whose assaults nothing seems to have availed them but an indurated external covering, too hard for its soft jaws to master. As the prey became protected in this manner the destroyer probably improved in strength of jaw, and there may have been a successively more complete growth of protective devices in the prey and of powers of mastication in the foe. And thus arose the conditions which first made fossilization possible, in the development of a series of armor-clad creatures which were really late comers upon the stage of life, remote as they seem when measured by our standard of time.

But the story is only half told. The trilobite, as it is known to us, is under armor also. Not only is it clothed in a dermal

skeleton, but, in its later forms, is capable of rolling up into a hard ball with no part of its body exposed. Evidently the destroyer himself in time came into peril and needed protection. Some still more powerful and voracious foe had come upon the field, and the triumphant trilobite was forced to acknowledge defeat.

We cannot well imagine any of these animals assuming such armor except for protective purposes. The weight laid upon them rendered them slow and sluggish, fixed some of them immovably, and greatly decreased their powers of foraging. The only cause which seems sufficient for their assuming this disadvantageous condition is that of imminent peril—a peril which affected all known forms alike.

Whence came this peril? Where is the voracious foe against whom they all put on armor, even the preceding master of the seas? No trace of such a creature has been found. In truth, we cannot fairly expect to find it, since it was probably destitute of hard parts, and left behind it nothing to be fossilized. It had no foe and needed no armor, while lightness and flexibility may have been of such advantage to it that armor would have proved a hindrance. It probably was a swimming creature and thus left no impress of its form upon the mud. It is to this unknown creature that we must ascribe the armored condition of all known forms of life at that period, even the later cephalopods, large and powerful mollusks, becoming clothed in a cumbrous defensive shell, which they were obliged to drag about with them wherever they went.

It is a strange state of affairs which thus unfolds before our eyes. All the life we know of seems diligently arming itself against some terrible enemy, which itself has utterly vanished and left as the only evidence of its existence this display of universal dread. The creature in question would appear to have been without internal or external hard skeleton and without teeth, trusting to indurated jaws for mastication. At a later date, when its prey became less easily destroyed, teeth may have developed, and it is possible that we have remains of them in the hard, cone-like, minute substances found in the lower Silurian strata, and known as conodonts.

If we may try and rebuild this vanished beast of prey from conjecture, aided by collateral evidence, we should consider it an elongated, flexible form, developed from some swimming worm-like ancestor, perhaps like the Ascidian embryo, stiffened internally by a cord of firm flesh extending lengthwise through the body, and moving not by cilia, but by the aid of fleshy side flaps, the progenitors of the fin. We conjecture it to have been, in short, the early stage of the fish, a creature perhaps of considerable size and strength, due to the abundance of easily obtained food, but as destitute of hard parts and as little likely to be fossilized as Amphioxus.

We may offer this conjecture with some safety, for it is not long before we come upon actual traces of fish, and of a degree of development which indicates a long preceding stage of evolution. In fact, the fish in time appears to have been forced to put on armor, as its prey had earlier done. Internicine war began in the fish tribe itself. A wide specific variation arose, with great differences in size and strength, the stronger attacked the weaker species, and eventually two distinct types of fish appeared, the Elasmobranch and the Ganoid ; the former, represented to us by the modern sharks, being much the most powerful and voracious, and holding the empire of the open seas, while the latter dwelt in shallower waters. The Ganoids, preying on the bottom forms, become themselves the prey of their strong and active kindred, and, as a result, the evolutionary process just described was resumed. The weaker fish put on armor, in many cases heavy and cumbrous, a dense bony covering which must have greatly reduced their nimbleness, but which safety imperatively demanded. It is these armored forms that first appear to us as vertebrate fossils; the first fish, as the first mollusk or crinoid known to us, being the resultant of a very long course of development. As regards the Elasmobranchs, they, too, became in a measure protected, though not sufficiently to indicate any very active warfare among themselves.

There is little more which we can say in this connection. The story of the evolution of life bears an analogy worth mentioning to that of the development of arms of offense and de-

fense among men. After thousands of years of war with unarmored bodies, men began to use defensive armor, the body becoming more and more covered, until it was completely clothed in iron mail, and became rigid and sluggish. In the subsequent period offensive weapons became able to pierce this iron covering, and it was finally thrown aside as cumbrous and useless. A similar process is now going on in the case of war vessels, they being clad in heavy armor, which may yet be rendered useless by the development of cannon of superior piercing powers, and be discarded in favor of the light and nimble unarmored ship.

The analogy to animal evolution in this is singularly close. After long ages of active warfare between naked animals, defensive armor was assumed by nearly every type of life, except the lowest, highly prolific forms, and the highest, which had no foes to fear. But the powers of offense grew also, and in time the employment of armor ceased, as no longer available, its last important instance being that of the ganoid fishes. The later fish reduced their armor to thin scales, and gained speed and flexibility in proportion, while in land animals armor was seldom assumed. In several instances creatures have gone back to the old idea, as in the armadillo, the porcupine, the turtle, etc., but the thinly clad, agile form has become the rule, armor no longer yielding the benefit that was derived from it in the days of weak powers of offense. This result is a fortunate one, since with increase of agility mental quickness has come into play, the result being a development of the mind in place of the old development that was almost wholly confined to the body. In the highest form of all, that of man, physical variation has almost ceased, in consequence of the superior activity of mental evolution.

In conclusion it must be admitted that there are certain formations in nature which seem to militate against the argument here advanced. I have already spoken of the much questioned Eozoon canadense. In addition there are the beds of limestone and graphite in the Laurentian formation. But these prove too much for the advocates of their organic origin. If so large a fossil as Eozoon had appeared so early, the subse-

quent barrenness of the rocks would be incomprehensible. And had coral animals and large plants capable of producing such masses of limestone and graphite existed so early, the absence of any fossils earlier than the Cambrian would be inexplicable. It is acknowledged, however, that such formations might have been produced by inorganic agencies, and the facts strongly indicate that such was their origin, and that fossils began to be preserved very shortly after the power in animals to secrete hard skeletons appeared.

BIRDS OF NEW GUINEA (FLY CATCHERS AND OTHERS).

By G. S. Mead.

(*Continued from page* 195.)

The Thickheads (*Pachycephala*) are of many species and scattered widely over the Archipelago. Many have come under trained observation only during recent years. Probably many more await discovery.

Pachycephalopsis poliosoma, Gray Thickhead, was discovered by Mr. A. Goldie in Southeastern New Guinea, and owing to its distinctive coloration was classed as a new genus. It is really one of a group of birds which might form a subgenus and is accordingly so divided by Mr. Gadow. Above the general color is dark gray, almost brown, with the head still darker. The square, rather short tail is also dull of hue. Beneath is dull gray, lighter on the abdomen and tail coverts, whitish to white on the jugulum, throat, chin and side face. It is a pretty, soft colored little bird about 6 inches long, sufficiently numerous among the mountains of the Astrolabe range to be called common.

Pachycephala melanura ranges widely over Northern Australia and the Archipelago. The general color above is olive-green ; wing coverts, tail, head and an irregular band passing

over the head, neck and breast, black and glossy black. The under parts, with a broken collar about the neck, are a warm light yellow. Throat a pure white. Whitish lines the under side of the wings and tail. Bill and feet black. The female lacks the vivid coloring of the male, being brownish where he is a jet black, buff or whitish where he is a bright gold. Length 7 inches.

Very like the above, but of reduced size, is *Pachycephala schlegelii*, whose total length is under 5.5 inches. The differences lie in the greater width of black band across the breast, in the line of black edging the wings, and the orange rufous on the abdomen. The female resembles the female of *Pachycephala soror*, found also among the Arfak Mountains. This bird is olivebrown above, wings and head darker. The under surface is a bright yellow, omitting the grayish wings and dull thighs. Like her mate, the throat and chin are white. The male *P. soror* is unmarked by the yellow nuchal collar but is not without the black crescent. A bright yellow covers the breast and abdomen. The head is black, the tail dusky. Total length about 6 inches.

There are several other species of *Pachycephala* resident in Papua, almost all bearing a greater or less resemblance to each other. Among these may be mentioned without detailed description, *P. hyperythra* from Southeastern New Guinea whose under parts are of the warm reddish color that gives it its specific name.

P. albispecularis, from the Arfak region, is another species—a somewhat larger bird than its kind, gray and dark brown in general coloring with white markings on the wings.

Still another is *P. griseiceps* or *virescens*, with local differences, a bird of the average length, somewhat diversified plumage and a mottled head.

Smaller than the foregoing but with throat and chest crescent more distinctly outlined, is *P. leucogaster*, collected in the Motu country. *P. leucostigma*, from the northeast, is considerably mottled, with much rufous on the under parts, the usual white in this instance somewhat discolored, on the throat, and much streaked on the mantle.

Pachycephala fortis has its habitat in the Astrolabe Mountains, though found probably elsewhere in New Guinea. Its total length is nearly 7 inches, colored almost entirely above dark olive, below ashy gray. The head and mantle are dark gray, the tail dusky, the back and wings greenish olive. On the face are gray shadings. White prevails on the abdomen, passing into yellow. The under wings do not differ from the uniform cloudiness but are, if anything, even duller than the body.

Pachycare flavogrisea, set apart from *Pachycephala*, is colored a bluegray above, somewhat varied on the tail and wings by black or white edgings, while the under parts are a "deep, shining yellow, the yellow on the forehead and the sides of the head and neck being separated from the bluegray of the head by a broad dark stripe." Total length 4.5 inches.

If we look for those attractive little birds—the Titmice—in New Guinea, we shall find very few, if any, specimens. One is mentioned in the books, viz., *Xerophila leucopsis*, an Australian species, abundant in Queensland but not so numerous in Southern Papua. The little bird in question has a length of 4 inches. Its general color is brown, ashy above, whitish and yellowish beneath. Along the tail, neck and head the brown is positive; this is true also of the under wings; elsewhere, however, the colors are pale and indistinct, shading off gradually, as on the sides and breast, into a clouded white.

Several species and subspecies of the genus *Cracticus* range between Australia and New Guinea. These are Lanidine birds of good size, strong of beak, black, white or gray of color.

Cracticus quoyi, a typical representative, is one of these distributed pretty generally over North Australia and Southern Papua. It is almost entirely black and blueblack, the only variation being in the shading and lustre. The length is about 14 inches. Sexes alike.

Cracticus cassicus or *personatus* is more peculiarly insular, being confined chiefly to New Guinea and its islands.

The bird is strikingly conspicuous in its contrasted black and white. The former color covers the head and neck, throat and chest, upper wings and tail, excepting the two

middle feathers which are partially white. There are scattered markings, moreover, of black, intermingled with white on the back and wings. All else is a pure white above and beneath. The female is perhaps not of such glossy plumage and has less white on the back. She is also smaller than her mate by half an inch. Total length 13 inches.

Another species from Southeastern New Guinea, collected by Mr. Stone and others, is called *Cracticus mentalis* or *spaldingii*. This Dr. E. P. Ramsay of the Sydney Museum believes to be identical with *C. crassirostris*, a species separated by Count Salvadori from *C. quoyi*, already described, though by some regarded as one and the same. *C. mentalis* is about 10 inches long. The white is banded so as to divide the black of neck and back. Chin black.

In addition to those not very happily named birds—Eupetes—already mentioned in a previous article, two or three species may be briefly described.

Eupetes incertus is colored above a warm ruddy brown, the tail not quite so bright. White, bordered by dusky covers the throat, side face and abdomen. Over the chest and along the side body the plumage is rufous, the under tail coverts buff. Bill and feet are dark. Total length about 7 inches. The mountains of the northwest are the home of this species, as also of *Eupetes leucostictus* whose breast is flecked with white as its name indicates. This *Eupetes* is boldly colored with its chestnutbrown head and mantle, and its glossed dark green body and black wings spotted white on the coverts. Instead, however, of the usual white throat, the throat is black, although there is much white on either side. Black marks, too, lie on the face near the eye, the chin and upper breast. The lower parts are gray with a bluish tinge. The tail is black, the exterior feathers tipped with white, the middle ones oily green. The bill, feet and eye are black. Altogether this specimen is a remarkably fine one, unlike, in many respects, most of its family.

Eupetes pulcher, discovered in the Astrolabe Mountains, by Mr. Goldie, may be briefly described as differing from *E. castanotus* (AMER. NAT., No. 343, p. 634) only in having the head

a decidedly dusky shade instead of chestnut, and a narrow black edging to the throat in place of a somewhat broad band of black. Length 9 inches. Female a trifle smaller.

Eupetes ajax (Temm.) or *Cinclosoma ajax*, as Dr. Sharpe prefers to call it, classing it as distinct from the *Eupetes*, is a thrushlike bird about the same size as the foregoing. The general color above is a dull brown, becoming darker near and upon the tail and wings. The wing coverts, however, are a shining black; the same is true of the exterior tail feathers, excepting their ends. About the head also there is considerable glossy black which runs down the sides of the neck and becomes the sole color of the throat and upper breast.

White, which appears on the face, is seen on the underparts sometimes rimmed with a streak of black, as on the breast and abdomen, sometimes intermixed with it as on the tail and wing coverts. The sides of the body are of a ruddy tinge.

The general color of *Eupetes nigricrissus* above, including the tail and wings, is bluish, becoming dark, almost black towards the wing extremities, with bluish margins. On the face, especially about the eye there is much black; a band of the same runs around the neck, bordering the pure white throat. White spots the cheeks, also enclosed by black. The under parts are a slate color, with a bluish cast; this is true as well of the tail and under wing. Length 8 inches. The female is similar though a little smaller. The male lacks the clear stripe of white above the eye, which the female possesses. Habitat, Southeastern New Guinea.

Of the *Drymoedus*, a group allied with the *Eupetes*, a species named *Drymoœdus beccarii* is the inhabitant of Southern New Guinea and the neighboring islands. The color of this pretty bird is a warm brown above, the head darker, the wings pale brown and black with white tips. The tail is similarly marked. White and black markings diversify the side face about the eye. The rest of the face and throat are clear white. The under parts are a buff, more or less variable; the crissum a dark brown. As on the wings above, so below the coloration contains bars of white in addition to the dusky brown. The bill is black. The length is about 7 inches.

Another bird of kindred species and not very unlike in plumage is *Orthonyx novæguineae.* In this case, however, the white on the under surface is far more extended. This hue is intruded upon by brown and black. The white above is less developed.

Pomatorhinus isidorii of the same family does not differ greatly in appearance. It is rather longer than the preceding and of a prevailing brown or russet, shaded more or less. Its length is about 8 inches. The female is like the male, perhaps a trifle larger in size.

A much smaller genus of birds is *Crateroscelis*, represented in New Guinea by two species, *C. murina* and *C. monarcha.* Here the ground color is still brown, brighter on the tail, darker on the head. Even the throat which is white is slightly tinged. So, too, the abdomen and lower parts generally. Total length 4.5 inches. The latter species has more white upon the under body, otherwise is mainly like the preceding.

RECENT LITERATURE.

Murray's Introduction to the Study of Sea-Weeds.[1]—In this work from the press of Macmillan & Co., George Murray has given us a book which will be of much service to those beginning the study phycology. The introduction treats briefly of the history of phycology, of the geographical and littoral distribution, and the structure of sea-weeds, and there is appended thereto some valuable information on the collection and preservation of material. Following the introduction there is given a well selected list of eighty books and papers on phycology. The book is illustrated by eight full paged colored plates—four on the red, two on the green and two on the brown sea-weeds—and eighty-eight figures in the text. The figures in the colored plates are somewhat crowded, and the specimens figured are in some cases rather

[1] An Introduction to the Study of Sea-weeds, by George Murray, F. R. S. E., F. L. S., Keeper of the department of Botany, British Museum. With eight colored plates and eighty-eight other illustrations. London, Macmillan & Co., and New York, 1895, 271 pp., 12 mo.

fragmentary, but the figures in the text are very good. Most of them having been taken from the recent works of Retuke, Solms-Lauback and the author.

Five sub-classes are recognized, i. e., *Phæophyceæ*, *Chlorophyceæ*, *Diatomaceæ*, *Rhodophyceæ* and *Cyanophyceæ*. The general arrangement of the book is poor; the more complex groups are treated of first and the simpler last, except in the *Rhodophyceæ*, where the reverse order is followed. The *Rhodophyceæ* moreover "present so many difficulties to be understood only after the study of other groups that the author has chosen the *Phæophyceæ* with its familiar forms of sea-wracks and tangles for the first sub-class. The *Chlorophyceæ* and *Diatomaceæ* follow naturally. The *Rhodophyceæ* next make a series by themselves, and finally, come the simple *Cyanophyceæ*. In the *Phæophyceæ* seventeen orders are recognized which are the same as those of Kiellman in Engler and Prantl's *Pflanzenfamilien* with a few exceptions. *Spermatochnus* is placed in the *Sporochnaceæ* and *Myriotrichia* in the *Elachistaceæ* instead of each standing in an order by itself; the *Dictyoteæ* are placed between the *Cutlereaceæ* and *Tilopteridaceæ* instead of being left out altogether; the *Ralfsiaceæ* are placed near the *Sphacelariaceæ* instead of near the *Laminariaceæ* as they have been by Kiellman and others. *Splachnidium*, a monotypic genus found only in the southern oceans, which has until recently been included among the *Fucaceæ*, is placed in an order by itself—the *Splachnidiaceæ*. It has been found that the conceptacles of *Splachnidium* contain sporangia similar to those of the *Laminariaceæ* instead of oospores and antheridia, hence it is placed near that order. The marine *Chlorophyceæ* are treated under eleven orders; many recent facts as to their reproduction being incorporated. At the end of two groups, the *Pereclineæ* and the Coccospheres and Rhabdospheres are briefly mentioned as being on the borderland between the vegetable and animal kingdom. In the twenty pages devoted to the *Diatomaceæ*, the structure, reproduction, geographical and geological distribution are quite fully discussed, but nothing is said of the arrangement of the groups and very little of its systematic position. We can agree with the author that the diatoms should not be placed in the *Phaeophyceæ* solely because they have a coloring matter closely related to that of the brown sea-weeds, but we can hardly agree that a siliceous covering and the presence of diatomine are sufficient to separate so widely two groups otherwise so closely related as the diatoms and desmids.

According to the preface "the account of the *Rhodophyceæ* is based on the scattered papers of Schmitz, who by utilizing his own researches

and the splendid investigations of Thuret and Bornet, has almost wholly altered the classification of the sub-class." Four orders are recognized, based upon the development of the cystocarp; the *Menalionaceæ, Gagartinaceæ, Rhodomenaceæ, Cryptonemiaceæ.* The *Bungiaceæ*, including *Perphyra*, are placed at the end of the *Rhodophyceæ* as an *Anhang*. In the last ten pages the *Cyanophyceæ* are briefly treated under two orders, the *Nostocaceæ* and *Clerocaccaceæ.* Throughout the work each order and in the larger orders each family is synoptically treated under four heads; general character, thallus, reproduction and geographical distribution. In it are embodied the results of the latest investigation on all groups, much having been taken from the able investigations of the author and his associates. Errors are comparatively few, one of the most noticeable being the mentioning of genus *Egregia* as one of the Fucaceae (P. 55). It is again mentioned in its proper place among the *Laminariaceae* (P. 85).

De Alton Saunders.

Taxonomy of the Crinoids.—The true position of a science in the scale of progress is measured by the degree of perfection exhibited in the systematic arrangement of the phenomena of which it treats. Its claims to philosophic recognition are proportional to the accuracy of the genetic relationships shown in its system of classification. If this be true of a general science, it is no less a reality in its various departments. There is, perhaps, nowhere a better exemplification than the Crinoids; and no zoological group has made in recent years more rapid progress towards a rational classification.

The data upon which the systematic arrangement of the stemmed echinoderms rests are elaborately set forth in the lately issued work of Messrs. Charles Wachsmuth and Frank Springer.[2] It is of great interest to know that the advancement in an understanding of the group has been almost wholly from the paleontological side and that the results are accepted practically without change by the most eminent students of the living forms. As is well known, the crinoids are to-day almost extinct; but that in past geological ages they were the most prolific forms of life. On account of the peculiar construction, unusually great opportunities are afforded for the solution of morphological problems, and full advantage has been taken. Upon so firm a foundation does the classification of the crinoids, as prepared by Wachsmuth and Springer now rest, that it is hardly probable that it will require radical change for a century to come.

[2] North American Fossil Crinoidea Camerata: Memoirs Museum Comp. Zool., 2 parts, 800 pp., and atlas of 83 plates. Cambridge, 1895.

As regards the major subdivisions of the stemmed echinoderms three groups are recognized: the cystids, the blastoids, and the crinoids. These are considered as groups of equal rank. The forms of the first are earliest in time, lowest in taxonomic position, and are regarded as the ancestral types of the other two. The crinoid type itself is a very old one, dating from the Cambrian in which it is even then in a high stage of development. During the Ordivician the cystidian features had almost wholly disappeared. The crinoidal group is remarkable for the persistence it has shown in preserving its pentamerous symmetry; and although the introduction of the anal plate so disturbed it as to well nigh produce a permanent bilateral arrangement, the former was finally permanently retained.

Neocrinoidea and Palæocrinoidea, the two primary groups of crinoids which were formerly almost universally recognized, are abandoned. In their stead are recognized three principal subdivisions: Inadunata, Camerata and Articulata. It is quite remarkable that this ternate grouping of the crinoids is essentially the same as Wachsmuth originally proposed more than twenty years ago, and that often being compelled by students of the recent forms to abandon it and to substitute others, a careful survey in the light of recent discoveries of all crinoids both fossil and living has clearly shown that the main subdivisions first suggested are essentially valid and are applicable to all known forms. The criteria for separating the crinoids into orders are briefly as follows:

1. Condition of arms, whether free above the radials or partly incorporated in the calyx.

2. Mode of union between plates of the calyx, whether movable or rigid.

3. Growth of stem, whether new plates are formed beneath the proximal ring of the calyx or beneath the top stem joint.

The simplest forms, the Crinoidea Inadunata, have the dorsal cup composed invariably of only two circlets of plates or three where infrabasals are present; there are no supplementary ossicles except an anal piece, which is, however, not always present; the arms are free from the radials up. In the construction of the ventral disk two different plans are recognizable, and upon these are established two sub-groups, the Larviformia and Fistulata. The former has the disk in its simplest possible form, being composed of five large orals arranged in a pyramid; the second has the ventral side extended into a sac or closed tube often reaching beyond the ends of the arms.

The Camerata are distinguished by the large number of supplementary pieces which bring the proximal arm plates into the calyx, thus enlarging the visceral cavity; all plates are heavy and immovable; the mouth and food grooves are tightly closed.

The Articulata have to some extent the incorporation of the lower arm plates with the calyx, but the plates are movable instead of rigid. The mouth and food grooves are open. The infrabasals are fused with the top stem joint which is not the youngest plate of the stalk. According to whether or not pinnules are present two suborders are recognized: the Pinnata and Impinnata.

An analytical synopsis of the families of Camerata as proposed by the authors and as now understood is as follows:

I. Lower brachials and interbrachials forming an important part of the dorsal cup.

A. Interradials poorly defined.

The lower plates of the rays more or less completely separated from the primary interradials by irregular supplementary pieces; dicyclic or monocyclic — Reteocrinidæ.

B. Interradials well defined.

1. Dicyclic.

a. Radials in contact except at the posterior side — Thysanocrinidæ.

b. Radials separated all around — Rhodocrinidæ.

2. Monocyclic.

a. Radials in contact all around.

Symmetry of the dorsal cup, if not strictly pentamerous, disturbed by the introduction of anals between the brachials only — Melocrinidæ.

Arms borne in compartments formed by partitions attached to tegmen; dorsal cup perfectly pentamerous; plates of calyx limited to a definite number — Calyptocrinidae.

b. Radials in contact except at the posterior side, where they are separated by an anal plate.

First anal plate heptagonal, followed by a second between two interbrachials — Batocrinidae.

First anal plate hexagonal, followed by two interbrachials without a second anal, arms branching from two main trunks by alternate bifurcation — Actinocrinidae.

II. Brachials and interbrachials slightly represented in the dorsal cup.

1. Dicyclic,

Radials in contact except at the posterior side — Crotalocrinidae.

2. Monocyclic.

a. Radials in contact all around; base pentagonal — Platycrinidae.

b. Radials separated at posterior side by an anal plate; base hexagonal.

Basals directly followed by the radials — Hexacrinidae.

Basals separated from radials by accessory pieces — Acrocrinidae.

Regarding the terminology employed, special attention should be called to the clear and concise definitions given of the various structural parts. The terms should be universally adopted, and they form

by far the best collection ever proposed. American writers especially will need no appeal to at once use them, not only in order to secure uniformity in nomenclature but to insure precision of description. Heretofore the names of the various plates or groups of ossicles have been used in a rather haphazard way. Not only have different designations been given to the same part but the same title has been repeatedly applied to structures widely separated morphologically.

CHALES R. KEYES.

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General Notes.

PETROGRAPHY.[1]

Examples of Rock Differentiation.—Yogo Peak in the Little Belt Mountains, Montana, consists of a stock of massivei gneous rock which breaks up through surrounding horizontal sediments, that have been metamorphosed on their contact with the eruptive. A vertical section through the south face of the mountain caused by a branch of Yogo Creek has afforded Weed and Pirsson[2] and excellent opportunity to study the relations of different phases of the eruptive to one another. The massive rock shows a constant variation and gradation in chemical and mineralogical composition along its east and west axis which is two miles in length. In its eastern portion the rock is a syenite, containing pyroxene, hornblende, biotite, orthoclase, oligoclase, quartz and a few accessories. The pyroxene is a pale green diopside and the hornblende a brownish-green variety. The latter is thought to be paramorphic after the former. In structure the syenite is hypidiomorphic with a

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Amer. Journ. Sci., Vol. L, 1895, p. 467.

tendency toward the allotriomorphic structure. Further west, about in the center of the mass, the syenite changes to a darker gray rock with a tinge of green, somewhat resembling a diorite. It is more coarsely crystalline than is the syenite and is much more basic. The minerals are the same as in the syenite, except that quartz is lacking, but differ somewhat in their character and in the proportions present in the two rocks. The augite is now a bright green idiomorphic mineral. Hornblende is rare and biotite abundant. The great difference between this rock, which the authors call yogoite, and the syenite, is in the relative proportions of augite and orthoclase present in them. In the yogoite the pyroxene predominates over the orthoclase, while in the syenite the reverse ratio exists. In the western portion of the rock mass, the prevailing type is shonkinite, a very dark basic rock, very similar to that of Square Butte.[3] Augite and biotite are very abundant as compared with the orthoclase, which in turn predominates over plagioclase. This latter mineral is represented by andesine, a more basic feldspar than that in either the syenite or the yogoite. Analyses of the three types of Yogo Peak rocks follow:

	SiO_2	TiO_2	Al_2O_3	Cr_2O_3	Fe_2O_3	FeO	MnO	MgO	CaO	BaO	SrO	Na_2O	K_2O	H_2O	P_2O_5	
Syenite	61.65	.56	15.07	tr	2.03	2.25	.09	3.67	4.61	.27	.10	4.35	4.50	.67	.33	—
Yogoite	54.42	.80	14.28	tr	3.32	4.13	.10	6.12	7.72	.32	.13	3.44	4.22	.60	.59	—
Shonkinite	48.98	1.44	12.29	tr	2.88	5.77	.08	9.19	9.65	.43	.08	2.22	4.96	.82	.98	—

Shonkinite contains in addition .22 per cent. of Fl.

From a consideration of the nature of the three types of rock the authors conclude that the Yogo Peak stock exhibits the results of a progressive differentiation along its major axis. There is a progressive increase in the ferro-magnesian constituents from the east to the west and a consequent increase in basicity. All the components of the three types exhibit the effects of this differentiation in the proportions present in the different rocks. The Yogo Peak mass is thus an illustration of a "Facies suit" as distinguished from a "rock series." In the former differentiation took place in situ, whereas in a 'rock series' differentiation occurred before the eruption of rocks into their existing positions. The facies suit of Yogo Peak together with the rocks of neighboring mountains comprise a distinct rock series.

The authors close their paper with an appeal for a more specific nomenclature in petrography—a nomenclature that will take account not only of the qualitative relations between the minerals that make up rock masses but of the quantitative relations as well. The Yogo Peak

[3] Compare AMERICAN NATURALIST, 1895, p. 737.

rocks form a natural series with sanidinite and peridotites. Rocks composed of orthoclase and no augite = sanidinite; when orthoclase exceeds augite = augite-syenite; when orthoclase equals augite = yogoite; when augite exceeds orthoclase = shonkinite; when augite alone is present = pyroxenite and peridotite. In this scheme the term augite includes also other ferro-magnesian minerals, and the terms orthoclase other feldspars.

In connection with the article above referred to Iddings[4] mentions the existence of a series of rocks associated with typical basalts and andesites in the Yellowstone National Park. They represent like phases of differentiation belonging to separate, but similar rock families. Most all of these rocks are basaltic looking. They occur in flows and dykes and sometimes as breccias, constituting the major portion of the Absaroka Range. These rocks present a wide range of composition within definite limits, forming a series connected by gradual transitions. Three classes are distinguished, the first of which is characterized usually by abundant phenocrysts of olivine and augite and an absence of feldspar phenocrysts; the second class is characterized by the presence of labradorite phenocrysts in addition to those of olivine and augite, and the third class by the presence of labradorite phenocrysts. The names given to the three classes are absarokite, shoshonite and banakite. The distinctions between the classes is based principally upon their chemical relationships. A large number of analyses, most of which were taken from other papers, illustrate their points of difference. A comparison of the analyses, besides showing the close relationships existing between the rocks of the three classes, shows also what mineralogical differences may obtain for rocks of the same chemical composition. The shoshonite from the base of Bison Peak and the banakite from Ishawooa Canyon have practically the same chemical composition. The former, however, contains abundant phenocrysts of labradorite, augite and olivine, while the latter contains numerous labradorite phenocrysts, but few and small ones of the other two minerals. The groundmass of the first shows much less orthoclase than that of the second, and no biotite, which abounds in the second. The author compares the series of rocks studied by him with the series studied by Merrill[5], with the series discussed by Weed and Pirsson and with Brögger's[6] grorudite-tinguaite series. The conclusion reached by this comparison is to the effect that it may be doubted whether the gen-

[4] Journal of Geology, Vol. III, p. 935

[5] Cf. American Naturalist, 1896, p. 128.

[6] Cf. American Naturalist, 1895, p. 567.

etic relations between igneous rocks can properly mark the lines along which a systematic classification of them may be established.

Petrographical Notes.—In a phyllite-schist found in blocks on the south shore of Lake Michigamme in Michigan, Hobbs[7] has discovered large crystals of a chloritoid like that described by Lane, Keller and Sharpless in 1891. The rock in which the crystals occur is a mass of colorless mica scales through which are distributed large flakes of biotite, small blades of chloritoid, a few acicular crystals of tourmaline and grains of magnetite. Most of the chloritoid is in large porphyritic crystals imbedded in this matrix. The optical properties of the mineral correspond to those of masonite.

In a summary of the results of this work in the upper Odenwald Chelius announces the existence there of two granites—the younger a fine grained aplitic variety and the older a coarse grained porphyritic variety, with a parallel structure due to flowage. Pegmatitic veins that cut this granite are looked upon as linear accumulations of porphyritic feldspar crystals. Many notes are also given on the diorites, gabbros and basalts of the Odenwald, on the basic enclosures in the granite, which the author regards as altered fragments of foreign basic rocks, but nothing of a startling nature with reference to these subjects is recorded. A gabbro porphyry was found occurring as a dyke mass. It consists of phenocrysts of labradorite in a gabbro-aplitic groundmass.

In a general paper on the divisibility of the Laurentian in the Morin area N. W. of Montreal, Canada, Adams[8] describes the characteristics of the members of the Grenville series of gneisses, quartzites and limestones. The augen gneisses, the thinly foliated gneisses and the granulites of the series are all cataclastic or granulitic in structure. They are regarded as squeezed igneous rocks. The crystalline limestones and quartzites are recrystallized rocks that are thought to be changed sedimentaries. Pyroxene gneisses, pyroxene granulites and other allied rocks are of doubtful origin. In addition to all these rocks there is present in the series a group of peculiar banded garnetiferous gneisses which from their chemical composition are regarded as in all probability metamorphosed sedimentary rocks.

[7] Amer. Jour. Sci., Vol. L, 1895, p. 125.
[8] Amer. Jour. Sci., Vol. L, 1895, p. 58.

GEOLOGY AND PALEONTOLOGY.

The Paleozoic Reptilian Order Cotylosauria.—A paper was read before the American Philosophical Society, November 15, 1895,[1] by Prof. E. D. Cope, on the reptilian order Cotylosauria. The following is an abstract of the characters of the order.

Quadrate bone united by suture with the adjacent elements. Temporal fossa overroofed by the following elements: Postfrontal, postorbital, jugal, supramastoid, supratemporal, quadratojugal. Tabular bone present. Vertebræ amphicoelous; ribs one headed. Episternum present. Pelvis without obturator foramen.

This order is of great importance to the phylogeny of the amniote Vertebrata. The structure of the temporal roof is essentially that of the Stegocephalous Batrachia, while the various postorbital bars of the amniote Vertebrata are explained by reference to the same part of its structure.

The palatal elements in this order are more or less in contact on the middle line, and the pterygoids diverge abruptly from this point, and return to the quadrate. The occipital condyle is single, and does not include exoccipital elements (unknown in Elginia).

Intercentra are present in Pariasauridæ, Diadectidæ and Pariotichidæ, and they are wanting in Elginiidæ. The hyposphen-hypanterum articulation is present in the Diadectidæ, but is wanting in the Elginiidæ and Pariasauridæ.

The scapular arch is best known in Pariotichidæ, Pariasauridæ[2] and Diadectidæ. In the two former there is a T-shaped episternum, over which are applied the median extremities of the clavicles; and there are well-developed coracoid and praecoracoid. In Diadectidæ[3] (probably genus Empedias) the episternum is articulated by suture with the clavicles.

In the Proceedings of the American Philosophical Society, 1892, p. 279, in a paper on "The Phylogeny of the Vertebrata," I wrote as follows: "Moreover, the Pelycosauria and the Procolophonina have the interclavicle, which is an element of membranous origin, while in the Prototheria we have the corresponding cartilage bone, the episternum. This element is present in the Permian order of the Cotylo-

[1] See Proceedings Amer. Philos. Soc., Vol. XXXIV, 1896, p. 436.
[2] Seeley, Philos. Trans. Roy. Soc. London, 1888, p. 89; 1892, p. 334.
[3] Cope, Proceeds. Amer. Philos. Soc., 1883, p. 635.

sauria which is nearly related to the Pelycosauria." The examination of the sternal region in Pariotichus has led me to the conclusion that the episternum and interclavicle are present and fused together in that genus, and also to the belief that the episternum is present in the genus Procolophon. The structure is generally similar in the two genera, and I think that Seeley is in error in determining the element in question in Procolophon as the interclavicle only.[4] Gegenbaur pointed out in his *Comparative Anatomy* the different (*i. e.*, membranous) origin of the interclavicle of the Lacertilia, but he included it with the episternum under the same name. The true episternum is not present in the Lacertilia. It is present in the Sauropterygia and Testudinata and probably in all the orders with one postorbital bar, or Synaptosauria, while it is wanting in most or all of the Archosaurian series, and in the Squamata. Whether the element I have referred to in the genus Naosaurus as interclavicle, is that element or the episternum, must remain uncertain until I can see it in place. Its edges are thin, as in the interclavicle of the Lacertilia. Of course, the Reptilian order which is in the line of ancestry in the Mammalian will have an episternum and not an interclavicle only. The Stegocephalia among Batrachia possess an episternum, with, perhaps, an adherent interclavicular layer as in the Testudinata.

Seeley describes four sacral vertebræ in Pariasaurus. In Empedias there are but two. The pelvis is without obturator foramen. The humerus has an entepicondylar foramen. The tarsal and carpal elements are incompletely known.

There are palatine teeth in Empedias and Pariasaurus, but none in Elginia; vomerine teeth none.

The inferior surface of the cranium is known in Elginia, Pariasaurus, Empedias and Pariotichus, and has been described as to the first three genera by Newton, Seeley, and myself. Pariotichus displays generally similar characters. There is a pair of posterior nares, and a pair of zygomatic foramina, but no palatine foramen. The palatine elements meet on the middle line, but gape behind. The vomers (prepalatines) are distinct, and are well developed anterior to the palatines. The ectopterygoid is large and has a prominent posterior border. I have stated that in Empedias there are teeth on the vomer. Better preserved specimens of Pariotichus show that the teeth are really borne on the edges of the palatines, which are appressed on the median line in the former genus. Similar palatine teeth are present in Pariasaurus, but are wanting in Elginia. Teeth are also present on the posterior

[4] Philos. Transac. Royal Society, 1889, p. 275, Pl. IX, fig. 9.

edge of the ectopterygoids in Pariasaurus and Pariotichus, but not in Elginia or Empedias. A character of the American genera is the weakness of the attachment of the basioccipital to the sphenoid. The basioccipital is lost from the only known specimen of Elginia, and the sphenoid projects freely below it in Pariasaurus, The roof of the mouth in this order is a good deal like that of the Lacertilia, lacking the palatine foramen.

The order Cotylosauria was defined by me in the AMERICAN NATURALIST for 1880, p. 304, and in 1889 (October). In 1889 (Transac. Roy. Soc. London, p. 292), Prof. Seeley gave it the name Pariasauria. In my *Syllabus of Lectures on Vertebrate Paleontology* (1891, p. 38), I arranged the group as a suborder of the Theromora. In 1892 (*Trans. Amer. Philos. Soc.*, p. 13, Pl. I), I again regarded the Cotylosauria as an order, and described the characters of the skull in three of the genera, and gave figures of them.

Seeley has objected to the reference of the genera Pariasaurus and Empedias to the same order, on the ground that the elements connecting the supraoccipital and the quadrate rest on the occipital elements in the latter, while they are elevated above them in the former. This character would not, however, define orders, as both conditions are found in Lacertilia; but might distinguish families within an order. However, Seeley's description and figure of the occipital region in *Pariasaurus bainii*[5] show that the structure only differs from that of the Diadectidae in the presence of a large foramen between the supraoccipital and exoccipital bones on each side.

The known species of the Cotylosauria range in dimensions from that of the South American Caimans (Chilonyx, Pariasaurus sp.) to that of the smaller Lacertilia, e. g., *Eumeces quinquelineatus* (Isodectes and Pariotichus sp.). They range from the Coal Measures to the Trias, inclusive, and have been found in South Africa, North America and Scotland. A single genus has been found in the Coal Measures of Ohio, which is represented by a species which I called *Tuditanus punctulatus*.[6] It is of small size, and as the maxillary teeth are of equal length, I cannot distinguish it from Isodectes, which belongs to the Pariotichidæ. The other species which were referred to Tuditanus are Stegocephalia.[7] *This is the first identification of a true reptile in the Coal Measures.*

[5] Philos. Transac. Roy. Soc. 1892, p. 326, Pl. XVIII, Fig. 2.

[6] Transac. Amer. Philosoph. Society, April, 1874, separate p. 11. Report Geol. Survey of Ohio, 1875, Paleontology, p. 302, Plate XXIV, fig. 1 (erroneously named in explanation *Tuditanus longipes*).

[7] Proceeds. Amer. Philos. Soc., 1871, p. 177.

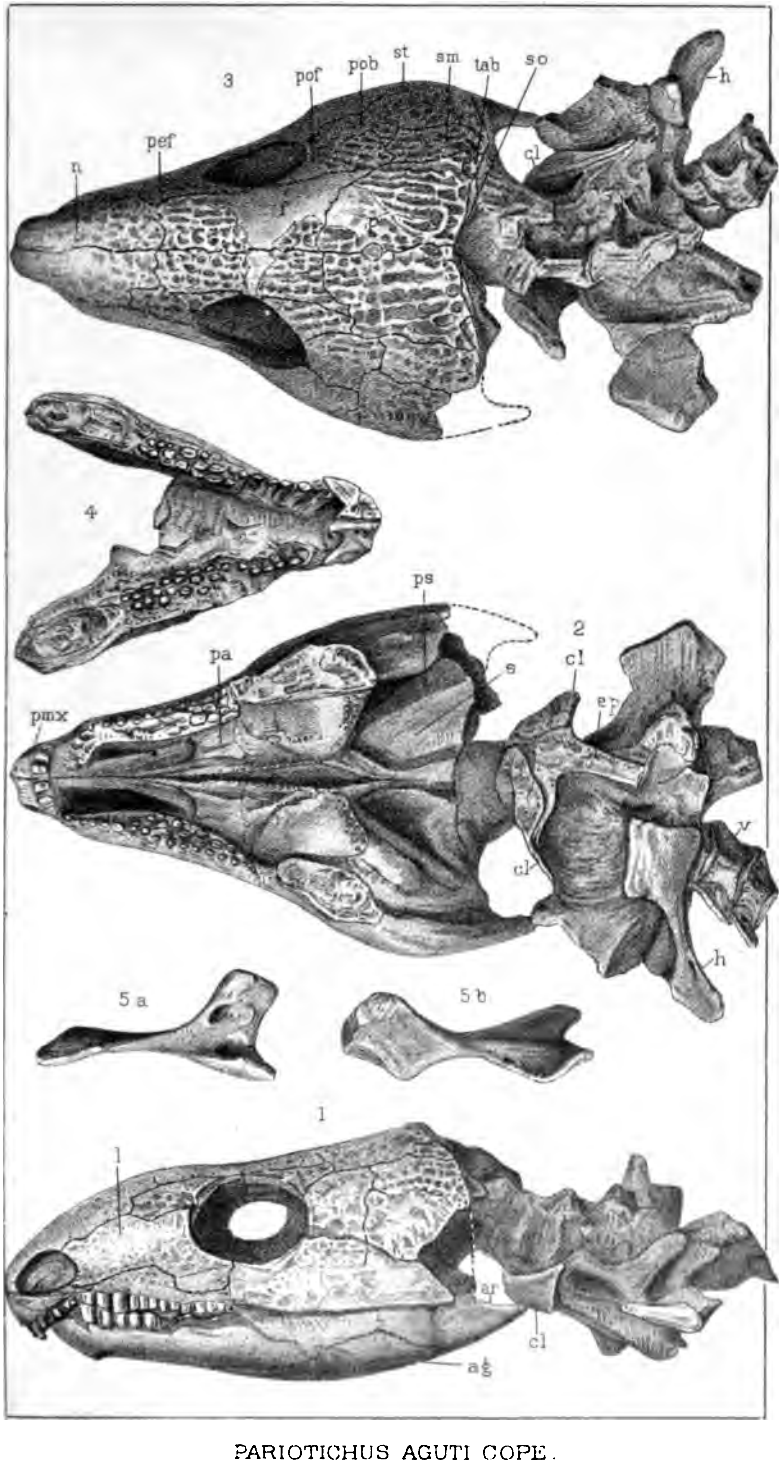
This order embraces, at present, four families, comprising 24 species distributed among 12 genera, as follows: Elginiidae, 1 genus, 1 species; Pariasauridæ, 3 genera, 7 species; Diadectidæ, 5 genera, 9 species; Pariotichidæ 6 genera (of which 3 are new, viz.: Isodectes, Captorhinus and Hypopnous), and 12 species, of which 5 are new. Total, 29 species, 15 genera.—E. D. Cope.

EXPLANATION OF PLATE VIIa.

Pariotichus aguti Cope. From the Proceeding Amer. Philos. Society, November, 1895. Fig. 1, Skull, from side. Fig. 2, Skull, with angular parts of mandible adherent, cervical vertebrae and scapular arch, from below. Fig. 3, Skull, from above, with cervical vertebrae. Fig. 4, Anterior two-thirds of mandibular arch, with adherent premaxillary bones, from above. Fig. 5, Humerus. *N.*, Nasal bone; *F.*, Frontal; *Pef.*, Prefrontal; *Pof.*, Postfrontal; *P.*, Parietal; *Pmx.*, Premaxillary; *Mx.*, Maxillary; *J.*, Jugal; *Qj.*, Quadratojugal; *St.*, Supratemporal; *Sm.*, Supramastoid; *Tab.*, Tabulare; *So.*, Supraoccipital; *V.*, Vomer; *Pa.*, Palatine; *Par.*, Paroccipital; *Ecp.*, Ectopterygoid; *Ps.*, Pterygoid; *Q.*, Quadrate; *Cs.* Clavicle; *Ep.*, Episternum; *H.*, Humerus.

The Puget Group.—Sir Wm. Dawson confirms the opinion advanced by Dr. G. M. Dawson in 1890 that the formation in the north-western part of the United States to which the name Puget group has been given, extends into British Columbia as far as Burrard's Inlet. This great estuarine deposit extends southward as far as the Columbia River and from the coast line to the Cascade range, within which its beds rise to a height estimated at from 800 to 5000 feet above the level of the sea. They overlie the Cretaceous Chico series in the United States, and its equivalent the Nanaimo formation in Canada. The latest views of paleobotanists and geologists of the United States seem to be that these beds are of Eocene age and that the fossil plants may be best compared with those of the Upper Laramie of the interior plains. In so far as Canada is concerned it has been established that the Upper Laramie beds underlie a formation containing animal fossils of the White River Miocene period, so there can be no doubt as to their Eocene age, and consequently of the Eocene age of the Puget group in Canada.

A further confirmation as to this view of the age of the formation in question is found in a collection of fossil plants from the vicinity of Burrard Inlet. These were referred to Sir Wm. Dawson for identification who sums up the results of his study as follows:



PARIOTICHUS AGUTI COPE.

"A comparison with the flora of the Upper Cretaceous Nanaimo series shows that the Burrard Inlet species are distinct and of more modern aspect. On the other hand, they are also distinct from those of older Miocene deposits of the Similkamen district and other parts of the interior of British Columbia. Between these they occupy an intermediate position; in this reseect corresponding with the Laramie of the interior plains east of the Rocky Mountains. They also resemble this formation in the general facies of the flora, which is not dissimilar from that of the Upper Laramie or Fort Union group."

"We may thus refer these plants to the Paleocene or Eocene, and regard them as corresponding with those of the Atanékerdluk beds in Greenland, the lignitic series of the McKenzie River, and the beds holding similar plants in Alaska."

"This flora thus serves to fill the gap in our western series of fossil plants, namely, that between the Cretaceous and the Lower Miocene." (Trans. Roy. Soc. Can. (2), Vol. I, 1895–'96.)

The Geological Structure of Florida is according to Prof. E. T. Cox, remarkable for its simplicity. The underlying rock is a soft limestone of Upper Eocene age; resting on this are beds of phosphate of lime; and covering the phosphate and limestone is a bed of sand that varies from a few inches to 20 feet and more in depth.

The Eocene limestone is filled with fossil marine shells. It shows no evidence of disturbance and is without a trace of stratification. It has an amorphous structure and is of unknown thickness. The phosphate of lime occurs in detached masses scattered over an area about 20 miles wide, and exending in a belt, follows in general way the trend of the Gulf coast from the northern limits of the state and beyond, to the western edge of the Everglades on the south. The author believes the phosphate to be the result of the mineralization of guano.

The covering of sand is found all over the Peninsula. It has been blown by the winds from the gulf and ocean beaches. Mixed with the sand is clay in the form of fine dust. In several localities the associated clay has been separated from the sand by running water and deposited as kaolin. This kaolin has been tested and found to be of superior quality for the manufacture of the finest porcelain.

Florida is not a level plain. A ridge from 30 to 50 miles wide extends from the northern part of the state to the Everglades, having an elevation of more than 230 feet in some places. From this ridge the land slopes to the Atlantic on the east and the Gulf on the west.

The elevation of the Peninsula was due to that continental force, extended over a vast period of time, which brought the tops of the Rocky

Mountains above the waters of the Pacific. (Trans. Amer. Inst. Mining Engineers.)

The Eocene age of part of Florida was first asserted by Prof. Eugene Smith of Alabama, and this conclusion was confirmed by paleontologic data by Prof. Heilprin, of Philadelphia. Dr. W. H. Dall subsequently delimited exactly the area of these beds with the Neocene and Plistocene beds to the south, east and north of them.

Notes on the fossil Mammalia of Europe Pt. II.—*On the affinities of the Genus Tapirulus, Gervais.*—Tapirulus is one of those aberrant types, where we find a curious assemblage of characters, which to the systematist is a great annoyance, although to the morphologist most instructive.

A superficial examination of the teeth has lead some palæontologists to assign this genus a position near the Tapir. Gervais[1] established this genus on the characters of the lower true molars.[2] He referred *Tapirulus* to the family *Anoplotheriidæ*, which I shall endeavor to prove is its proper position, although this reference on his part I believe was accidental, as he placed in the same family the genus *Adapis*. Gaudry[3] has assigned *Tapirulus* a position near the genus *Tapirus*, and Zittel[4] referred it to the *Suidæ*.

Through the great kindness of Prof. Albert Gaudry, who has so generously allowed me to study so many of the beautiful specimens in the collection of the Jardin des Plantes, I have had the opportunity of examining a skull of *Tapirulus*, in which the greater part of the upper dentition is preserved. On examination of this skull I was at once struck by its close resemblance to that of *Anoplotherium*, and *Dacrytherium*.

The skull in *Tapirulus* is slender and much elongated, the dorsal contour is nearly straight, and the facial portion is strongly compressed and slender. There is no preorbital fossa, as in *Dacrytherium*, and the occiput is high and narrow, like that of *Anoplotherium*. The auditory region very closely resembles that of *Dacrytherium*, the paroccipital process is long, slender and the posttympanic and glenoid processes are applied closely to the external auditory meatus. The brain case in the *Anoplotheriidæ* is much extended anteroposteriorly, being about one-half the total length of the skull in *Dacrytherium*. In *Tapirulus* the

[1] Comptes Rend. Acad. Sci., 1850, p. 604.

[2] Zoologie et Palæontologie Francaise, p. 173, pl.

[3] Journal de Zoologie, XIV, 1875, p. 5.

[4] Traité de Palæontologie, 1894, p. 338.

cranial portion of the skull is shorter relatively than in *Dacrytherium* and with this decrease in length I observe a greater development of the anterior part of the brain case, which encloses the frontal lobes of the brain. In short the brain of *Tapirulus* as compared with the size of the skull, must have been much larger than in *Dacrytherium*, and this greater development effected especially the frontal region of the brain.

As the Suilline type of skull was well established in the Phosphorites or Upper-Eocene of France (*Cebochœrus*), I shall compare the cranium of *Tapirulus* with that of *Cebochœrus*. In the latter genus the brain case is very much reduced with the extremely prominent zygomatic arches; sagittal and lambdoidal crests are very heavy and the occiput is broader than high. The cranial portion of the skull in *Cebochœrus* is much heavier and broader than the facial. All these cranial characters in *Cebochœrus* differ decidedly from those of *Tapirulus* and I enumerate them, so as to prove that *Tapirulus* has no direct affinity with the Suillines. The skull of *Tapirulus* somewhat resembles that of the primitive Selenodonts, (*Cœnotherium*), but it is more slender and its general form more closely resembles that of the *Anoplotheridæ*.

It is, however, the peculiar structure of the teeth, which is the most important consideration in studying *Tapirulus*. It is upon the characters of the teeth that *Tapirulus* has been assigned its various positions in the Ungulata. On a superficial examination of the upper true molars of *Tapirulus*, they exhibit a certain resemblance to those of the Tapir, but studied in detail I shall endeavor to prove that the molars of *Tapirulus* differ fundamentally in their plan from any of the known Lophiodonts. In the first place the external lobes of the superior molars in *Tapirulus* are concave, and not convex as in the true Tapirine molar. Again the transverse crests are straighter and their relation to the external lobes differ from those of the Tapirs tooth. At the junction of the transverse crests and the external lobes there is a strong notch, and in one specimen I can detect a faint trace of the intermediate tubercles.

The superior molars of the *Anoplotheriidæ*. as is well known, have deeply concave external lobes, the protoconule is distinct from the protocone, the latter element being in its primitive bunodont condition. In *Anoplotherium* and *Dacrytherium* the hypocone is selenodont in structure. In other words the form of superior molar found in the *Anoplotheridæ* is a slight modification of the buno-selenodont type, it differs from the exact form of this type in having the hypocone crescentoid.

Now in *Tapirulus* the two external concave lobes of the superior molars have nearly the same structure as those of *Dacrytherium*, and the internal primitive bunodont elements of the crown, have been transformed into crèsts, this occurred before the *Anoplotherium* type of molar had reached its present stage of evolution. It is then not difficult to derive the form of superior molar found in *Tapirulus* from the true buno-selenodont type, and I believe this to have been the origin of the peculiarly modified molar occurring in *Tapirulus*.

The superior and inferior premolars in *Tapirulus* have the same elongated form so characteristic of the *Anoplotheriidæ* in general, and like the latter group, the canines were not differentiated in form from the anterior premolars. The lower true molars of *Tapirulus* depart widely in structure from those found in any of the known genera of the *Anoplotheridæ*, although like the upper molars they are much specialized and can be derived from a less modified type, which was the common stock of both *Tapirulus* and *Anoplotherium*. A peculiarity in the structure of the lower molars of *Tapirulus* is the presence on each tooth of a well developed third lobe, hypoconulid, which projects posteriorly a considerable distance. The portion of the molar crown anterior to the hypoconulid consists of two high transverse crests, the antero-external termination of the front crest exhibiting a rudiment of the anterior spur, which is so largely developed in all the other genera of the *Anoplotheridæ*. The lower jaw in *Tapirulus* is long and extremely slender and its form closely resembles that of *Dacrytherium*.

In conclusion I believe the natural position of *Tapirulus* is in a subfamily of the *Anoplotheriidæ*, and that both *Tapirulus* and *Anoplotherium* have been derived from a common stock, the ancestral form having had the pure type of buno-selenodont molar. As already shown above, the resemblance in structure of the molars of *Tapirulus* to that of the Tapir is not exact, the Tapirs tooth having been derived direct from the bunodont type (See *Euprotogonia* and *Systemodon*).

III.

On the validity, and systematic position of Mixtotherium Filhol.—The genus *Mixtotherium*[5] is another interesting form, not as aberrant in its characters as *Tapirulus*, but which unites in a certain degree the Suillines and the Anoplotheroids. Prof. Zittel in his "Traité de Palæontologie" does not consider *Mixtotherium* as a good genus, and refers it to the milk dentition of *Diplobune*. I am quite confident that Prof. Zittel is mistaken in this determination, and I shall endeavor to

[5] Mém. quelques Mam. fossiles, Toulouse, 1882.

prove that *Mixtotherium* is a valid genus and entirely distinct from either *Diplobune* or *Dacrytherium.*

The genus *Adiotherium* was described by Filhol in 1884. This genus is referred by Zittel to the milk dentition of *Dacrytherium,* I can not agree with Prof. Zittel on this reference either, as I believe *Adiotherium* to have been based upon the milk dentition of *Mixtotherium.*

The skull in *Mixtotherium* is essentially Suilline, but exhibiting some characters like those of the Anoplotheroids. The form of the brain case is longer and narrower than in *Cebochœrus,* and it closely resembles that of *Acotherulum* which is one of the most primitive of the early Suillines of Europe. The occipital region of the skull in *Mixtotherium* is much broader than high, and is not constricted in the middle as in *Dacrytherium;* the occiput has nearly the exact form of *Acotherulum* and *Cebochœrus.*

In the primitive pigs of the Phosphorites the auditory bullæ are extremely small, in *Mixtotherium* they are large. The basioccipital region of the skull in *Mixtotherium* is rather long and narrow, and like that of *Dacrytherium.* In *Cebochœrus* of the Phosphorites, the peculiarly elongated and constricted snout of the pigs is well differentiated, however, in *Mixtotherium* as well as *Acotherulum* the facial region of the skull is broader and shorter, its form being more as in *Dacrytherium.* *Mixtotherium* agrees with *Diplobune* and differs from *Dacrytherium* in lacking a preorbital fossa in the maxillary bone. The general form and proportions of the skull in *Mixtotherium* is very much like that of the peculiar American genus, Oreodon.

The dentition of *Mixtotherium* resembles that of the *Anoplotheridæ* in the absence of any diastemas, it differs, however, from this family in the large size of the canines, which in form resembles more those of the Suillines. The superior premolars are normal in form, and not elongated as in the Anoplotheroids. The last upper premolar closely resembles in structure a true molar, it has two external cusps, which are intermediate in structure between the bunoid and selenoid forms. The deuteroconid forms a crest with the antero-intermediate tubercle, the tetastoconid is present, but small and bunoid in structure. The structure of the superior molars of *Mixtotherium* differ from those of *Diplobune* and *Dacrytherium* in the following details; the external crescents are united externally by a prominent mesostyle, which is more constricted than in *Diplobune;* in *Dacrytherium* this portion of the molar is open widely internally.

In the *Anoplotheridæ* the protocone is distinct from the protoconule, whereas in *Mixtotherium* these elements are united and form a well

developed protoloph. In *Mixtotherium* the hypocone is selenoid in structure as in *Dacrytherium*, but this cusp is much smaller and it is much less extended internally than in that genus. I emphasize especially the large development of the mesostyle, and the presence of a protoloph, characters of the upper molars of *Mixtotherium* which differs decidedly from those of *Dacrytherium*. The structure of the fourth upper premolar in *Mixtotherium* resembles somewhat that of *Agriochœrus*, but differs from this genus in the presence of the postero-internal cusp. In *Dichodon* Owen, the complication of the fourth upper premolar is carried still further than in *Mixtotherium*, as in *Dichodon* this tooth is completely molariform and selenodont in structure. However, I believe, that *Mixtotherium* has no close affinity with *Dichodon*, as the structure of the skull and dentition in *Dichodon* is quite modernized.

The lower jaw in *Mixtotherium* is rather short and deep below the last lower molar, these characters differ strikingly from those of the *Anoplotheriidœ*, where the jaw is very slender and elongated. The mandibulæ are strongly ankylosed at the symphysis as in the primitive pigs, *Acotherulum* and *Cebochœrus*, this is a character I believe seldom found in the Mammalia outside of the Primates. The last lower premolar in *Mixtotherium* is intermediate in structure between a last milk tooth and permanent molar. It consists of an antero-median cusp, bunoid in form, and posterior to it, of two external crescents and two flattened internal elements. The structure of the inferior true molars is like that of *Dacrytherium*.

It appears to me that the genus *Mixtotherium* is of importance phylogenetically, and demonstrates how closely the Suillines and Anoplotheroids are related. In the characters of the skull and the large development of the canines *Mixtotherium* is more like the pigs, but showing affinities to the Anoplotheroids in the form of the brain case. The structure of the molars, as already shown, resemble very closely those of the *Anoplotheriidœ* and have gone one step further in their specialization by the development of a well defined protoloph.

Schlosser in his paper, "Stammesgeschichte der Hufthiere" speaking of the origin of the Sullines remarks "die Herkunft diese Stammes ist noch in vollständiges Dunkel gehüllt, nur so viel dürfen wir als sicher annehmen, dass derselbe wohl von der gleichen Grundform ausgegangen ist wie der der Suiden." The Oreodonts are considered by Scott to be related to the Anoplotheroids, and if this be the case it is not strange that the skull of *Mixtotherium* resembles that of *Oreodon*. The genus *Protoreodon* of the Uinta or Upper Eocene, has the five lobed superior molar typical of the Anoplotheroids, and the primitive Suillines.

In conclusion, *Mixtotherium* is then a type intermediate between the Suillines and the Anoplotheroids, and has been derived from a common stock, which also gave origin very probably to the Oreodonts.—CHARLES EARLE, Laboratoire de Palæontologie, Jardin des Plantes, Paris.

The Glaciers of Greenland.—Prof. Chamberlin's report on the Geology of Greenland contains the results of his observations of glacier phenomena in the region explored by the Peary auxillary expedition of 1894. The seventeen glaciers visited fall into two classes designated the southern and northern types. The former are distinguished by ending in a slope of moderate declivity, the latter end in abrupt terminal walls which rise to heights of 50 to 150 feet. The author notes here that he is speaking of glaciers that end upon the land. Obviously, those that reach the sea terminate in vertical walls through the breaking away of the ends. Not only are the ends of the glacial tongues vertical, but in some instances the sides are so likewise. To some extent the edge of the ice-cap itself is vertical.

The stratification of these glaciers is remarkable for extent and definiteness. The ice is almost as distinctly bedded as sedimentary rock. The following points are noted by the author:

"In the vertical face there are usually presented two distinct divisions, an upper one of nearly white ice, whose laminations are not conspicuous, from lack of differential coloration, and a lower one discolored by debris, which gives great distinctness to the bedded structure. The lower divisions is divided by very numerous partings, along which are distributed rocky debris, embracing not only sand and silt, but rubble and boulders. Often the amount of this interspread debris is so sligbt as to constitute the merest film, while at other times it reaches a thickness of an inch or two. . . . . . In general, the rocky debris is arranged in very definite and limited horizons leaving the ice above and below as clean and pure as any other. It is very notable and significant that the ice next the debris layers is the firmest and most perfect that the glacier affords. The coarser debris is arranged in the same horizons with the fine silt and clay. . . . . . Where ice is well laminated, as it commonly is, the laminations bend under and over the embedded boulders. This seems to indicate that the embedded boulders do not descend through the ice by virtue of superior gravity, but are retained in the original position given them by the embedding process. The extent to which the basal portion of the ice is laminated is remarkable. In selected cases twenty laminations might be counted to the

inch. These laminations are sometimes symmetrical, straight and parallel. At other times they are undulatory, and in instances they are greatly curved and contorted in an intricate fashion."

It was observed that the debris bearing layers were parallel to the base of the glacier and were confined to its lower 50 or 75 feet, with some few exceptions. Even at the border of the glacier clean layers of white ice above the debris strata constituted one-third or more of the section. This is contrary to the view that the debris habitually works up to the surface and forms a layer there as it nears the border of the glacier.

Prof. Chamberlain was fortunate in being able to observe the process of introduction of debris in progress. At a point in the Gable glacier there was found an embossment of rock over which the ice was forced to pass and in so doing to rise in a dome-like fashion. One side of the dome was melted away, revealing operations at its base. Combining a number of observations, the author gives the following interpretation of the process:

"The bottom layer of the ice in passing over the crest of the embossment would be pressed with exceptional force upon it, and would, as a result, be especially liable to detach fragments from it and imbed them within itself. If debris were being pushed or dragged along between the ice and the rock surface beneath, it would be pressed into the ice and the ice compacted about it with exceptional force. As any given portion of the basal layer passed beyond the crest of the embossment, the vertical pressure would tend to cause it to follow down the lee slope, while the horizontal thrust of the moving ice would tend to force it straight forward. If any given portion yielded to the first and passed down the slope, it would produce a curve in the hardened basal layer of ice. As a result of this, the horizontal thrust, instead of continuing to act along the disadvantageous curved line, and against the superior friction of the bottom, would be disposed to cause the layer to buckle at the bend. The fold so formed would be elongated and appressed by the continuation of the process and become a layer. The ice, beneath, however, would gradually yield, and the debris layer would settle down out of the line of maximum thrust and the conditions for a new fold be induced."

Cases of true faulting and overthrust were seen, the rocky debris being carried along the fault plane.

As to the method of movement, Prof. Chamberlin presents evidence, which taken in connection with the intrusion and interstratification of earthy material, would seem to indicate that these glaciers move, in some notable part at least, by the sliding of one layer upon another.

Several instances were noted where the glaciers had advanced over their terminal moraines by riding up over them, but none where the ice showed any competency to push the frontal material, even its own debris, before it.

A driftless area was discovered on the east side of Bowdoin Bay immediately adjoining the present great ice-cap. (Bull. Geol. Club, Phila., 1895.)

BOTANY.[1]

New Species of Fungi.—The activity of our fungologists is indicated by the long lists before us which have been published within the last few months. From the Proceedings of the Academy of Natural Sciences of Philadelphia (1895, pp. 413 to 441) we have "New Species of Fungi from Various Localities," by J. B. Ellis and B. M. Everhart, including ninety-nine species. Many of these are from Colorado and other western regions. We note among the more interesting species the following, viz.: *Fomes alboluteus*, from an altitude of 10,000 feet, in Colorado; *Bovista cellulosa*, *Lycoperdon alpigenum*, both from Colorado, the latter from an altitude of 11,500 feet; *Rosellinia geasteroides* from Louisiana; *Phyllachora plantaginis*, parasitic on *Plantago rugelii*, in Wisconsin.

The same authors publish in the October (1895) *Bulletin of the Torrey Botanical Club*, a paper on "New Species of Fungi" in which there are described eight new species from the Sandwich Islands, eleven from Florida, and six from Mexico. It is with much pleasure that we observe that but two of the specific names are dedicated to persons, viz.: *Schizophyllum egelingianum* (probably a synonym for *S. commune*) and *Melogramma egelingii* from Mexico. It is to be hoped that the good example here set may be followed by others upon whom it falls to find names for new species.

In the Fourth Report of the Botanical Survey of Nebraska, just issued, fifty-five new species of fungi are described by Roscoe Pound, F. E. Clements and C. L. Shear. These are distributed as follows: in the *Mucoraceæ*, 1; *Sphærioideæ*, 1; *Mucedinaceæ*, 2; *Dematiaceæ*, 2; *Stilbaceæ*, 1 (in the new genus *Trichurus* of Clements and Shear); *Tuberculariaceæ*, 2; *Helvellaceæ*, 2, *Pezizaceæ*, 24; *Bulgariaceæ*, 1; *Agari-*

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

caceæ, 20. In the last named family the name *Gymnochilus* is substituted for *Psathyra* of Fries (1821) which must fall, since it is identical with Commerson's *Psathura* (Juss. Gen. ,1789). In the *Pezizaceæ* the name *Lachuea* Fries (1822), being identical with *Lachuæa* L. (Sp. Pl., 1753), must give way to *Sepultaria* Cooke (1879).—CHARLES E. BESSEY.

Alaskan Botany.—In the Contributions from the U. S. National Herbarium (Vol. III, No. 6), F. V. Coville makes a report upon the collections of plants made on Yakutat Bay, Alaska, in 1892, by Frederick Funston. Mr. Coville's paper is preceded by a Field Report made by Mr. Funston. The latter contains much interesting information as to the country and its vegetation. In regard to the latter the author says, "The plant life of the region about Yakutat Bay is characterized by the dense and vigorous growth of a comparatively small number of species, giving the forests especially an appearance of great sameness. The almost level country lying on the eastern side of the bay, between Ocean Cape and the foothills of the mountains, is covered with a forest growth practically impenetrable. The great amount of fallen timber, together with the tangled and heavy undergrowth constitute such obstacles to travel that even the Indians who have lived here many years have never penetrated the forests of the mainland for a mile from their own village. The great bulk of this forest is composed of the Sitka Spruce (*Picea sitchensis*), which in this region reaches a height of seventy feet. This tree extends from sea level to an altitude of 2,200 feet on the sides of Mt. Tebenkof; but as one follows the coast line up the bay from this mountain, the upper limit becomes lower and lower, until at the entrance of Disenchantment Bay it reaches sea level, the tree not being found on the shore of this bay. A large forest lies along Dalton Creek, and there are several of considerable extent between this place and Point Manby."

"The timber of the spruce tree plays a most important part in the economy of the natives, as from it are constructed their houses and canoes, and it is used in the manufacture of oil crates, bows, arrows and other implements, while the smaller roots after being boiled and split are used in basket weaving."

The other woody plants mentioned are the hemlock (*Tsuga mertensiana*), Sitka cypress (*Chamæcyparis nootkatensis*), red alder (*Alnus rubra*), a willow (*Salix barclayi*), the elder (*Sambucus racemosa*), the Menziesia (*Menziesia ferruginea*), high bush cranberry (*Viburuum pauciflorum*), the blueberry (*Vaccinium ovalifolium*), salmon berry

(*Rubus spectabilis*), devil's club (*Echinopanax horridum*), and black currant (*Ribes laxiflorum*).

The catalogue of species includes 159 species, of which 122 are Anthophytes; 3, Gymnosperms; 9, Pteridophytes; 25, Bryophytes. The ten largest families are as follows: *Rosaceæ*, 13 species; *Carduaceæ* (*Compositæ*), 10; *Poaceæ* (*Gramineæ*), 10; *Ranunculaceæ*, 9; *Saxifragaceæ*, 9; *Scrophulariaceæ*, 8; *Ericaceæ*, 7; *Polypodiaceæ*, 6; *Ammiaceæ* (*Umbelliferæ*), 6; *Brassicaceæ* (*Cruciferæ*), 5.—CHARLES E. BESSEY.

Aquatic Plants of Iowa.—R. I. Cratty has published in the Bulletin of the Laboratories of Natural Science of the University of Iowa (Vol. III, No. 4) some "Notes on the Aquatic Phenogams of Iowa," which will be useful in recording the past and present distribution of plants which are fast disappearing. It is a pity that author and editor permitted the antiquated spelling of Phanerogam to be used. There can be no valid excuse for "Phenogam." The species noted are *Arisaema triphyllum*, *A. dracontium*, *Symplocarpus fœtidus*, *Acorus calamus*, *Lemna minor*, *L. trisulca*, *L. polyrrhiza*, *Wolffia brasiliensis*, *Typha latifolia*, *Sparganium simplex*, *S. androcladum*, *S. eurycarpum*, *Naias flexilis*, *Zannichilia polustris*, *Potamogeton natans*, *P. amplifolius*, *P. nuttallii*, *P. lonchitis*, *P. heterophyllus*, *P. illinoensis*, *P. prælongus*, *P. perfoliatus*, *P. zosteræfolius*, *P. foliosus*, *P. major*, *P. pusillus*, *P. spirillus*, *P. pectinatus*, *Triglochin maritima*, *Scheuchzeria pulustris*, *Alisma plantago*, *Echinodorus rostratus*, *E. parvulus*, *Sagittaria arifolia*, *S. latifolia*, *S. rigida*, *S. graminea*, *S. cristata.*—CHARLES E. BESSEY.

Another Elementary Botany.—Professor MacBride has recently brought out a little book on botany for secondary schools, under the title of "Lessons in Elementary Botany," issued by the house of Allyn and Bacon of Boston. The author presents in small space essentially that phase of botany with which we have long been familiar in Gray's "Lessons," Miss Youmans's "First Book," "Second Book" and "Descriptive Botany," and Wood and Steele's "Fourteen Weeks in Botany." Whatever merits and demerits these works have are here reproduced, somewhat modified of course. The lessons begin with "buds," followed by "stems," "roots," "the leaf," "inflorescence," "the flower," "the fruit and seed." These topics occupy about eighty-five pages, and while the subject matter is essentially similar to that in Gray's "Lessons," the treatment resembles that of Youmans's books, considerably simplified. The pupil is required to work out the

details of structure by actual examination. The remainder of the body of the book (pp. 85 to 207) is taken up with selected plants whose structure is to be worked out, and here the treatment reminds one of Wood and Steele's book and the corresponding chapters in Miss Youmans's earlier books. There is here, however, a considerable improvement in the presentation of the matter, the pupil being led on by questions which direct attention to different details.

A valuable part of the book is found in the appendix, where directions are given for collecting and preserving materials for study. Taken altogether, the book is a good one, although we cannot agree with the author that gross anatomy alone, and that practically confined to the flowering plants, is all that can be done in the secondary schools. We prefer the work suggested by the Natural History Conference, as reported by the Committee of Ten, and know from much personal experience that the high schools are rapidly supplying themselves with compound microscopes, by means of which the pupils are obtaining some knowledge of the lower plants, and of the vegetable kingdom as a whole. Neither can we endorse what the author says in the preface as to the relative value to the pupil of a knowledge of the higher rather than the lower plants. But with all these criticisms it must not be thought that the book is a poor one; on the contrary, for schools where the conditions are such as the author describes and where they must so remain, the book is a very good one.—CHARLES E. BESSEY.

Botany in the United States Department of Agriculture.—From the recent Report of the Secretary of Agriculture, we glean the followings items, relating to the work in botany. Investigations for determining the strength of timbers of various species have been continued in the Division of Forestry, no less than 13,000 tests having been made during the year preceding the report. Measurements upon a large scale of the rate of growth of pine trees have been begun and some preliminary results obtained. Under this head the announcement is made of the establishment of experimental plantings at several points upon the Great Plains. In the Division of Botany the following announcement is gratifying to botanists. "The herbarium of the Department of Agriculture, commonly called the National Herbarium, having out-grown its old quarters, was, by kind permission of the Secretary of the Smithsonian Institution, removed and well installed in the fire-proof building of the National Museum, where it will be cared for by the botanists of this Department. This herbarium is steadily being built up and enlarged at the expense of the Department of Agriculture."

The new division of Agrostology, established during the current fiscal year has for its special work the scientific and economic study of the grasses and others forage plants. In connection with this work it is the purpose of the officers of the Division to establish "Experimental Grass Stations" in which the study of particular species may be more readily pursued.

The division of Vegetable Pathology "has been broadened during the year to include plant physiology," and the Secretary adds, "It is believed that this will add materially to the value of the investigations."

The abolition of the "Division of Microscopy" is announced. When first established, twenty years ago microscopy "was considered a separate branch of technology, but since that time the microscope has come into daily, almost hourly, use in nearly all scientific laboratories." The Secretary very properly concludes that a separate division is now "an absurdity."—CHARLES E. BESSEY.

Notes on Recent Botanical Publications.—From the Division of Botany of the U. S. Department of Agriculture we have John M. Holzinger's "Report on a collection of Plants made by J. H. Sandberg and Assistants in Northern Idaho in the year 1892." Some new species are described, viz., *Cardamine leibergii* (figured in Plate III as *C. sandbergii*), *Peucedanum salmoniflorum, Dicranoweisia contermina, Orthotrichum holzingerii, Bryum sandbergii*, and *Peronospora giliæ*.—Another contribution from the same source is the "Report on Mexican Umbelliferæ, mostly from the State of Oaxaca, recently collected by C. C. Pringle and E. W. Nelson," by John M. Coulter and J. N. Rose. As was to be expected, many new species were found in the collection. —With the preceding paper is a smaller one by J. N. Rose, entitled "Descriptions of plants, mostly new, from Mexico and the United States," the new species from the United States are *Ligusticum eastwoodæ*, from the La Plata Mts., Colorado; *Vlæa glauca*, from Oregon; and *Thurovia triflora*, a curious Texan composite for which a new genus had to be erected.—From the Field Columbian Museum we have C. F. Millspaugh's "Contribution to the Flora of Yucatan," which is marked "Botanical Series, Vol. I, No. 1," of the publications of this new centre of scientific activity. It includes the results of an expedition to Yucatan made in January, 1895, to which the author has added species compiled from Hemsley's *Biologia Centrali-Americana*.—M. E. Jones's "Contributions to Western Botany," published in the Proceedings of the California Academy of

Sciences, is mainly taken up with the new species discovered by him while acting as Field Agent for the U. S. Department of Agriculture. The author says "the long delay in the publication of the report necessitates the early publication of the new species." The author does not follow the "Rochester Rules" of nomenclature, and gives some reasons for not doing so, but the reader is amused to find under *Oxytropis acutirostris* (Watson) the remark "should it be necessary to reduce this genus to *Spiesia*, the name must be *S. acutirostris* (Watson)," and again under *Oxytropis nothoxys* (Gray), the synonym *Spiesia nothoxys* (Gray). For one who does not accept the "Rochester Rules" this is indeed a remarkable proceeding, since it is the deliberate addition of two synonyms (with "Jones" as the authority) to what the author calls "the mass of new names, nine-tenths of which are wholly useless."—K. C. Davis has issued a "Key to the Woody Plants of Mower County, in Southern Minnesota, in their Winter Condition" in the form of a five-page pamphlet. It will be useful in the region for which it is intended.—An interesting paper comes from Dr. G. Clautriau of Brussels, entitled Étude Chemique du Glycogène chez les Champignous et les Levures," from which we hope to make extracts in some future number.—CHARLES E. BESSEY.

VEGETABLE PHYSIOLOGY.[1]

Ambrosia.—By this name Schmittberger designated a soft watery substance found in the burrows of certain beetles and supposed to be of use in feeding the larvæ. The exact nature of this ambrosia appears to have been for a time in doubt, owing to the fact that it was generally seen by entomologists rather than by mycologists. Of late years, however, it has been conceded to be of fungous origin, although no one appears to have studied it critically. Since the appearance of Möller's book on the Fungous Gardens of South American Ants, the subject of ambrosia has received renewed attention. In this country, Mr. Henry G. Hubbard, who has long paid especial attention to the habits of coleoptera, has repeatedly observed this substance in the chambers of *Xyleborus pubescens* in orange trees in Florida, and has recently discovered it in the burrows of *Corthylus punctatissimus* in

[1] This department is edited by Erwin F. Smith, Department of Agriculture Washington, D. C.

the roots of whortleberry near Washington, D. C. Specimens from the latter source were submitted to various students of fungi in Washington last autumn for identification, and the writer had full opportunity to examine this substance. Some of the chambers were filled with it, others partly filled, and others free from it. It is a colorless much septate mycelium, inclined to be constricted at the septa, and in places consisting of rounded, nearly iso-diametric, colorless, rather thick-walled cells, not sufficiently differentiated from the mycelium to be considered as true spores. It appears to be the mycelial or oidial stage of some higher fungus, probably of some Ascomycete. From its distribution in the burrows and the behavior of the beetles toward it, there can be little doubt that it serves them for food. Whether like the ants they actually cultivate it, is another question and one more difficult to solve. In Germany, where this ambrosia was first discovered, Prof. R. Goethe, Director of the Royal Lehranstalt fur Obst-Wein-und Gartenbau zu Geisenheim am Rhein, has recently published an account of its discovery in the chambers of *Xyleborus dispar*. Prof. Goethe's brief note (p. 25, *Berichte d. Kgl. Lehranstalt* etc.) is accompanied by a good figure, judging from which the fungus appears to be the same as that found in the chambers of *Corthyllus punctatissimus* near Washington. This fungus is said to be the same as that found in 1883 in the burrows of *Xyleborus* in cherry trees at Kamp am Rhein. Concerning the use made of this fungus by the beetles he makes the following statement: Seine Wucherungen dienen ganz unzweiflehaft den Käfern zur Nahrung, denn man sieht deutlich, wie der Ueberzug stückweise abgeweidet wird. Further study of this subject would undoubtedly bring to light many interesting things. In the next number of the NATURALIST I hope to publish a note from Mr. Hubbard on this subject.—ERWIN F. SMITH.

White Ants as Cultivators of Fungi.—In connection with the preceding it may be worth while to reprint part of a note which appeared in *Grevillea*, June, 1874, p. 165–6, relative to the occurrence of fungi in the nests of termites in India. A writer in the *Gardeners' Chronicle* stated that he had never seen any fungi on or in nests of white ants except very small ones less than the size of a pin head. In opposition to this Mr. W. F. Gibbon, Doolha, Goruckpore, wrote to the Horticultural Society of India as follows: "I send you now a bottle containing mushrooms I extracted a few days ago from the center of a white ant hillock. When I collected them they were in appearance like asparagus, over 14 inches in length, and the people about here

consider them particularly good eating, partaking of them both raw and cooked. When I read the above article in your Society's Journal somewhat over a year ago, I was then aware that mushrooms existed in the interior of ant hills, for I had often seen them, but I did not know their season of sprouting, and whenever I searched was unsuccessful till the other day. I have now ascertained the season they sprout is the end of August or the beginning of September, and I believe all ant hills produce them. These mushrooms appear to me to proceed from a peculiar substance always found in ant hills in this country (whether white or black), generally called ants' food, a bluish gritty substance, like coarse wheat flour turned mouldy and adhesive. In dry weather brittle, and in damp weather like soft leather. It is this substance, under the combined influence of heat, damp and darkness from which the mushrooms grow. As my experience is at variance with the writer in the *Gardeners' Chronicle*, you may care to record it. * * * I would like these mushrooms, if possible, referred to some mycologist, and their names ascertained; and I would like also to know if the bluish substance, the ants' food, was collected and treated artificially, could similar mushrooms be raised." These mushrooms were submitted to Dr. D. D. Cunningham, who reported as follows: "I herewith return the letter sent to me more than a month ago, along with specimens of fungi said to have been procured from the interior of a white ant hill. The specimens apparently belong to some species of *Lepiota*, and are chiefly remarkable for the extreme length and coarse fibrous contents of the stem. The occurrence of fungi in connection with ant hills is well known, but in so far as I am aware, those hitherto described as occurring on the hills of the white ant belong to species of the Gasteromycetous order *Podaxinei*, so that the occurrence of a species of one of the sub-genera of Agaricus in such localities is a new and interesting fact. With regard to the material from which they arise, and which must apparently be of the same nature as the so-called spawn of the cultivated mushroom, consisting of vegetable debris permeated by the mycelium of the fungus, it may be noted that a similar substance is described by Belt as occurring in the nests of the leaf-cutting ants in Nicaragua, and is supposed by him to serve as food—the ants culling and storing the leaves for the sake of the fungi which are subsequently developed in the debris (*Naturalist in Nicaragua*, p. 80). Were this spawn artifically exposed to conditions similar to those which it naturally encounters in the interior of the hillocks—heat, darkness and moisture—I believe that the pilei

might very probably be raised at will, and if they really are good eating, the experiment would be well worth trying."

—ERWIN F. SMITH.

Desert Vegetation.—Perhaps the most interesting part of Rev. George Henslow's recent book, *The Origin of Plant Structures*, are the two chapters on desert plants. The first of these chapters is devoted to a consideration of the origin of the morphological peculiarities of desert plants; the second to the histological peculiarities of such plants. A large amount of data are brought together, rather hastily it would appear, going to show that the peculiarities of desert plants are the direct outcome of the conditions under which they grow, in other words, that these peculiar modifications, such as reduction of leaf surface, increase of succulency, acquisition of spines, development of water storage tissues, sinking of the stomata below the level of the surface, excessive development of cuticle, of wax, or of hairiness, change from annual to biennial or perennial, increased length of roots, etc., are all brought about by the direct action of environment on the plant. "Natural selection," in the author's own words, "plays no part in the origin of species." These two chapters are well worth the perusal of all who are interested in the study of the flora of our western mountains and arid plains, and the whole book will serve to provoke thought. Other chapters deal with origin of structural peculiarities of alpine and arctic plants; maritime and saline plants; phanerogamous aquatic plants, etc. The book is a companion volume to the author's *Origin of Floral Structures through Insects and other Agencies*.—ERWIN F. SMITH.

A Second Rafinesque.—*Die Pestkrankheiten (Infectionskrankheiten) der Kulturgewächse; Nach streng bakteriologischer Methode untersucht und in völliger Uebereinstimmung mit Robert Kochs Entdeckungen geschildert von Prof. Dr. Ernst Hallier*, is one of the querest books it was ever the lot of the writer to read. It was published at Stuttgart in 1895 by Erwin Nägale, and contains 144 8 vo. pages and 7 fairly well executed plates. Concerning this book it may be said that its author is either an undiscovered great genius or else a very crazy man. About one-third of the book is given up to caustic abuse of Anton de Bary and his students, relative to which it may be said that Dr. de Bary's reputation is safe not only in the hands of his friends but also in the hands of all who love clear thinking and honest work; and all this without defending any of the errors into which he may have fallen. Another third of the book or thereabouts is devoted to the description of old and well known species of Peronosporaceæ,

little that is really new being added, but most facts being correctly stated. The names of many of the species, however, are changed for reasons which would not be recognized as good even by the most ultra radical. For example *Cystopus candidus* is changed to *C. capsellæ* E. H. because the fungus is said to grow mostly on Capsella and every fungus should be named as far as possible from the host it infests. In like manner *Cystopus cubicus* becomes *C. compositarum* E. H.; *Phytophthora infestans*, *P. solani* E. H.; *Peronospora sparsa*, *P. rosæ* E. H., etc. In the same way the author puts his initials after many old genera e. g., Phytophthora and Peronospora, or substitutes other names, e. g. *Zoospora* E. H. for *Plasmopara*, because he concieves the name to have been originally employed in a different sense from that in which it is now used or in which he employs it. The other idea running through the book and occupying at least a third of it is that bacteria originate from plastids developed inside of the cells of fungi, and that we shall never make any progress in the study of animal and plant diseases due to bacteria until we determine from just what fungi they originate. The potato rot, for example, is due to bacteria developed from the broken down mycelium of the fungus *Phytophthora infestans*.

"If now one keeps for a long time in observation under the microscope such an escaped mass of plasma [from the mycelium or conidia of Phytophthora] one beholds, just as in the cases already mentioned by us, the freeing of the plastids, their change into micrococcus, and the elongation, division, etc. of these." (P. 82). In *Peronospora ficariæ* also "the origin of the microörganisms is unquestionable, but until now I have not been able to follow them further. These organisms are visible in a fresh section in the interior of the leaf tissue of the host." (P. 134.) The converse of this proposition is also true i. e. that under certain conditions bacteria change back again into the original fungus, the growth of certain yeast cells into mycelium being cited as a case in in point. "If these [bacteria] arise from definite fungi by the finally free development of the plastids, it must also come to pass that the micrococcus, which is the first product of the freed plastids, will again give rise to the higher fungoüs form from which it originated. Of this the first well known and precise example is the history of the development of the beer yeast." (P. 105.). The author who is a graduate of one of the German Universities, formerly held a chair of botany in one of them, and has been writing books similar to this one for the last 30 years claims to have seen the change from fungous plastids inside of mycelia or spores into genuine free swimming bacteria, rods and cocci. This change is difficult to bring about artifically, requiring long watch-

ing at the microscope and the partial exclusion of air from the preparation. Figures are given of these plastids and of the bacteria. All of which reminds us of the proof of miraculous healing by holy water at certain wells, viz., " the well is with us to this day." The author complains that nobody reads his books, but this cannot be charged against the writer who has patiently waded through the whole of this one, to very little profit, however, it must be confessed. The absurdities, however, are not so numerous as in the author's *Phytopathology*, published in 1868. Therein may be found, full fledged or in embryo, most of the queer notions here set forth and also many others.

ERWIN F. SMITH.

ZOOLOGY.

The Cruise of the Princess Alice.—The zoological material obtained by the Prince of Monaco during the past summer cruise of his yacht, the Princess Alice, is abundant and valuable. The fortunate capture of a sperm whale in the vicinity of the yacht, off the coast of Terceira Island, resulted in the acquisition of some rare specimens of the animal kingdom which otherwise might never have been known. From the Prince's narrative of the voyage we learn that the cachalot was the " catch " of some Portugese whalers with whom the Prince arranged to secure what portions of the animal he wished, especially the brain. Unfortunately some days elapsed before the skull was penetrated, and then the brain was found to be in too advanced a stage of decomposition to be of use for preservation. Meantime a large number a parasites were collected from the stomach, the digestive organs, the blubber and the skin of the animal, and the contents of the stomach secured for examination. While in the act of death the whale ejected several large cephalopods which it had only just swallowed, as was evident from their perfect preservation. These were also obtained by the Prince for his collection. Amongst them were three large specimens, each over one meter in length, of a species probably new, of the little-known genus Histioteuthis; also the bodies of two other immense cephalopods so different from all hitherto known that it is impossible to place them in any genus or even family of this order. M. Jonbain proposes for them the name *Lepidoteuthis grimaldii.* One of these specimens is a female, of which the body, or visceral sac after prolonged immersion in formol and alcohol, still measures 90 cm. in length, from which it is

estimated that the length of the complete animal would exceed 2 meters. The surface of the sac is covered with large, solid rhomboidal scales, like those of a pine cone. The fin is very powerful, forms one-half of the length of the body and is not furnished with scales.

When the stomach of the whale was opened it was found to contain over a hundred kilogrammes of partially digested debris of cephalopods, all of them of enormous size. The crown and tentacles of a Cucioteuthis were identified. This genus has hitherto been known only by few fragments. The muscular arms, though shrunken and contracted by the preserving fluid, are as thick as those of a man, were covered with great suckers, each armed with a sharp claw, as powerful as those of the larger carnivora. More than one hundred of these suckers remain adhering to the arms.

Another cephalopod found in the stomach of the whale is provided with a large fin, in the skin of which are enclosed certain photogenic organs. The form of the body suggests a new species, but as the head is wanting, it cannot be positively identified.

These cephalopods are all powerful swimmers, and very muscular. They appear to belong to the fauna of the deep intermediate waters, an almost unknown region. They never come to the surface, no do they lie on the bottom of the sea. Their great agility prevents their capture in nets, hence it would seem that the only way to obtain these interesting gigantic creatures is to kill the giant who feeds upom them and rescue the fragments from his huge maw.

Accordingly, for the next season's cruise, the Princess Alice is to have, in addition to her present fittings, those of a sperm whaler, or else to have as a compainion a special whaling tender.

The further working up of the material in hand is being pushed forward with energy, and interesting results are anticipated. (Nature, Jan., 1896.)

Australian Spiders.—Among the new Arachnida reported from New South Wales are three species of Nephila; *N. fletcherii*, *N. edwardsii* and *ventricosa*. These are described and figured by Mr. W. J. Rainbow in the Proceeds. of the Linn. Soc. N. South Wales. The author includes in his paper some interesting observations on the habits of Nephilæ and their supposed bird-snaring propensities. Representatives of this genus abound in tropical and subtropical regions. Their webs are composed of two kinds of silk; one yellow, exceedingly viscid and elastic, the other white, dry and somewhat brittle. The latter is used for the framework of the web, the guys and radii, and the former

for the concentric rings. These snares are at varying heights, sometimes within reach, again 10 to 12 feet from the ground, but always in a position exposed to the rays of the sun. The diameter is also variable, from 3 feet upwards. One seen by Gräffe in the Fiji Islands (probably a Nephila) constructs a web 30 feet in diameter.

These snares are strong enough to entrap small birds. In the author's opinion the web is not set for such game, and the spider does not feed on her ornithological victim. In the cases where she has been observed with her fangs in the body of the ensnared bird it is probable that it is for the purpose of hastening the death of the bird in order to prevent its injuring the web in its struggles to escape.

Spiders of the genus Nephila are easily tamed. Although exceedingly voracious, they can nevertheless exist for many days without either food or water. They pair in autumn. The sexes inhabit the same web for a considerable time, the female in the center and the male on the upper edge of the web. His efforts to ingratiate himself in the favor of his mate are not always successful. It not infrequently happens that he has to retire from her presence minus two or three legs. "Ultimately says the author, he succeeds in attaching himself in the requisite position, and performing the necessary act of fecundation." (Proceeds. Linn. Soc. N. South Wales, [2] Vol. X, Pt. 2, 1895.)

Autodax iëcanus.—According to Mr. Van Denburg, *Autodax iëcanus*, a black Salmander first found in Shasta Co., California, is a nocturnal forager. It usually walks slowly along, moving one foot at a time, but is capable of rapid motion when necessary. At such a time it aids the action of the legs by a sinuous motion of the whole body and tail. In addition to being prehensile, the tail is put to a third use. When caught the animal will often remain motionless, but if touched will raise the tail and strike it forcibly against the surface upon which it rests, and accompanying this action with a quick motion of the hind legs, will jump from four to six inches, rising as high as two or three inches. Mr. Van Denburg finds that the species has a wide distribution in California. (Proceeds. Calif. Acad. Sci., Vol. V, 1895.)

Reptiles and Batrachians of Mesilla Valley, New Mexico.—The following list may be worth publishing as a contribution to the more exact knowledge of the distribution of animals in New Mexico. It may be relied upon as correct, as all the species have been identified by Dr. L. Stejneger, and the specimens are to be found in the U. S. National Museum. The valley about Las Cruces, where most of the species were obtained, is 3800 ft. above sea-level, its extreme sides rise

to about 5000 ft. The records marked Lane Coll. are based on specimens obtained by Mr. Lane of Las Cruces, mainly by purchase from the Mexicans.

(1.)* *Bufo lentiginosus* v. *woodhousei.* Common about the town.

(2.) *Rana pipiens* v. *brachycephala.* Common in suitable places.

(3.) *Amblystoma tigrinum.* Not rare about the town. A large speimen found by Mr. J. Schmidt.

(4.) *Cistudo ornatus.* Common about the town.

(5.) *Sistrurus edwardsii.* Close to the College building.

(6.) *Heterodon nasicus.* Close to the College, rather common.

(7.) *Coluber emoryi.* One near Las Cruces, April, 1894 (J. M. Walker).

(8.) *Pituophis sayi.* Our commonest snake. One specimen had the head-scales arranged as in the so-called genus *Churchillia.*

(9.) *Bascanion testaceum.* One specimen.

(10.) *Thamnophis dorsalis.* Frequent, the commonest snake after *Pituophis.*

(11.) *Lampropeltis pyrrhomelas.* H. B. Lane Coll.

(12.) *Lampropeltis splendida.* Lane Coll.

(13.) *Diadophis regalis.* Lane Coll.

(14.) *Arizona elegans.* Lane Coll.

(15.) *Rhinocheilus lecontei.* Lane Coll.

(16.) *Liopeltis vernalis.* Lane Coll.

(17.) *Tantilla nigriceps.* Lane Coll.

(18.) *Leptotyphlops dulcis.* Lane Coll., also one obtained by Prof. C. H. T. Townsend.

(19.) *Eumeces obsoletus.* Not rare near the College.

(20.) *Cnemidophorus tessellatus.* Common about the mesquites bushes.

(21.) *Cnemidophorus perplexus.* Lane Coll.

(22.)* *Sceloporus magister.* One in Coll. Exp. Sta., one in Lane Coll.

(23.)* *Uta stansburiana.* Our commonest lizard, abundant on the college campus.

(24.)* *Crotaphytus wislizenii.* One. Remains of beetles in stomach.

(25.)* *Crotaphytus baileyi.* Apparently not uncommon. One had two young *Phrynosoma modestum* in its stomach.

(26.) *Phrynosoma cornutum.* Common. At Lamy and Santa Fé it is replaced by *P. hernandezii,* which in the neighborhood by Santa Fé ascends to 7475 ft.

(27.) *Phrynosoma modestum.* Common. There also occurs a bluish mutation.

The Death Valley Expedition, much further west, obtained 56 reptiles and batrachians, of which only five (those marked with an asterisk) are common to our list. It is especially noteworthy that there is not a single snake in common.

—T. D. A. Cockerell, N. M. Agr. Exp. Sta.

Professor Cope's criticisms of my drawings of the squamosal region of Conolophus subcristatus Gray; (Amer. Natural., Febr., 1896, p. 148–149) and a few remarks about his drawings of the same object from Steindachner.—In the February Number of this Journal Prof. Cope makes the following remarks: "Dr. Baur again denies that the exoccipital [paroccipital] articulates with the quadrate in certain genera of the Iguanidæ and gives some figures of that region in the *Conolophus subcristatus*, to sustain his allegation. Unfortunately, though he seems to have taken the elements apart, as I suggested that he do, he did not put them together in their original relation when he had them drawn. I now give two drawings traced from the skull of the same species given by Dr. Steindachner. As these plates represent exactly the characters, which I have observed and described in allied genera, I regard them as correct. It will be observed that there is a considerable contact between the exoccipital and the quadrate. There is also contact with the supratemporal [prosqamosal] and probably with the paroccipital [squamosal]. *The articulation of the quadrate with the exoccipital is universal in the Iguanidæ.*"

To this I have to reply the following: 1. The single elements of the skull of Conolophus were not taken apart at all. The quadrate, prosquamosal, and squamosal of the right side were separated from the rest of the skull, in such a way, that they remained together in natural position. The corresponding left side of the skull remained intact. All this was done two years ago, without the advice of Prof. Cope. My figures were drawn with the camera-lucida and are absolutely correct in every respect. I have two other skulls of Conolophus; several of Amblyrhynchus, Iguana and Cyclura. In all I find the same condition. I have not to change a single word in my original description nor a line in my drawings. The quadrate is not supported by the par occipital [exoccipital Cope] in the Lizards, as Cope stated, but by the squamosal [paroccipital Cope], the prosquamosal [supratemporal Cope] taking also part, if present. The paroccipital does not even touch the quadrate.

2. I know the drawings of Steindachner very well. These drawings, however, have not been made, to show the detailed relations of the different elements of the skull. Especially the regions copied by Cope are drawn quite insufficiently. The sutures between the different elements cannot be made out.

Prof. Cope's drawings are not exact tracings from Steindachner for he has drawn sutures, which do not exist at all in Steindachner's figures. There is no such suture, as he figures between the postorbital (Pob.) and his supratemporal (St.), in the actual specimen, nor in Steindachner's drawing. The real suture between these two elements, crosses Cope's alleged suture at right angles, as can be seen in any of the skulls of the Ignanidæ. In Prof. Cope's figure the outer and upper portion of the distal end of the paroccipital process separates the parietal process from the prosquamosal (supratemporal, Cope). This is not the case; the parietal process is always united with the prosquamosal above the distal end of the paroccipital. The squamosal (paroccipital, Cope) is also drawn quite incorrect; besides its true relations can not be made out at all from Steindachner's figures, which are quite useless in this respect.

Prof. Cope really believes now, that the element in the Testudines called by Cuvier "*occipital extérieure*"; by Owen "*paroccipital,*" by Huxley "*opisthotic*" consists of *two* elements, and this he simply does, in order to hold his absolutely unfounded idea of the homology of the squamosal of the Squamata with the paroccipital of the Testudines. The paroccipital is a single element which, in all Reptilia known, lodges in front the posterior semicircular canals. It is free from the exoccipital in the Ichthyosauria, Testudines, and young Rhynchocephalia, it is united with the exoccipital in the whole Archosaurian branch of Reptiles containing the Crocodilia, Phytosauria, Aetosauria, Megalosauria, Iguanodontia, Cetiosauria, Pterosauria; it is also united with the exoccipital in the Plesiosauria and Squamata. It is a fact, that the exoccipital plus paroccipital of the Ichthyosauria, Testudines, and the young Sphenodon are homologue to the exoccipital plus united paroccipital in the Squamata and other Reptilia. In the first case the paroccipital is free from the exoccipital either during the whole life, or during the younger stages, in the second case it becomes very early united with the exoccipital. The paroccipital portion of the exoccipital of the adult Sphenodon is homologous to the paroccipital portion of the exoccipital in the Squamata, and to the free paroccipital in the Testudines. If the paroccipital of the Testudines contains 2 elements, as Prof. Cope sees it, then the paroccipital portion of the Squamata

must also contain 2 elements. How then it is possible that the squamosal of the Squamata, can be one of the two elements? The paroccipital has always been a single element from the oldest Fishes and Batrachia up to the Reptilia.

The squamosal of the Squamata (Ophidia, Lacertilia, including Mosasauridæ) is homologous to the squamosal of *Saphæosaurus* (Sauranodon) of the Rhynchocephalia, and of the Ichthyosauria. In all these groups the squamosal is free from the prosquamosal. I have shown that the so-called squamosal of Sphenodon is the homologue of the squamosal plus prosquamosal of the Jurassic Saphæosaurus and the squamosal plus prosquamosal of the Lacertilia. In all other Reptilia, in Birds and Mammals the squamosal is a composit, which is homologous to the squamosal plus prosquamosal of the Stegocephalia, Ichthyosauria, Lacertilia, and Saphæosaurus of the Rhynchocephalia. (Baur G. Bemerkungen über die Osteologie der Schläfengegend der höheren Wirbelthiere. Anat., Anz. X, 1894, pp. 314–330).

I wish Prof. Cope would find the ancestors of the *Testudines*, it would be very important; but for the establishment of the homology of the paroccipital we do not need them; even then the paroccipital will be found to be a single element and not two.

—G. Baur, University of Chicago.

The Food of Some Colorado Birds.—Looking over some old memoranda, I came across the following records of the stomach-contents of birds, which may possibly be worth preserving.

(1.) *Cyanocitta stelleri macrolopha.* Shot Aug. 27, 1889. Willow Creek, Custer Co., Colo. Food, wheat grains and fragments of an insect, not complete enough for identification.

(2.) *Zenaidura macroura.* Shot Aug. 28, 1889. Willow Creek, Custer Co., Colo. Food, a single rather large seed.

(3.) *Canaca obscura.* Willow Creek, Custer Co., Colo. Food: crop containing many berries of *Arctostaphylos uva ursi*, and some leaves of a different plant; stomach containing seeds; intestine with remains of insects.

(4.) *Falco sparverius.* Shot by Rev. A. Wright, Willow Creek, Custer Co., Colo. Food, stomach full of grasshoppers, apparently *Camnula pellucida* var. *obiona.* The mass was colored red.

(5.) *Falco sparverius.* Shot by Mr. W. P. Lowe at the head of the Big Arroyo, Colo. Food, remains of grasshoppers, and a species of *Anabrus*, identified for me by Prof. L. Bruner as *A. purpurascens.*

(6.) *Tyrannus verticalis* ♂. Shot by Mr. W. P. Lowe, Big Arroyo,

Colo., May 11, 1890. Food, *Euphoria inda*, one specimen, almost whole; *Cicindela* sp., fragments; hymenopterous insects, broken, rather large, thorax black, abdomen red.

(7.) *Pyranga ludoviciana.* ♂. Shot by Mr. P. Lowe, Big Arroyo, Colo., May 14, 1890. Food, fragments of a blow-fly (*Lucilia* or *Calliphora*); fragments of a beetle (perhaps a longicorn); and some green eggs, apparently those of a *Smerinthus*. These eggs were numerous, but I found no fragments of the ♀ moth in which they must have been when eaten.

(8.) *Salpinctus obsoletus.* Shot by Mr. W. P. Lowe, Big Arroyo, Colo., May 14, 1890. Food, fragments of bettles, including a weevil.

(9.) *Geococcyx californianus*, ♂. Shot by Mr. W. P. Lowe, Big Arroyo, Pueblo Co., Colo., Dec. 5, 1889. Food, grasshoppers (*Acrididæ*), *Ophryastes tuberosus* and perhaps another allied species, and a blue-green rugose metallic fragment of an unknown insect.

(10.) *Ampelis garrulus*, ♀. Shot by Mr. W. P. Lowe, Badito, Huerfans Co., Colo. Food, berries of juniper (*Juniperus communis*).

(11.) *Aphelocoma woodhousei*, ♂. Shot by Mr. W. P. Lowe, Badito, Huerfans Co., Colo. Food, fragmentary seeds (papilionaceous?), fragments of bones of a small passerine bird.

(12.) *Merula migratoria.* Cusack Ranch, Custer Co., Colo., April, 1888. Food, seeds and geodephagous beetles.

Mr. Lowe is responsible for the identification of the birds shot by him; he sent me only the stomach-contents.

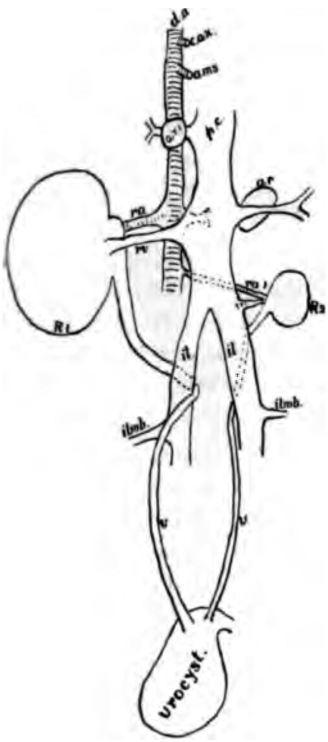
—T. D. A. Cockerell, N. M. Agr. Exp. Sta.

The Manx Cat.—A correspondant of the Zoologist notes an interesting fact concerning a Manx cat in his possession. This tailless cat took of its own accord a mate of the normal type, and from the union resulted a litter of three, which like the mother lacked tails. Friendly relations continued to exist between the parent cats until six successive litters had been produced, each litter in turn showing to a less degree the mother Manx cat's influence upon the form of the progeny, as may be seen in the following table compiled by the owner of the cats.

Litters.	Tailless.	Half tail.	Normal tail.
1	3	0	0
2	2	1	0
3	1	2	0
4	0	2	1
5	0	1	2
6	0	0	3

It would be interesting to carry the experiment further and see if a union of the Manx cat with one of her own race would result in restoring with the same regularity with which she lost it, the power to produce her own type. (Revue Scientif. T. 4, 1895.)

A case of Renal Abnormality in the Cat.—Anomalous condition of the renal organs and accompanying blood vessels was recently disclosed in a dissection in this laboratory. The accompanying diagram explains the phenomenon. The left kidney was a miniature of the right though functional. The dimensions of the right kidney in another subject of equal size as the specimen under discussion were found to be—length 3 cm., width 2 cm. and dorso-ventral thickness 15 mm.; the left os is natural being slightly smaller than this. The dimensions just given may be regarded as normal.



In the subject whose renal anatomy has been here figured, the measurements of the right kidney were as follows: length 4 cm. breadth 2½ cm. and thickness (dorso-ventral) 19 mm., considerably above the normal as one would expect when the extremely small size of the left kidney is considered. The dimensions of the latter were as follows: length 12 mm., breadth 8 mm., and thickness or dorsoventral diameter 5 mm., less than one-third the dimensions of the right kidney.

Upon hardening, staining and sectioning in the usual way the glomeruli and uriniferous tubules were found to be normal though, the presence of a small amount of fat in the kidney was noted. The histological condition of the kidney and the presence of the left ureter, which, though smaller than the right was clearly functional, proved that the left kidney was of value in the vegetative processes of the organism. The right renal artery (*ra*) was, as one would expect larger than the left (*ra*). The postcava (*pc*) in this cat was divided very far forward in the lumbar region to form the common iliac veins, causing the left

renal vein (*rvi*) to empty into the left common iliac. This variation from the normal in postcaval structure is by no means uncommon.

Letters in the figure, not referred to in the text are as follows: *da.* dorsal aorta, *cax.* cœliar axis, *a. m. s.* anterior mesenteric artery, *rv.* vein from right kidney, R_1. right kidney, R_2. left kidney, *ar.* and *ari.* left and right adrenal bodies with accompanying veins. *Il* left and right common iliacs, *ilmb.* ilio-lumbar veins *u* ureters, *urocyst* urinary bladder.—F. L. WASHBURN, Biological Laboratory, University of Oregon.

Zoological News.—Mr. O. F. Cook has published a monograph of Scytonotus. He considers this genus to be the most specialized of the Polydesmid Myriapoda, basing his conclusion on its secondary sexual characters. He recognizes nine species as belonging to the genus. (Ann. New York, Acad. Sci., VIII).

A gigantic Cephalopod, supposed to be a new species of Architeuthis, was driven inshore on the eastern side of the bay of Tokyo. A description of it, illustrated with drawings, is published by K. Mitsukuri and S. Ikeda. It is characterized by shape of its fins and of its beaks, the unequal lengths of the sessile arms, and other minor details. (Zool. Mag., Vol. VII, 1895).

Prof. Gegenbaur has in the Morphologisches Jahrbuch for the year 1895, instituted a study of the clavicle and the elements adjacent to it and the scapular arch. He calls attention to the fact that there are two elements in the position of the former in Dipnoi, Crosopterygia and Chondrostei. He then shows that the element nearest the scapula is retained in some of the Stegocephalia, while the anterior and distal element is increased in length. He calls the former the cleithrum, and retains for the latter the name clavicle. The clavicle only remains in the existing order of Batrachia, and higher groups, while the cleithrum only remains in the higher fishes, beginning with Lepidosteus and Amia.

According to Dr. Delisle the cranial capacity of the Orang-Outang averages 408 cubic centimeters. (L'Anthropologie Tome, VI, 1895.)

Ranke's researches show that the weight of the human brain is much greater in proportion to the weight of the spinal cord than in any other vertebrate. (Correspondenzblatte).

Dr. E. Rosenberg publishes in the Morphologisches Jahrbuch for 1895, an investigation into the reduction of the number of the incisor teeth which is seen in the human species. He shows: first, that the loss of

the external incisor, which was first pointed out by Cope, and which has been observed independently by several others, is frequently observed in Europe as well as in America; second, that the loss of the first inferior incisor is also not very uncommon in Europe and that the final reduction of the inferior incisors, should it take place, will be by the loss of this tooth and not by that of the external incisor as in the superior series. He, therefore, believes that the ultimate formula of the incisive dentition in man will be I$\frac{1}{1}$, and not I$\frac{1}{2}$, as Cope left it.

ENTOMOLOGY.[1]

The Segmental Sclerites of Spirobolus.—The structure of the segments of Diplopoda has long been a morphological puzzle. On account of the possession of two pairs of legs they have in a general way been supposed to be double segments, that is, formed by the coalescence of two distinct embryonic or theoretical segments. Toward a morphological demonstration of this idea there has been little progress. Indeed, there are many facts which give grounds of suspicion as to its correctness. Among these may be noticed that the double-footed state does not occur in the embryo at all, and that the segments which in the adult bear two pairs of legs either do not exist in the newly hatched larva or do not bear any legs at that stage, the newly hatched diplopod larva having but three pairs of legs, the posterior of which is attached to the fourth segment (at least in the Polydesmoidea). Moreover, all Diplopoda have apodous segments not differing otherwise from those which bear legs; also all Diplopoda have segments which bear but one pair of legs, and yet have not been found to be greatly different from the others. Growing Diplopoda acquire segments by intercalation in front of the last. The segment is added at one moult, the legs for it at the next. As the possession of two pairs of legs has been the occasion of the theories of duplex segments, these facts are the more relevant as objections, since more difficulties are introduced than are disposed of by the theories.

The existence of pluræ in the Oniscomorpha has long been known, and for a less period in the Colobognatha and Limacomorpha. In the other orders these elements of the segmental ring are so thoroughly coalesced or eliminated that their existence was theoretical until their

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

discovory in Stemmatoiulus. In that form, however, the multiplicity of peculiar characters weakens the application of homologies to the other orders, unless these can be based on structural facts. It is thus a matter of interest that the existence of pleuræ in another Diplopod order can be affirmed.

Some weeks since in examining large African Spirobolidæ I noticed what seemed to be traces of pleural sutures. On mentioning this fact to Mr. F. C. Straub who was studying with me, he called my attention to a specimen of *Spirobolus marginatus* Say which proved to be very remarkable. Possibly it was collected just after moulting, before the sclerites had become coalesced, or it may have been merely an individual anomaly. At any rate, it had on each side an obliquely longitudinal white line across each segment above the pedigerous lamina, indicating a pleural element about as broad as the lamina. That this is the pleural suture seems very probable, on theoretical grounds and more so that on the surface a special striæ followed the line of white.

What is more remarkable, this line was met above by two others which were transverse, dividing the segment into three subequal parts. These two lines extended completely over the animal, the space between them being somewhat greater above. There is also a median longitudinal suture, and a lateral just below the pore, thus dividing the dorsal portion of the ring into twelve subequal parts. The posterior of the transverse sutures follows the depression found in the segments of Spiroboli and usually called "the suture" in descriptions. The anterior line and the median line are indicated by minute differences in the sculpture, which would not have been noticed had not the white line drawn attention to them. It should be added that the lighter color was not due to anything inside or outside the segmental wall, but was in the wall itself and clearly indicated some structural difference. The phenomenon was exhibited by the anterior and middle segments of the body, becoming indistinct caudad. In all cases the pattern was the same; the whole series of lines could be made out on many segments, and there were no other similar lines or discolorations. The lines were not straight if examined under a microscope, even the median showing minute irregularities. Median sutures are known in four or five of the Diplopod orders and hence may reasonably be expected in all.

Had only the median line been marked as related, there would have been no hesitation in supposing that a median suture was indicated. Theoretical considerations only stand in the way of the reasonable presumption that the other exactly similar lines indicate sutures. If such an interpretation is allowed we are brought to the position that

the segmental ring of Spirobolus consists of sixteen sclerites; twelve dorsal, two pleural and two ventral or pedigerous laminæ. It will be seen that only the last tend to indicate a transverse division of the segment, and in no Diplopod as yet has there been shown a transverse suture carried around the segment and dividing it into two parts. Only the legs and the parts necessarily connected with them, such as the pedigerous laminæ and nerve ganglia are duplicated. Even in the Oniscomorpha, Limacomorpha and Colobognatha where the pleuræ are most distinct, there is not the slightest indication that they were ever divided, and as they are the elements to which the pedigerous laminæ are next related, their evidence is more important than any drawn from the dorsal parts of the segment.

There is another way, however, in which a diplopodous animal might be developed from a monopodous ancestor. Alternate segments may have been suppressed, while the corresponding legs have been preserved. For such a supposition we have the analogy of the Chilopoda, where the pedigerous segments alternate with more or less rudimentary segments. In this case, however, the legs have been lost, that is, we must suppose so if we claim the analogy. Such a theory, while no more fantastic than the other, is probably no nearer the truth. After theoretical explanations have been exhausted we may, perhaps, learn that the double-footed condition is a peculiarity of this group of animals, not explainable by any general morphological considerations, but *sui generis*, after the manner of the branched segmental appendages of the Crustacea.—O. F. COOK.

Secretion of Potassium Hydroxide.—Mr. O. H. Lalter has some further notes[2] on the secretion of potassium hydroxide by *Dicranura vinula* and similar phenomena in other Lepidoptera. He finds that the imagines of eight species secrete from the mouth an alkaline fluid on emerging from the pupa. The three species of *Dicranura* wear what is called a shield, derived from the pupa case as they emerge, and they subsequently remove it by their legs. He finds that the strength of the solution in *D. vinula* is about 1.4 grm. of potassium hydroxide in every 100 ccm. of liquid. The mesenteron of the same species develops an anterior dorsal diverticulum for storage of the alkali during pupal life.—*Journal Royal Mic. Society.*

Lake Superior Coleoptera.—Mr. H. F. Wickham publishes[3] an admirable list of Coleoptera from the southern shore of Lake Supe-

[2] Trans Ent. Soc. Lond.. 1895, 399-312.

[3] Proc. Davenport Acad. Nat. Science, VII, 125–169.

rior. More than 200 species are enumerated in this list which have not before been credited to the region of the Lake. The collections were made at Bayfield, Wisconsin, during June and July.

The following introductory remarks are of sufficient general interest to be quoted at some length. The time for an accurate map of the faunal regions of the continent has not yet come—nor will it before another century at least of careful investigation has enabled us to fix approximately the range of the rarer forms of insect life. It is evident to any one who will read with care and with some understanding of the general principles of distribution, that many of the recent theories as to the division of our country into "life-zones" have very little foundation in fact. If better proof were wanting of this, we might point to that of authors changing from year to year their arbitrary arrangement of our zoö-geographical regions—uniting to-day two or three of those of older authors, and separating them again a few months later on. All this may or may not be progress, but it will all have to be gone over again in the light of a wider knowledge than seems to be at present in the possession of certain writers who cannot rest without having first shown us that all previously conceived ideas are totally wrong, and that their explanation of the distribution of life is the only plausible one. A single group of animals may or may not indicate in a general way the lines of distribution followed by a larger number—but it is manifestly unreasonable to hope for a stable method of division of a country into life-zones before the life of that country is well-known.

EMBRYOLOGY.[1]

The Effect of Lithiumchloride upon the Development of the Frog and Toad egg (R. fusca and Bufo vulgaris.)[2]—The results of the series of experiments performed in the histological laboratory at Münich with this salt seem of no little interest, and especially is this the case with the result obtained with a 0·5 per cent solution. In every instance the eggs were placed in the solutions (varying from 1 per cent to 0·2 per cent) between a half and an hour and a half after fertilization.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] A. Gurwitsch, cand. med. Anat. Anz., XI, 65–70.

The blastula obtained with the 5% solution the author attempts, with some degree of plausibility, to make out to be of far reaching morphological importance. Whereas in all other cases development was either more or less hindered or was abnormal, in this case it was entirely symmetrical. The first indication of gastrulation appeared as a ring sinking about the equatorial plane and embracing the entire circumference. Sections showed a large mass of what the author calls passive yolk cells or endoderm forming the lower half, while the upper half, composed of a layer of ectoderm and one of active endoderm, forms a sort of cap covering it.

At a later stage this cap almost includes the lower passive entoderm, and, as the author points out, forms a gastrula that, if the passive yolk be removed, very closely resembles the gastrula of *Amphioxus*. From this it may seem more or less probable that the primitive amphibian gastrula may have been radially symmetrical, and that bilateral symmetry appeared later. Further it appears that the upper or large invagination of the amphibian egg is not the blastopore, but this is represented by the entire circle including the yolk plug.

It may be noted also, that if instead of supposing the passive entoderm to be removed, it be supposed to be greatly increased, one then has a gastrula of the meroblastic type.

Another point of interest is the manner in which the cells of the so-called "active endoderm," or those bordering the equatorial ring, proliferate. This proliferation according to the author has already begun when the invagination of the outer surface commences ; so that instead of there being a *pushing in* of the outer surface, as the process is usually described, there seems to be a *pulling in*. Whether this process is due to the "cytotropism" described by Roux for the cells of the dividing frog egg, or to the taking up of the space occupied by the absorbed contents of the blastula cavity, as described by Hatschek for *Amphioxus*, is not clear.

The embryos obtained differ from those obtained by O. Hertwig with NaCl, in that the brain capsule does not close up and the dying away of the brain matter does not take place, and again instead of the animal cells breaking down as in NaCl, it is the yolk cells that crumble away.

Finally one abnormal lithiumchloride embryo has an adverse significance for the concrescence theory.

It is to be hoped that the author intends later to publish a more extensive paper, which shall be more fully illustrated.—F. C. K.

ANTHROPOLOGY.[1]

An Inquiry into the Origin of Games.—An examination of the games of the Far East (Korea, China and Japan) and a comparison of them with certain games of the North American Indians as explained by Mr. F. H. Cushing, has induced Mr. Culin (Korean Games, with Notes on the Corresponding Games of China and Japan, by Stewart Culin, Director of the Museum of Archeology and Paleontology of the University of Pennsylvania, Philadelphia, 1895) to believe that the true game in the American and Asiatic region referred to, is a traceable descendant of primitive religious divinatory formulæ, reaching back to a time in the process of human development, when man freshly inspired by the phenomena of earth an sky, symbolized in his ceremonies the directions of the four winds, and foretold fate or fortune with arrows.

Because American Indians divine by arrows, because archery, and sets of arrows corresponding in number to Asiatic cosmic divisions, arrow derived grave posts, and guild tallies notched and named like arrows, still survive in Korea, and because arrow like rods are still used there in divinatory formulæ by fortune tellers, Mr. Culin has been led to regard arrow divination as a primitive and original form of fortune telling, and while the totemic arrow marks on short round gambling

Fig. 1. Haida Indian gambling stick suggesting derivation from the arrow. One of a set of 32 bearing devices of the totemic animals of the worlds' quarters supposed to have been derived (traceably perhaps through an intermediate set marked with colored ribbons) from arrow shaftments such as were used by the McCloud River Indians.

sticks of northwest coast Indans are urged as indications of the arrow ancestry of the latter, the same interesting suggestion is made as to the cylindrical earthen stamps from Ecquador and the round and flat engraved cylinders from Babylonia. Twenty-three out of the ninety-seven Korean games described (though in many cases the clue is not

[1] This department is edited by Henry C. Mercer, University of Penna, Phila.

stated) and particularly games played on diagrams like the Korean *Nyout*, (*Pachesi*) or chess, are held to suggest a primitive divining board

Fig. 2. Cylindrical earthenware stamp from Ecuador suggesting derivation from the arrow. It bears a highly conventionalized device representing a bird. It's striking resemblance to the Haida gambling sticks suggests its own derivation from the carved shaftments of arrows and furnishes also a clue to the probable origin of the Babylonian seal cylinders.

—the world, with the quarters of the four winds "the heavens above and the earth beneath," where the relations of arrows thrown, scattered red or distributed, symbolized the early callings of man upon fate, the first soothsayer's translation of unseen causes into the events of life.

The investigation deals with evanescent and elusive conditions and of necessity the family tree of games is often vague and disjointed. An etymology, arrow notches on a card, scorings on sticks, the pas-

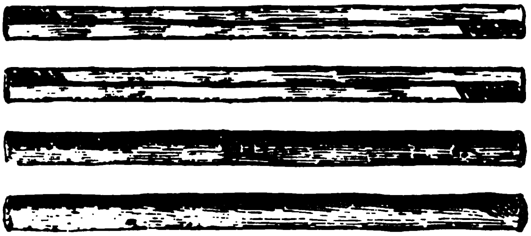


Fig. 3. Count Staves of wood used by Kiowa Indians suggesting derivation from arrows, emplosed in the game of *Zohn ahl*, they are inscribed with marks resembling arrow decoration and shaftment.

times of Indians in America and of far Orientals in Asia. depending often upon the impartial testimony of the investigator have seemed to lead hnmanity backward in the cases and countries cited, not to the flight of birds, the observed instincts of animals, or the virtues of plants or minerals, but to the arrow as the ancestral symbol of the human necromancer. The Korean game of *Nyout*, where the throwing of marked sticks scores on a dotted diagram, seems related to divi-

nation because its arrangement of dots looks like magical diagrams in an ancient Chinese book of divination while there are throws, and

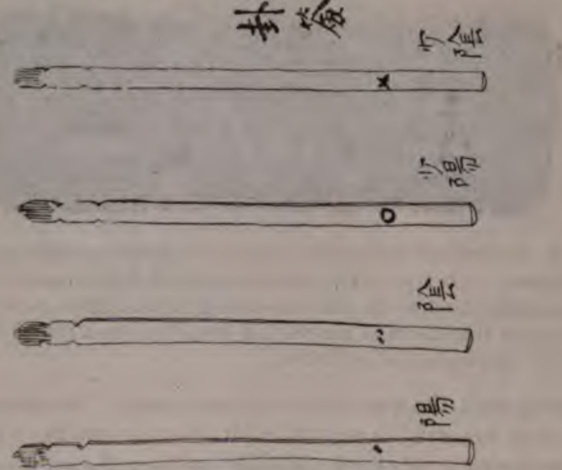


Fig. 4. *Kwa Zsin* Chinese (wooden) divining splints. Four of a set of sixth-four, suggesting by their notched points and name (*tsim* resembling *tsin*, arrow) a derivation from arrows.

arrangements, and suites, and figures in the game that seem to connect it with chess, and with dice and backgammon and other Korean dice and board games, thus, we are told, putting the latter familiar and Europeanized class of games into the line of succession from the primitive formulæ of the arrow diviner. Long narrow Korean playing cards, resembling a set of Chinese lottery arrows similarly marked,

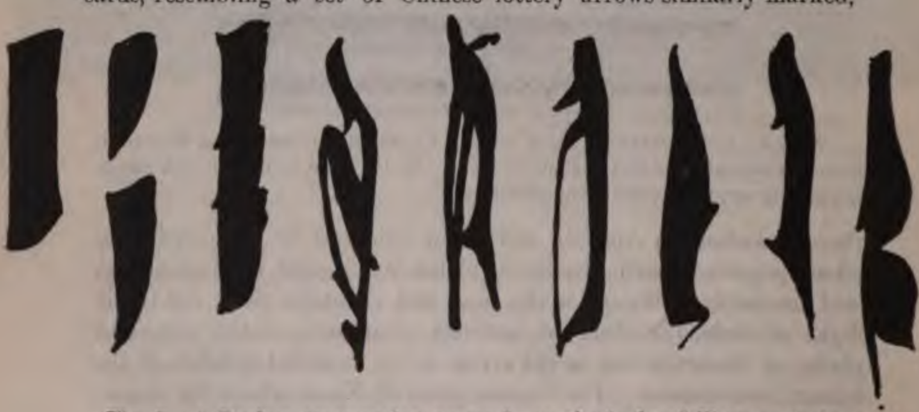


Fig. 5. $\frac{2}{3}$ Devices representing numerals on the backs of Korean playing Cards, (*htou-tjyen* fighting tablets). The devices suggests in their shape a derivation from the cut cock feathers on arrows.

and with arrow feathers painted on their backs refer us strikingly to the arrow, and this fact, illustrated by aseries of surprising pictures is one of the telling features of Mr. Culin's book. Whether we agree or not, whether we prefer to wait till more evidence is in for regions like parts of Australia, Tasmania and the Andaman Islands where man appears never to have had arrows, and whether we believe that we have reason to doubt that the notion of the four world quarters ever was universally impressed upon humanity the original suggestions of Mr. Culin pointing out new and seemingly widespread relations among games and tracing or seeking to trace in them fresh illustration for the story of human development, is of importance and interest.

Following further the author's dignified and always sympathetic presentation of the subject into a description of other games which sometimes, like the counting out rhymes of children, are regarded as less conscious survivals of the diviners' doings, sometimes as mere festive or athletic pastimes, we gather pleasing evidence of the world kinship of children in the record (often illustrated by native Korean artists in color), of blind man's buff, leap frog, horse stick, tug of war, stone fighting, pop guns, tops, tilt ups, and jack stones. Too briefly the pages reflecting remembered joys of youth tell of the loosened waters of a brook breaking, if they can, a juvenile dam, of hostile kites sawing their abraisive strings as they soar, of violet whipping, of shovel playing, of youthful mouths crammed with cherries, to be eaten without swallowing the stones, and of dragon flies, caught in spider webbed hoops by children reciting poems and released with unconscious cruelty when impaled with paper banners. But new aspects of an ever present floral sympathy in the land of cherry blossoms and the chrysanthemum are revealed to us when we learn of such Japanese names for bands of combatants as "spring willow blossom," "summer rest forest," and "autumn garden," shouted across the green turf in the foot ball game.

Notwithstanding the similarities urged between some of the arrow games of North America and their Asiatic representatives we look in vain in the book for suggestion of contact of races, or proof of migration. Lines of investigation such as other observers might choose in tracing the rolling of stone discs scored in motion with sticks or arrows (*Chungke*) from the Sandwich Islands to Georgia, the author eschews as unfruitful and inconclusive "unless supported by linguistic evidence." His valuable and original investigation has not essayed to furnish new light as to the geographical origin of the human race but has rather multiplied the evidence showing that man's mind has worked alike everywhere.—H. C. MERCER.

PSYCHOLOGY.

Prof. Mark Baldwin on Preformation and Epigenesis.—In the last number of the Naturalist was republished from *Science*, Prof. Baldwin's observations on my presentation of the contrasted hypotheses of the development of mind.[1] One of these theories was supposed to be in accordance with the evolutionary doctrine of preformation, the other was thought to bear the same relation to that of epigenesis. Prof. Baldwin asks why the three theses arranged under epigenesis may not with equal or greater propriety be arranged in the preformation column. He believes that consciousness has had an influence in directing the course of evolution in accordance with the "general law now recognized by Psychologists under the name of Dynamogenesis—*i. e.*, that the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement." He also says, "I do not suppose that any naturalist would hold to an injection of energy in any form into the natural processes by consciousness. The psychologists are, as Mr. Cattell remarks, about done with a view like that." Prof. Baldwin also remarks that "Prof. Cope can say whether such a construction is true in his case." He adds that "it is only the physical basis of memory in the brain that has a causal relation to the other organic processes of the animal."

To reply to the last question first. The facts seem to show that conscious states do have "a causal relation to the other organic processes of the animal." I have gone into this subject briefly, but more fully than can be done here, in Chap. X of my book on the "Primary Factors of Organic Evolution" (1896). The evolution of the brain, the organ of consciousness, would indicate this, as well as the evidence for Kinetogenesis or evolution by motion. This would follow, if the doctrine of Dynamogenesis referred to by Prof. Baldwin be true, at the psychic end of the process, and if acquired characters be inherited, as required by the doctrine of epigenesis If then consciousness has such a function, the question arises as to its immediate mode of action. Prof. Baldwin says "only the physical basis of memory has a causal relation," etc. This proposition I can accept, and it is true whether that physical basis be due to a conscious state called a sense-impression, or not. But the directions of the acts (motions) which flow from that physical basis are very various in organic beings, having adaptations

[1] See Primary Factors of Organic Evolution, 1896, p. 14.

to as many ends as there are benefits to be obtained. It is evident that the physical basis of memory undergoes a change from the condition in which it is first produced. Its component parts are evidently rearranged in accordance with some purely psychic factors, *i. e.*, in accordance with qualities and properties which are only appreciable by conscious states. One may suppose that a reflection of the physical basis of a memory may be transmitted to different parts of the cortex, and that in one part it is located in accordance with one criterion of classification, and in another region in accordance with another criterion. In other words, the representative functions of the brain control the structure of the physical basis of memory, or cause a modified reproduction of it. These representative functions may be of the simplest —*i. e.*, they may consist only of criteria of size, color, utility, etc., or they may be more complex, involving judgments, concepts, etc. Finally, no criteria can violate the ultimate "forms of thought," which are essentials of all representative mental action. These, in short, are the fundamental reasons why mental conditions may be believed to direct the course of energy, without increasing the amount of that energy.

The relation of this factor of evolution to the the theories of Preformation and Epigenesis may be now considered. The reason why I believe that the process of mental evolution has been and is at bottom epigenetic, is because there is no way short of supernatural revelation by which mental education can be accomplished other than by contact with the environment through sense-impressions, and by transmission of the results to subsequent generations. The opinion is simply a consistent application to brain tissue of a doctrine supposed to be true of the other organic structures. The injection of consciousness into the process does not alter the case, but adds a factor which necessitates the progressive character of evolution.

I do not perceive how promiscuous variation and natural selection alone can result in progressive psychic evolution, more than in structural evolution, since the former is conditioned by the latter. The objections to this mode of accounting for progressive structural evolution are well known, and are enumerated in my book on page 474. It is true, no doubt, that as we rise in the scale of mental faculty the capacity for acquisition increases. How far these acquisitions are in inheritable is a question of detail, but no one denies, so far as I am aware, excepting consistent preformationists, that they are more or less inheritable. It is to be supposed that the longer special aptitudes are cultivated the more likely they are to be inherited, precisely as the ef-

fects of constant use of an organism are inherited, while sports and mutilations are not inherited. The importance of the social influences among men on which Prof. Baldwin justly lays so much stress, consists in the fact that they are continuous in their operation, and produce permanent habits. This accounts for the phenomena referred to by him when he remarks that "the level of culture in a community seems to be about as fixed a thing as moral qualities are capable of being; much more so than the level of individual endowment. This latter seems to be capricious or variable, while the former moves by a regular movement and with a massive front." Here we have portrayed exactly what occurs in structural evolution. The habitual influence of the environment, internal and external, conditions the steady advance, while sports produce only temporary effects or are effective only in proportion to their ratio to the entire movement.

In an essay published in Science of March 20th, 1896, Prof. Baldwin comments on the lectures of Prof. Lloyd Morgan, in support of his own doctrine of Social Heredity. This is the name he has applied to this transmission of habits through their persistence in societies, so that the young acquire them through imitation or instruction, without the intervention of physical heredity. As a foundation for this view he disputes the necessity of any inheritance of acquired habits by the inheritance of the nervous mechanism which they express, and denies therefore that use is a necessary agent in the evolution of such habits. In order to prove that instincts are not "lapsed intelligence" he says; "The intelligence can never by any possibility create a new movement or effect a new combination of movements, if the apparatus of brain, nerve and muscles has not been made ready for the combination which is effected. This point is no longer in dispute," etc. Immediately before this, however, he says. "But let us ask how the intelligence brings about coördinations of muscular movement. The physchologist is obliged to reply; "Only by a process of selection (through pleasure, pain, experience, association, etc.,) from certain alternative complex movements, which are already possible for the limb or member used."

It is granted in the last quotation that pleasure, pain and other conscious states, select the motions which become habits. Such selection is intelligent, and such act is an expression of intelligence, though of the simplest sort. All that Prof. Baldwin alleges is that intelligence is impotent to construct the mechanism of new habits out of mechanisms already too far specialized in definite directions to permit such a reorganization of structure. This truth in nowise contradicts

the construction of the mechanism of new habits from tissues capable of reconstruction or of modification, a quality which resides very probably in brain tissue, or at least certainly has resided in it at various stages of organic evolution, when new "selections through pleasure, pain, experience, association, etc.," were made; otherwise the selection would have been impossible. This is the history of all the other tissues, and why not of brain tissue? Though Prof. Baldwin denies the necessity of the Lamarckian Factor, he admits it in this doctrine of selection; and his denial of inheritance, only covers the case of physchological sports, as above pointed out. Hence he both admits and denies both Lamarckian and Weismannism.

Weismannism has recently struck the physchological camp, and in Prof. Baldwin and in Mr. Benjamin Kidd, we see some of its recent effects. But since the biologists have generally repudiated Weismannism, the evolutionary physchologists must try and get along without it. Nevertheless, as above remarked, Prof. Baldwin's "Social Heredity" is a real factor, especially in human evolution; but as it is not heredity, I think it should have a new name, which shall be less confusing.

E. D. Cope.

Psychologic Data Wanted.—For purpose of extended comparison I wish data as to habit, instinct or intelligence in animals, above all, minor and trifling ones not in the books, *useless* or *detrimental* ones, and the particular *breed*, *species* or *genus* showing each. Purring, licking, washing face, kneading objects with fore-paws, humping back, and "worrying" captured prey (like the cat), baying (at moon or otherwise); urination and defecation habits (eating, covering up, etc.); disposition of feces and shells in nest; rolling on carrion; cackling (or other disturbance) after laying; eating "afterbirth" or young; sexual habits; transporting eggs or young; nest-sharing; hunting partnerships or similar intelligent associations; hereditary transmission of peculiarities; rearing young of other specics with resulting modification of instinct; feigning death; suicide; "fascination;" are examples. Circular of information will be sent and full credit given for data used, or sender's name will be confidential, as preferred.

Answer as fully as possible, always stating age, sex, place, date (or season), species, breed, and whether personally observed.

R. R. Gurley, M. D.

Clark University, Worcester, Mass.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Nova Scotian Institute of Science.—March 9th.—The following paper was read: "Some Illustrations of Dynamical Geology in Southwestern Nova Scotia," by L. W. Bailey, Esq., M. A., Ph. D.

HARRY PIERS, *Secretary.*

Boston Society of Natural History.—February 19th—The following papers were read: Mr. Outram Bangs: "The Terrapin an Inhabitant of Massachusetts." Dr. Joseph Lincoln Goodale: "The Vocal Sounds of Animals and the Mechanism of their Production."

March 4th.—The following paper was read: Prof. F. W. Putnam: "Symbolism in Ancient America.".—SAMUEL HENSHAW, *Secretary.*

New York Academy of Sciences—Biological Section.—February 7th, 1896.—Dr. J. G. Curtis in the Chair.

A communication from the Council was received asking that the Section take action on Rep. Hurley's bill "To fix the standard of Weights and Measures by the adoption of the metric system of weights and measures."

On motion of Dr. Dean, the Section approved the bill and the Secretary was directed to express the entire commendation of it to the Council.

Dr. Arnold Graf read a paper on "The Structure of the Nephridia in Clepsine." He finds, in the cells of the intra-cellular duct, fine cytoplasmic anastamosing threads which form a contractile mechanism. These are stimulated by granules which are most numerous near the lumen of the cell, and thus a peristalsis is set up which moves the urine out of the duct. In the upper part of the intra-cellular duct, the two or three cells next to the vesicle or funnel have no distinct lumen, but are vacuolated; the vacuoles of the first cell being small, those of the second larger, and so on, till the vacuoles become permanent as a lumen. He explains the action of the first cell as being similar to the ingestion of particles by the infusorians. The matter taken up thus from the funnel by the first cell is carried by the rest, and so on till the cells having a lumen are reached. The presence of the excretum causes the granules to stimulate the muscular fibres of the cells; penstalis results and the substance is carried outwards. The character of this contractile reticulum offers an explanation of the structure of a cilium as being the continuation of a contractile reticular thread.

N. R. Harrington, in "Observations on the Lime Gland of the Earthworm," described the minute structure of these glands in *L. terrestris*, and showed that the lime is taken up from the blood by wandering connective tissue cells which form club-shaped projections on the lamellae of the gland, and which pass off when filled with lime. The new cell comes up from the base of the older cell and repeats the process. This explanation is in harmony with the fact that in all other invertebrates lime is laid down by connective tissue cells. Histological structure and the developmental history confirm it.

Dr. Bashford Dean offered some observations on "Instinct in some of the Lower Vertebrates." The young of *Amia calva*, the dogfish of the Western States, attach themselves, when newly hatched, to the water plants at the bottom of the nest which the male Amia has built. They remain thus attached until the yolk sac is absorbed. As soon as they are fitted to get food they flock together in a dense cluster, following the male. When hatched in an aquarium they go through the same processes. The young fry take food particles only when the particles are in motion, never when they are still. The larvæ of Necturus also take food particles that are in motion.—C. L. BRISTOL, *Secretary*.

American Philosophical Society.—January 17th.—Prof. Hilprecht presented a paper on "Old Babylonian Inscriptions, Chiefly from Nippur," Pt. ii.

February 21st.—Prof, A. W. Goodspeed read a paper on the Röntgen method, with demonstration. Remarks were made by Prof. Houston, J. F. Sachse, Prof. Robb of Trinity College, and Prof. Trowbridge of Cambridge.

March 6th.—The following paper was presented: "Eucalypti in Algeria and Tunisia from an Hygienic and Climatological Point of View," by Dr. Edward Pepper.

Academy of Natural Sciences of Philadelphia—Anthropological Section.—February 14th.—The following papers were read: Dr. Allen on "Prenasal Fossæ of the Skull;" Dr. Brinton on "Human Hybridism;" Dr. McClellan, Skulls and Photographs exhibited.

CHAS. MORRIS, *Recorder*.

The Academy of Science of St. Louis.—February 17, 1896.—Dr. Adolf Alt spoke of the anatomy of the eye, and, by aid of the projecting microscope exhibited a series of axial sections representing the general structure of the eye in thirty-one species of animals, comprising two crustaceans, the squid, three fish, two batrachians, two reptiles, ten birds, and eleven mammals.

Professor F. E. Nipher gave an account of the Geissler and Crookes tubes and the radiant phenomena exhibited by each when used in connection with a high-tension electrical current of rapid alternation, and detailed the recent discoveries of Professor Röntgen, showing that certain of the rays so generated are capable of affecting the sensitized photographic plate through objects opaque to luminous rays. Attention was also called to the experiments of Herz and Lodge with discharges of very high tension alternating currents, which showed that by the latter certain invisible rays are produced, which, like the Röntgen rays, are capable of passing through opaque bodies, such as pitch, but differing in their refrangibility by such media.

March 2d.—Mr. F. W. Duenckel presented a comparison of the records of the United States Meteorological Observatory, located on the Government building in the city, with the record for the Forest Park station, showing that the daily minimum averaged decidedly lower at the Forest Park station than in the city, while the wind averaged decidedly higher for the city station.

Professor E. E. Engler spoke on the summation of certain series of numbers.—William Trelease, *Recording Secretary.*

SCIENTIFIC NEWS.

The *Journal of Comparative Neurology*, which is now entering upon its sixth volume, has its editorial facilities considerably enlarged by the addition to the staff of Dr. Oliver S. Strong, of Columbia College. Professor C. L. Herrick continues as Editor in-Chief. The Managing Editor for 1896, is C. Judson Herrick, to whom business communications should be addressed at Denison University, Granville, O. Editorial communications may be sent to either of the three editors.

THE

AMERICAN NATURALIST

VOL. XXX. May, 1896. 353

THE PROBABLE INFLUENCE OF DISTURBED NUTRITION ON THE EVOLUTION OF THE VEGETATIVE PHASE OF THE SPOROPHYTE.

BY GEO. F. ATKINSON.[1]

In this paper the discussion of the influence of nutrition, applies chiefly to that source of nutrition in plant organs provided with chlorophyll, and presupposes, in general, that the ordinary physiological processes, other than the one which is termed carbon assimilation, are normal. In all such plants some development of this vegetative part of the plant must take place before spore production, or fruiting, of a kind which represents a real increase at the time, can be accomplished. Some apparent, but not real, exceptions to this might be noted. In germination of the spores of Oedogonium, frequently spore production takes place without the development of any such vegetative part of the plant, but there is no real increase of the plant substance. This kind of spore production is only a means, perhaps, to tide over some condition unfavorable for the elaboration of the vegetative phase of the plant, which is present at the time and place. In Coleochæte, germination of the oospore results in the formation of a cellular mass, which

[1] Cornell University.

is larger than the oospore, breaks the enclosing wall, and the cells escape as a number of zoospores in place of one. But in this oospore are the stored products of carbon assimilation of the parent chlorophyll phase of the plant, and this case only differs from that of the Bryophyta, in that the sporophyte becomes separated, with stored products, from the gametophyte, before the differentiation of the spores.

In the higher plants many cases of bulbs, corms, tubers, etc., might be cited to show that the development of the sporophylls, and even fruit, might take place without the accompaniment of chylorophyll bearing organs. But here also the bulbs, corms, etc., represent, in the stored products of carbon assimilation, the preceeding green leaves. In certain ferns, as Osmunda cinnamomea, the sporophyll, which is completely differentiated from the vegetative leaf, appears first in the spring, and could mature its spores without the aid of the vegetative leaves of that season, but the green leaves of the previous season formed the necessary carbohydrates, which are stored in the rhizome and rudimentary leaves during the winter and in fact the sporophylls and sporangia are partly developed at the close of the previous season.

We might say, then, that in general, all spore production in plants, which themselves assimilate carbon dioxide, is necessarily preceeded by a greater or lesser development of chlorophyll bearing organs. This may appear to be a too well known axiom for even the brief discussion here given, but it is necessary in view of what is to come to have this axiom well in mind. Chlorophyll bearing organs, or tissues, then, as compared with sporogenous organs or tissues, are, in point of time within the life cyle, *primary*, while the latter are *secondary*. This proposition should not be regarded as opposed to the primary evolution of the sporophylls as compared with the foliar organs of the sporophyte. It applies only to a comparatively limited extent of time; to the usual cycle between the vegetative and fruiting phases; to the ontogenetic, not to the phylogenetic, development. It applies with equal force to plants in which either the gametophyte or the sporophyte forms the chlorophyll bearing organ.

There is a strong tendency in nature to an economy in the distribution of the food supply between foliar, and sporophyllary, organs; between the vegetative and fruit products of the plant. This is well seen in the varying sizes of plants having varying amounts of food supply, where with limited food supply small plants have few and small leaves accompanied by a limited out-put of fruit (other conditions are considered normal); while with increasing amounts of food, other things being equal, each of these plant products is increased, though not in the same ratio. With high feeding the vegetative increase shows a higher ratio than the fruiting. The food may be so abnormally abundant as to cause an abnormally abundant vegetative growth, accompanied in some cases with rudimentary fruit, or in others with the entire suppression of the fruit. In some rare cases it may be accompanied by the transformation of the sporophyllary organs to vegetative ones.

These facts teach that the fruit product, or sporophyllary development of plants is very sensitive to food supply, requiring a certain amount of food for perfection in even small quantities, increasing with additional food supply up to a given point, when it decreases again to zero; or in rare cases the sporophyllary organ may be transformed to a vegetative one, so antagonistic has the ratio between the vegetative and sporophyllary organs become because of the abnormally favorable conditions for vegetative growth. In addition to the sensitiveness which the fruit organs exhibit to varying amounts of food derived from the soil, they are very sensitive to disturbances in the supply of carbohydrates as a result of carbon assimilation in the vegetative organs, especially of a kind which partly or completely cuts off that supply. This is well seen in the diminished crops as a result of injury to the leaves at critical periods from insects or fungi, or as result of unfavorable meteorological conditions.

When the nutritive supply of the carbohydrates is suddenly disturbed by certain kinds and amounts of injury to the foliage leaves, or by pruning severely, thus cutting off a large number of forming or developed leaves, certain parts of the plant, either simple or in a rudimentary condition of development,

have the function of carbon assimilation forced upon them to save the plant from destruction and to provide for the development of the fruiting organs. As is well known, latent buds, which have been in a dormant condition, may be, for years, are frequently in such cases stimulated to development and form leafy shoots. It is a very common occurrence as a result of severe pruning or of injury to the ordinary leaves for young flower axes, or buds, to develop leafy shoots with other flower buds in the new leaf axils. There is a tendency here to force the vegetative function upon the young flower axes, and if this influence is felt before the specialized character of the floral organs has been developed from the cells at the apex of the axis, the development of these organs will be deferred, while these cells assume the form and function of vegetative organs. This is a matter, perhaps, of common observation as a result of severe pruning, and in some plants can be very easily demonstrated by trial. But it serves well to show the influence of disturbed nutrition on other or dormant parts of the plant when the function of the existing vegetative leaves is arrested. That function is forced from the ordinary and well developed organs upon undeveloped or rudimentary ones, which readily under this influence adapt themselves to continue this important office. This must not be regarded as an attempt to explain the development of adventitious or supernumerary buds, etc., or latent buds, in all cases, into leaf branches. Local stimuli, and a number of other causes at times, call forth leafy shoots from these. Nevertheless, in view of what has been said above, the following proposition might be formulated. Nutrition disturbed, and the development of the fruit product of the plant being threatened, by the loss of carbon-assimilating organs, the function of the latter may be taken up by some other part of the plant, either rudimentary or undifferentiated, their development into said organs being a direct result of that disturbance.

In the cases dealt with above, the function is either transferred to latent vegetative organs, or to undifferentiated tissues. It is, therefore, simply and easily comprehended. Observations have been made, however, which tend to show that in

the Angiosperms the vegetative function can be assumed not only by the floral envelopes, but also by the sporophyllary organs, more commonly by the macrosporophylls, or gynæcium. In some cases these open and expand into green leaves with the ovules, in a more or less imperfect stage of development, exposed. Perhaps no definite experiments have been carried out to demonstrate the cause of this transformation of form and function. It is supposed to be due to either excessive nutrition, or to some injury to the vegetative system of the plant. While such cases are unsatisfactory because of the lack of definite tests, they indicate that the sporophylls can assume the form and function of foliar organs when there is a disturbance of nutrition of a kind considered above. Since these sporophylls are, from a morphological standpoint, considered homologous with the green leaves, this change of function is not incompatible with that theory.

In the Pteridophytes direct experimentation proves beyond a doubt, that the sporophylls can be made to assume the form and function of the foliar organs by cutting off the latter, thus disturbing the nutrition and forcing the vegetation function on the sporophylls. The experiments performed upon *Onoclea sensibilis* and *O. struthiopteris* may be cited. In these cases after cutting off the early developed vegetative leaves the sporophylls appeared later in all stages of transformation, some complete vegetative leaves with only vestigial remnants in the form of rudimentary indusia to indicate to which series of organs they primarily belonged in the ontogeny of the plant. Between these and perfect sporophylls all gradations of intermediate forms occurred, the terminal portions of the sporophyll and of the pinnæ always being more fully expanded, while the basal portions of the same partook more or less completely of the true sporophyll. The details of the experiment and of the gradations of the development are given elsewhere, and cannot be dwelt upon here.

As an outgrowth of these experiments and observations a second proposition may be formulated as follows. Disturbed nutrition, resulting from the loss of the carbon assimilating organs of the sporophyte (vegetative leaves), may, and does,

force the vegetative function on the sporophylls, causing them to develop into more or less complete vegetative leaves.

The experiments on Onoclea convince me that there are a number of Pteridophytes, as well as Phanerogams, which would yield the same results following the amputation of their leaves, when carefully conducted, especially in those plants where, during one season, the vegetative leaves are developed sometime in advance of the sporophylls. Plants like some of the Lycopods would make extremely interesting ones to work with, and especially in the case of some of these should I expect to see a transformation of the sporophylls into vegetative leaves. This would be entirely in harmony with the relation and development of these organs. In species of Lycopodium and Selaginella all gradations between sporophylls with normal sporangia and the vegetative leaves can be found. The transitional stages are marked by the gradual degeneration of the sporangia on some of the leaves, the sporophyllary character being shown only by vestiges of the sporangia. Bower has shown how the strobilus of the Lycopods elongating by apical growth would result in the increase in the number of the sporophylls, and that the demand thus made on the vegetative system for nutrition would result in the transformation of some of the sporophylls to foliage leaves, accompanied by a corresponding sterilization of some of the sporangia. Practically it would disturb the balance of nutrition between the sporophyllary and vegetative systems, the effect being the same as it would be if some of the foliage leaves were destroyed.

In view of the ultimate purpose of this paper the question must be raised here as to whether this transformation of the sporophylls to foliar organs is a case of reversion, or whether it is an advance of a primary organ to a secondary organ of the sporophyte. It is my conviction that the latter alternative is the logical and true one, that we can by experimentation demonstrate phylogeny in ontogeny. Bower[2] has called attention to the importance which must be attached to the fact that the primary function of the sporophyte was not only the production of spores, but an increasing number; that the increase in

[2] Ann. Bot., VIII, pp. 345–365, 1894.

the mass of sporongenous tissue was necessarily accompanied by a sterilization of potential portions of the mass for purposes of protection, support, and for the conduction of nutritive material. From this condition he reviews the theoretical grounds for the relegation of the spore-producing cells to a superficial position, and the eruption of outgrowths on which the sporangia are supported, citing as illustrations of the early conditions of these outgrowths the strobilus of Equisetum and Phylloglossum. From the latter he traces the development of the elongated and branched leafy stem of species of Lycopodium by continued apical growth of its strobilus, while the sporangia on some of the lower sporophylls would be arrested, and the sporophylls themselves would develop as foliage leaves. For these and similar reasons elaborated by himself, he concludes, rightly I think, that the sporophylls are, from a phylogenetic point of view, *primary*, while the foliage leaves are *secondary*.

All the evidence which we have points to the fact that in the early development of the sporophyte, it was entirely dependent upon the gametophyte for nutrition including the supply of carbohydrates. The expanded green prothalloid structure performed the same function for the sporophyte, that foliage leaves of the sporophyte do in plants where this becomes independent of the gametophyte. This is practically true now in all the thalloid liverworts, and in all the Bryophyta is the sporophyte practically dependent upon the gametophyte for this function. In most of the Pteridophytes the sporophyte is dependent upon the gametophyte for its carbohydrates during the embryo stage. In some of the Pteridophytes, in the Gymnosperms, and in the Angiosperms, the gametophyte has entirely lost the function of carbon assimilation, this function being solely performed by parts of the sporophyte.

What influences led to the gradual transfer of this function of the gametophyte to parts of the sporophyte? Nutritive disturbances have been shown to play a very important part in the formation of sporophyllary organs quantitatively, in varying ratios between the vegetative and sporophyllary structures with increased food supply; in a tendency to produce a natural

but variable equilibrium between these two functional kinds of organs; and especially in the transformation of sporophyllary organs to vegetative ones. If these disturbances, especially in the nature of partial or complete loss of carbon assimilating organs of the sporophyte produce such an effect, why should there not be a similar influence brought to bear on the sporophyte, when the same function resides solely in the gametophyte, and a disturbing element of this kind is introduced? To me there are convincing grounds for believing that this influence was a very potent, though not the only one in the early evolution of sporophytic assimilatory organs. By this I do not mean that in the Bryophyta, for example, injury to the gametophyte would now produce distinct vegetative organs on the sporophyte, which would tend to make it independent of the gametophyte. But that in the Bryophyte-like ancestors of the Pteridophytes an influence of this kind did actually take place appears to me reasonable.

In the gradual passage from an aquatic life, for which the gametophyte was better suited, to a terrestrial existence for for which it was unadapted, a disturbance of this function was introduced. This would not only assist in the sterilization of some of the sporogenous tissue, which was taking place, but there would also be a tendency to force this function upon some of the sterilized portions of the sporophyte, and to expand them into organs better adapted to this office. As eruptions in the mass of sporogenous tissue took place and sporophylls were evolved, this would be accompanied by the transferrence of the assimilatory function of the gametophyte to some of these sporophylls. Even the protophylls may have originated by the eruption of certain of the sterile portions of the sporophyte under the influence of disturbed nutrition.

The sporophyte from its nature presented greater possibilities in the way of the elaboration of a complex, robust, perennial inhabitant of terrestrial zones. Increased sporogenous tissue was necessarily accompanied with a more bulky structure, which then necessitated a differentiation of its tissue by sterilization of certain external, then internal, parts for protection and circulation. Robust types of land plants could more naturally be developed from such a phase than from the ex-

panded and delicate gametophyte. When the sporophyte had largely assumed this function of the gametophyte, and by the development of absorbing organs in the soil was enabled to live an independent existence, it became gradually established, as conditions changed, in situations where the gametophyte could not exist. It has thus become the dominating vegetative feature of most land areas, while the gametophyte in these higher forms, has become an organ entirely dependent upon the sporophyte for nourishment, or has been developed into an organ to serve a secondary purpose in the nourishment of the sporophytic embryo.

PROGRESS IN AMERICAN ORNITHOLOGY. 1886–1895.

BY R. W. SHUFELDT, M. D.

What I have to say here in reference to the progress in American ornithology for the past nine or ten years is prompted by the recent appearance of the second edition of *The A. O. U. Check-List of North American Birds.* Most naturalists are familiar with the first edition of this work, it having been published in 1886. It was officially promulgated by the American Ornithologists' Union, and zoologists the world over have carefully considered "The Code of Nomenclature" that formed a part of the volume. Moreover, it contained a List of North American Birds which had been prepared according to the aforesaid Code of Rules, and *classified* in accordance with the views of the majority of the committee appointed by the Union to prepare it. In so far as the orders and families of this classification were concerned, the arrangement could be appreciated at a glance by reference to the Table of Contents of the book, and, as for the List itself, it not only was intended to represent the nomenclature of the Birds, but "a classification as well" (p. 15). At the close of the volume was presented a "Hypothetical List" to which had been referred those species and subspecies the zoological status of which could not be satisfactorily determined; and following this was a list of the fossil species of North American birds.

As the years passed by a second edition of this book was

eagerly looked for by zoologists at large, but it did not make its appearance until towards the close of December, 1895. It comes to us in the same form as its predecessor, but it does not appear to be as substantially bound or printed upon as good paper. Apart from the substitution of one member of the committee for another, it is likewise gotten out under precisely similar auspices, plans, objects and general arrangement. From it, however, has been omitted the "Code of Nomenclature," but in it are included all the new existing and fossil birds known to the committee, and which were not in the first edition of the Check-List. For this and minor changes it has but 372 pages against 392 of the original volume. In its preface it contains "extracts from the Introduction to the Code of Nomenclature," intended to serve "to explain the scope and plan of the Check-List, including the method of incorporating additions."

The second edition, then, of this work may be taken as setting forth the progress in North American ornithology as understood by a committee appointed by the American Ornithologists' Union, and for a period extending between the years 1886 and 1895 inclusive. In considering this from such a standpoint, let us first take into account the number of species and subspecies added to, or subtracted from, the List of 1886, in connection with other changes, and the same for the "hypothetical list" and for the "fossil birds." After this I will consider what improvements, if any, have been made in the matter of classification.

Designating the two volumes simply by the years of their publication, as 1886 and 1895 respectively, we find that in the first group of birds presented, or the Order PYGOPODES, there were included, in 1886, *33 species* and *4 subspecies*, while in 1865 but *32 species* are given and *4 subspecies*, the change being due to the omission of *Synthliboramphus wumizusume* (Temminck's Murrelet, No. 22).

In 1886, the Great Auk (*Plautus impennis*, No. 33) was "Believed to be now extinct," while in 1895 it is confidently asserted to be "Now extinct." This being the case, we would like to inquire what place has it in a list of the *existing* birds of this or any other country? It is simply absurd to include birds that have *no existence* in nature in a list of living forms.

Passing to the second group, or Order LONGIPENNES, we find upon comparison that in 1886 it contained *44 species* and *4 subspecies*, while in 1895 it is seen to contain *46 species* and *4 subspecies.* The additions here are the two new species *Larus barrovianus* and *Larus minutus* (a straggler). Another change in this group is the calling Pallas's Gull (*Larus cachinnans*, No. 52) the Vega Gull (*Larus vegæ* [1895]).

In the third group, or the Order TUBINARES, were included, in 1886, *31 species* and *3 subspecies*, to which list was added a new species in 1895 (*Oceanodroma macrodactyla*), making *32 species* and *3 subspecies* for that year. Peale's Petrel (*Æstrelata gularis*) (No. [99]), was likewise changed to the Scaled Petrel (*Æ. scalaris*) in this group.

In 1886, the fourth group, or the Order STEGANOPODES, was made to contain *17 species* and *5 subspecies*, and, in 1895, *19 species* and *5 subspecies*, the increase being due to the addition of the two new species of Gannets, *Sula gossi* and *S. brewsteri.*

Coming to the fifth group, or the Order ANSERES, there were contained in it in the 1886 List, *51 species* and *6 subspecies*, and, in the 1895 List, *51 species* and *8 subspecies*, the change being effected as follows: *Anas fulvigula maculosa*, the Mottled Duck, was added as a new subspecies, and *Somateria mollissima* was made the subspecies *S. m. borealis*; finally, *Chen cærulescens* was included in the list. *Camptolaimus labradorius* now being "extinct," it has no place in the List and ought not to appear there.

Group six, the Order ODONTOGLOSSÆ, remains the same, each List having the *1 species* of Flamingo (*P. ruber*).

In the seventh group, or the Order HERODIONES, there were to be found *19 species* and *2 subspecies*, to which were added in the 1895 List a new species and a new subspecies (*Ardetta neoxena* and *Ardea virescens frazari*). *Botaurus exilis* becomes in the new List *Ardetta exilis*, and *Ardea rufa* becomes *A. rufescens*, while the "subgenus" *Nycterodius* is changed to *Nyctanassa.*

The Order PALUDICOLÆ (eighth group) in the 1886 List, contained *17 species* and *3 subspecies*, to be changed in the 1895 List to *21 species* with only *1 subspecies.* This was effected by considering the subspecies *Rallus longirostris crepitans* (1886) to be the species *Rallus crepitans*, and adding also to the 1896

List the species *Rallus scottii* and *Rallus longirostris caribæus.* The subspecies *Porzana jamaicensis coturniculus* (1886) became the species *P. coturniculus* (1895). From these changes a less important one is to be noted, *viz.: Rallus longirostris saturatus* became, in 1895, *R. crepitans saturatus* (No. 211 *a*).

Passing to the Order LIMICOLÆ (ninth group), it is to be noted that in the List of 1886 there were included *66 species* and *4 subspecies*, and in 1895 these became *68 species* and *6 subspecies*, the changes being the addition of *Tringa damacensis* (a straggler); the two subspecies *Totanus solitarius cinnamomeus* and *Symphemia semipalmata inornata*, and the new species *Hæmatopus frazari.* Other changes in this group are the subgenus *Rhyacophilus* (1886) to read the subgenus *Heladromus*, and the name of the Mexican Jacana, instead of being *Jacana gymnostoma* (Wagl.), is now *J. spinosa* (Linn.).

Coming next to the Order GALLINÆ (tenth group), it is to be seen that in the 1886 List *22 species* are given and *18 subspecies*, while in 1895 there are *21 species* and *22 subspecies*. This reduction in the number of species was caused by the dropping out of *Colinus graysoni*, while the subspecies were increased by adding to the List *Oreortyx pictus confinis*, *Tympanuchus americanus attwateri*, and the two Turkeys, *M. g. osceola* and *M. g. ellioti.* *Callipepla gambeli* of the old work was corrected to read *C. gambelii.*[1]

In the eleventh group, or the Order COLUMBÆ, there were included in the List of 1886 *12 species* and no subspecies. In the 1895 List we find but *11 species*, while *4 subspecies* have been added. *Columba fasciata vioscæ* was recognized, while *Engyptila albifrons* (1885) became *Leptotila fulviventris brachyptera.* There was also added the subspecies *Columbigallina passerina pallescens* and the species *Columbigallina passerina* has became the subspecies *C. p. terrestris.*

There appeared *53 species* and *29 subspecies* in the twelfth group or Order RAPOTRES in 1886, while in 1895 these were increased to *54 species* and *37 subspecies.* In this group the

[1] In making these comparisons it is to be understood that they are direct between the Check-List of 1886 and that of 1895, and that the seven supplements (1889-94) and the Abridged Edition of 1889 are not taken into consideration. The second edition (1895) is taken to be the final finding of the Committee.

ume (1895) it is seen to include *26 species* and *7 subspecies.* The following alterations, subtractions, and additions have been made in the interim. *Antrostomus vociferus arizonæ* becomes *A. v. macromystax.* The genus *Phalænoptilus* has two new subspecies, *P. n. nitidus* and *P. n. californicus. Chordeiles v. minor* becomes *C. v. chapmani*, and *Chordeiles texensis* becomes *C. acutipennis texensis.* The genus of Swifts formerly in *Micropus* are now in *Aërqnautes.* We have but one species of it in this country—the White-throated Swift, which, known formerly as *Micropus melanoleucus*, now is written *Aëronautes melanoleucus.* Among the Hummingbirds we have the new species *Trochilus violajugulum, Trochilus costæ* is changed to *Calypte costæ*, and *T. anna* to *Calypte anna*, in other words, the subgenus CALYPTE has been raised to the rank of a genus. So likewise the subgenus SELASPHORUS has been similarly dealt with, and another species added to it, *viz.: Selasphorus floresii.* Also the subgenera STELLULA and CALOTHORAX become genera, each containing a single species. *Trochilus heloisa* has been omitted from the list, and *Basilinna leucotis* added to it.

Finally, we come to the last, or seventeenth group, that vast assemblage known as the PASSERES. It will not be as convenient to deal with these as the foregoing sixteen groups were dealt with, as many of the families contain more birds than several of the other "Orders" combined, so I shall resort to tabulating the comparisons, comparing family with family.

This comparison goes to show that in 1886 there were recorded 313 species of North American *Passeres*, and, in 1895, 321, giving a gain of 8 species for the nine years, while, for the same years and interval of time, there were 117 subspecies, and, in 1895, 185, showing a gain of 68 subspecies.

With respect to the *Cotingidæ*, the single species indicated in the above Table is Xantus's Becard (*Platypsaris albiventris*). Among the *Tyrannidæ* the following changes were made: (1) The new subspecies *Myiarchus cinerascens nuttingi* was added, and also (2) the subspecies *Contopus richardsonii peninsulæ*; (3) the species *Empidonax cineritius* is added, and (4) *Empidonax acadicus* becomes *E. virescens*, as does (5) *E. pusillus* become *E. trailli*, and (6) *E. trailli alnorum* (1895) has taken the place of

TABLE COMPARING THE PASSERES.

Families.	1886.		1895.		Remarks.
	Sp.	Subsp.	Sp.	Subsp.	
CLAMATORES.					
Cotingidæ.............			1		This family not in the 1886 list.
Tyrannidæ	29	7	32	9	
OSCINES.					
Alaudidæ..............	2	7	2	10	
Corvidæ	15	9	17	13	
Sturnidæ...............	1	0	1	0	(*Sturnus vulgaris*).
Icteridæ................	19	7	19	8	
Fringillidæ............	87	44	87	71	
Tanagridæ.............	5	1	6	1	(*Piranga rubiceps*).
Hirundinidæ..........	7	0	10	1	
Ampelidæ	3	0	3	0	
Laniidæ	2	1	2	2	(*L. l. gambeli*).
Vireonidæ.............	11	5	11	9	
Cœrebidæ.............	1	0	1	0	
Mniotiltidæ...........	57	9	57	13	
Motacillidæ...........	6	1	6	1	No changes.
Cinclidæ	1	0	1	0	No changes.
Troglodytidæ	24	7	24	16	
Certhiidæ..............	0	2	0	4	
Paridæ.	19	9	20	15	
Sylviidæ	7	1	7	2	*Polioptila cærula obscura* added.
Turdidæ................	15	7	14	10	
Total..............	313	117	321	185	

*E. pusillus traill*i; (7) *E. obscurus* (1886) becomes *E. wrightii*, and, finally, (8) *Empidonax griseus* appears as a new species.

In the family *Alaudidæ*, three new subspecies of "Horned Larks" are added to the List (*O. a. adusta*, *merrilli* and *pallida*).

In the family *Corvidæ* we have *Cyanocitta stelleri annectens* added as a subspecies, and *Aphelocoma cyanotis* as a species. In this genus occur also *A. californica hypoleuca*, *A. c. obscura* and *A. insularis*. A new subspecies of Raven is also recognized (*C. c. principalis*). Finally, and very properly, the generic name *Picicorvus* is replaced in the 1895 List by *Nucifraga* of Brisson.

Among the *Icteridæ* I note that *Dolichonyx o. albinueha* (1886) has been omitted, and that the genus *Callothrus* of Cassin has been adopted and made to contain *C. robustus*, which was formerly *Molothrus æneus* (1886). The subspecies *Agelaius phœniceus sonoriensis* and *A. p. bryanti* have been added.

The largest of all the passerine groups of birds is the family *Fringillidæ*. The following synopsis will show the changes that have been made in it since 1886:

SPECIES ADDED.

Junco ridgwayi.
Junco townsendi.
Melospiza insignis.
Euetheia canora.

SPECIES OMITTED.[1]

Carpodacus frontalis.
Zonotrichia intermedia.
Zonotrichia gambeli.
Sporophila morelleti.

SUBSPECIES ADDED.

Coccothranstes vespertinus[2] *montanus.*
Carpodacus mexicanus frontalis.
Carpodacus mexicanus ruberrimus.
Spinus tristis pallidus.
Plectrophenax nivalis townsendi.
Poocætes gramineus affinis.
Ammodramus henslowii occidentalis.
Ammodramus caudacutus subvirgatus.
Ammodramus maritimus peninsulæ.
Ammodramus maritimus sennetti.
Zonotrichia leucophrys intermedia.
Zonotrichia leucophrys gambeli.
Spizella pusilla arenacea.
Junco hyemalis shufeldti.
Junco hyemalis thurberi.
Junco hyemalis pinosus.
Junco hyemalis carolinensis.
Amphispiza belli cinerea.
Melospiza fasciata rivularis.
Melospiza fasciata graminea.
Melospiza fasciata clementæ.
Pipilo fuscus senicula.
Cardinalis c. canicaudus.
Pyrrhuloxia sinuata beckhami.
Pyrrhuloxia sinuata peninsulæ.
Guiraca cærulea eurhyncha.
Passerina versicolor pulchra.
Sporophila moreleti sharpei.

SUBSPECIES OMITTED.

Carpodacus frontalis rhodocolpus.

[1] So long as the geographical range of a species is extended it makes not an iota's difference how that extension has been accomplished, whether it has been through human agency ("introduction"), or by other means, for when the bird becomes thoroughly established in sufficient numbers, and breeds, it is entitled to a place in any List presenting the ornis of the country into which it has come.

The Starling (*Sturnus vulgaris*) essentially gained a place and recognition, in the A. O. U. "List" from the fact that it has been successfully "introduced" from abroad. If this be granted, the Committee were guilty of very unscientific practice when they omitted the English Sparrow (*Passer domesticus*) from the "List," (also *Passer montanus*), and it can only stand as an example of how far men will allow their prejudices to carry them, and blind their scientific instincts.

[2] Spelled "*vespertina*" in 1886 edition.

Between the species *Carpodacus cassini* and the subspecies *Carpodacus mexicanus frontalis*, the subgenus BARRICA is introduced.

Progne subis hesperia has been added to the Swallows (*Hirundinidæ*), as well as *Progne cryptoleuca*, and *Petrochelidon fulva* as a straggler. The Bahaman Swallow (*Callichelidon cyaneoviridis*) having accidentally occurred on the Dry Tortugas, it introduces both the species and genus to which it belongs.

To the *Vireonidæ* were added *V. s. alticola* and *V. s. lucasanus*, as well as *V. n. maynardi* and *V. huttoni obscurus*.

In the case of the family Cœrebidæ, the genus Certhiola is superseded by Cœreba, and consequently *Certhiola bahamensis* becomes *Cœreba bahamensis*.

But few changes are noticeable among the Mniotiltidæ, and these principally the addition of new subspecies. The Dusky Warbler (*Helminthophila celata sordida*) is one of these, *Dendroica æ. sonorana*, *Geothlypis trichas ignota* and *Geothlypis poliocephala ralphi* being the others.

To the family Troglodytidæ there are to be noted a number of additions and some few changes. They may be shown thus:

1886.		1895.
Harporhynchus longirostris	=	*H. l. sennetti.*
		Subsp. added.
		Harporhynchus cinereus mearnsi.
Genus Campylorhynchus	=	Genus Heleodytes.
C. brunneicapillus	=	*H. brunneicapillus.*
		Subsp. added.
		H. b. bryanti.
714. *C. affinis*	=	Omitted.
		Subsp. added.
		713 *b. H. b. affinis.*
		Catherpes mexicanus punctulatus.
		Thryothorus ludovicianus lomitensis.
		Species added.
		Thryothorus leucophrys.
Thryothorus brevicaudus	=	*T. brevicauda.*

Subsp. added.
Troglodytes ædon aztecus.
Cistothorus palustris paludicola.
Cistothorus palustris griseus.
Sp. added.
Cistothorus marianæ.

In the Certhiidæ, *Certhia familiaris mexicana* becomes *C. f. alticola*, and *C. f. montana* and *C. f. occidentalis* are added as new subspecies.

Among the Nuthatches and Tits (*Paridæ*) the following additions and changes are to be noted.

1886.	1895.
	Subsp. added.
	Sitta carolinensis atkinsi.
	Sitta pygmæa leuconucha.
	Parus bicolor texensis.
	Subgenus PARUS inserted.
	Parus carolinensis agilis.
	Parus hudsonicus stoneyi.
	Parus hudsonicus columbianus.
	Species added.
	Psaltriparus santaritæ.
[745] *P. mlanotis*	= *Psaltriparus lloydi.*

Finally, among the family *Turdidæ*, we have:

1886.	1895.
Turdus f. salicicolus	= *T. f. salicicola.*
Sialia mexicana	= *S. m. occidentalis.*
	Subspecies added.
	Sialia mexicana bairdi.
	Sialia m. anabelæ.

I am now prepared to present some comparisons with respect to the numbers of species and subspecies in 1886 and 1895, and these may be best shown again by means of a Table, as follows:

TABLE.

Group.	Recorded in 1886.		Recorded in 1895.	
	Sp.	S. sp.	Sp.	S. sp.
Pygopodes	33	4	32	4
Longipennes	44	4	46	4
Tubinares	31	8	32	8
Steganopodes	17	5	19	5
Anseres	51	6	51	8
Odontoglossæ	1	0	1	0
Herodiones	19	2	20	3
Paludicolæ	17	8	21	1
Limicolæ	66	4	68	6
Gallinæ	22	18	21	22
Columbæ	12	0	11	4
Raptores	53	29	54	37
Psittaci	1	0	1	0
Coccyges	9	0	9	4
Pici	23	11	22	14
Macrochires	26	3	26	7
Passeres	318	117	321	185
Grand total	738	209	755	307

This table will go to show that taking the species and subspecies together in 1886, they amounted to 947, while in 1895 there were no less than 1062. In substracting the number of species recorded in 1886 from those in 1895, we find that there has been a gain of 17 species, and in dealing with the subspecies in the same manner, we find that there has been a gain of 98 subspecies. A study of this table is interesting in other ways, as the making of similar comparisons of any single group, or those groups exhibiting the greatest increase and the causes therefor; but all such data can be easily appreciated by the reader from what has been given above, and my space will not admit of enlarging upon it here.

For a moment we may now turn to the "Hypothetical Lists" of the two editions of the work I have under consideration. In 1886 there were 26 species and subspecies relegated to its hypothetical list, ranging from 1 to 5 for the families in which they occurred. In 1895, *Diomedea exulans* is seen to be added

to the number, while *Chen cærulescens* is considered to belong to our avifauna, and has therefore been added to the list of 1895. The Swallow-tailed Gull, given as *Oreagrus furcatus* in 1886, is now *Xema furcata*, and *nine* examples of it are said to be known to science, instead of only *three*, as reported in 1886. *Numenius arquatus* and *Chordeiles v. sennetti* are also added to the hypothetical list of 1895, while *Buteo fuliginosus* is ignored entirely.

Coming at last to the "List of Fossil Birds of North America," we find that as compared with the existing species, a greater number has been added to those previously known than there has been to the list of living birds. In 1886 there were 46 species of fossil birds reported, while in 1895 there were 64 upon the record. No doubt there are others that should have been added to these, overlooked by the Committee, as, for example, the rail-like bird called *Crecoides osbornii* Shufeldt, from the Upper Cenozoic of the staked plains of Texas. Marsh increased the list of Cretaceous Birds by the addition of three species, and the Tertiary Birds by one species, while Shufeldt added no less than fourteen new species of fossil birds as belonging to this latter geological horizon.

To this list should also have been added those belonging to the "Recent Era," as, for example, *Plautus impennis*—the Great Auk—and *Camptolaimus labradorius*—the Pied Duck. Of the first named species there is an abundance of subfossil material in existence, and of the latter there are doubtless bones to be found in the dried skins of specimens in museums and elsewhere. Both birds are quite as extinct as is the famous Jurassic bird, the *Archæopteryx* of the Solenhofen States of Bavaria.

But the addition of new birds to the avifauna of any country is by no means all there is to ornithology. Nor does the science see its end when these new forms have been described, figured and printed in an official list. The importance of giving a new bird a name, recording its superficial characters, and defining its geographical distribution is not to be underrated, the more especially so as all this greatly helps those who are engaged with the science of their morphology, their taxonomy, and their present affinities and past origin. One of the chief

aims of ornithology is to establish the true relations of existing and extinct forms of birds to each other, and to other groups of animals that are either to be found living at the present time, or else have existed during past ages of the earth's history. In other words, the true classification of birds is to be sought for, and ornithology in this sees its most difficult problem and its final goal.

But the knowledge of the origin of this most perplexing group of vertebrates, their evolution, and our power to correctly classify them can only come to us in one way; and that is through a complete understanding of their structure, and a comprehension of the anatomy of those groups more or less nearly related to them. Other departments, however, can lend great assistance here, and the avian taxonomist can have much light thrown upon his arduous task through the revelations of researches in the fields of physiology, of geographical distribution, nidology, paleontology, and other biological sciences.

With these facts before us, it is with no little interest that the taxonomist scans the pages of the second edition of "The A. O. U. Check-List of North American Birds," with the view of ascertaining what evidences there may be in the direction of a better knowledge of the classification of our birds. There may have been some excuse for the numerous symptoms of the somewhat antiquated taxonomy that characterized the arrangement of North American birds in the 1886 edition of the A. O. U. Check-List, but not so this last one, provided we find that the earlier classification has been retained. For, be it known, in the meantime, that is, from 1886 to 1895, the avian morphologists had not been idle. There were very many useful suggestions in the admirable work done by Dr. Stejneger that appeared shortly before the 1886 edition was printed. This was followed, in 1888, by the superb volumes of Fürbringer, with one of the most elaborate classifications of birds the world has ever seen; Seebohm, of England, had done a great deal, while the present writer had published accounts of the osteology of nearly every family of N. American Birds, and Mr. Lucas stands prominent in his excellent anatomical work upon many of the groups. English pens had contributed memoir after

memoir along similar lines, and one has but to turn to the essays and volumes of Newton, Gadow, Beddard, T. J. Parker, Sharpe and many others to appreciate this. But for one to fully know what a deal was done during the nine years I speak of, it is but necessary to read the enthusiastic address of Fürbringer given before the Section for the Anatomy of Birds at the Second International Ornithological Congress, held at Budapesth in 1891. A powerful light has been thrown upon the structure and affinities of the various groups of birds, and has it in any way affected the classification of the 1895 Check-List of North Ameican Birds, that is, in so far as the main groups are concerned? Not in the least. Apart from the addition to the List of the family *Cotingidæ*, the taxonomy of the orders and families as given in 1886 are identical with the arrangement reproposed in 1895. For example, we still find the Grebes, Loons and Auks retained together in the Order PYGOPODES, with the first-named separated from the last two by subordinal lines; whereas, Fürbringer, Thompson, Sharpe, myself and others, all of whom have examined the structure of these birds, have shown the affinity existing between the Grebes and Loons, and that these two families are very distinct from the Auks. The Auks, in fact, occupy a group by themselves, and are more nearly related to the Longipennes. Fürbringer separated them very widely from the Grebes and Loons, in which opinion Sharpe and others concur. That the Longipennes and the Limicolæ are akin is now generally recognized by those who have studied the anatomical structure of the members of the two groups, yet in the A. O. U. classification, six entire Orders stand between the Gulls and the limicoline assemblage. Fürbringer makes a "Gens" Laro-Limicolæ, and Sharpe keeps the two groups close together. As long ago as 1867 Professor Huxley clearly showed the osteological agreement between the skull of a Plover and that of a Gull.

That the Fowls (*Gallinæ*), Pigeons (*Columbæ*), Raptorial Birds (*Accipitres*), Parrots (*Psittaci*) and the Cuckoos (*Coccyges*) as groups should stand in lineal series I can well believe—but as Gadow, Hubert Lyman Clark, myself and others have frequently pointed out, the Owls do not belong with the Acci-

pitres or the Falcons and their kin, while I make separate groups for the Cuckoos, Kingfishers and Trogons. The Woodpeckers are not separated from the Passeres by the Goatsuckers, Swifts, and Hummingbirds, as the A. O. U. List now have them arranged, but the Woodpeckers, in the list of North American Birds, taxonomically arrayed, should stand immediately next to the Passeres, while the "Macrochires" is a thoroughly unnatural group, inasmuch as birds are no longer classified and restricted to groups on account of their having long pinions.

Finally we come to the *Passeres* with the lineal arrangement of the 21 families composing the group. Now, as a classificatory scheme, this lineal method of showing it is unsatisfactory in the extreme, but it appears to be the only available one to adopt in the Lists in books. A "tree" shows what is meant much better and truer, but it can never form a part of a List. Still these Lists show something, for we can, among other things, indicate in them the families that should, in our opinions, occupy the extremes—as, for instance, the *Tyrannidæ* and the *Corvidæ*, but in numerous cases it will be found to be exceedingly difficult to complete the sequence, even to carry out the hopes of the classifier. However, marked violences can usually be avoided, and marked affinities often shown in a classification of this kind.

The scheme adapted in the A. O. U. Check-List, although not altogether a bad one, is capable of showing a more truthful arrangement of the families of passerine birds. In the first place, this List should be completely reversed; then the Thrushes (*Turdidæ*) placed more nearly where they belong; and the *Laniidæ* removed very much nearer the Clamatorial end of the sequence, and away from the Vireos, with which family they have no special affinity. Thus much for the progress in American ornithology during the past ten years; our ornis has been most carefully studied in so far as the identification of new species and subspecies is concerned, but the matter of scientific classification of birds demands increased attention, and it is to be hoped that a greater number of avian morphologists will arise, and should that come about, the clas-

sification of the next edition of the A. O. U. Check-List will, in truth, be archaic if again printed without change; the 1895 one, just out, is a number of years behind the science of the times, so we may easily imagine how very backward it will appear ten years hence.

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

BY ERWIN F. SMITH.

Although Sachs' notion that the ascending water current in plants passes through the walls of the vessels and not through their interior, was rendered very doubtful long ago, if not thoroughly exploded, by the experiments of Elfving, Vesque, Erera, Boehm and others, the old statement still remains in many of the text books and continues to be taught. For this reason, and because the papers of the opponents of this view do not seem to have received much attention in this country, while Dr. Sachs' *Lectures on the Physiology of Plants* in H. Marshall Ward's admirable translation, is known and read everywhere and deservedly so, it may be worth while to call attention once more to the present state of our knowledge on this subject. This I shall do by presenting some experiments of my own, which were made a year ago on *Cucumis sativus* L. These were undertaken partly to verify some of Strasburger's statements in his book *Ueber den Bau und die Verrichtungen der Leitungsbahnen in den Pflanzen*, and partly to determine, as accurately as possible, the path of the water current in Cucurbitaceous stems, subject to the attack of *Bacillus tracheiphilus*. They were begun about March 20, and continued till some time in April, the weather being by turns warm and cold, sunny, windy, cloudy and rainy. About 30 well grown cucumber vines were experimented upon, the following being selected as typical. All were under glass in a large hot-house, devoted to the cultivation of cucumbers for the winter market. None of the vines trailed on the ground, but all were trained up on

stakes or over high strung wires. A sharp razor was used in cutting the stems.

Before proceeding to the experiments, it will be necessary for the sake of those who are not familiar with the structure of the cucumber stem, to briefly indicate its anatomy. The bundles are bi-collateral, i. e., there is a group of phloem on the inner, as well as on the outer face of the bundle. The outer phloem is separated from the central strand of xylem by a cambium zone, which is restricted to the bundle, i. e., not inter-fascicular. The inner phloem is separated from the xylem, by a meristematic tissue structurally much like cambium, but functionally different. The phloem consists of numerous large sieve tubes, with the usual accompanying cells and cambiform cells. The central or xylem strand of the bundle consists principally of large pitted vessels, held together by shorter tracheids and lignified parenchyma. The mode of origin of the pitted vessels, i. e., out of a series of large superposed cells, is plainly visible, the cross septa being sometimes present and perfect, but more often partially wanting or reduced to mere rims on the inside of a continuous tube. The walls of these tubes contain thousands of very thin places, or actual perforations, (in many cases the central slit takes no stain), and the tubes appear to be admirably adapted for water reservoirs, any adjacent portion of the plant being clearly able to draw from them without hindrance. It appears to me somewhat doubtful, whether they also function as direct water carriers. This business seems more suited to the spiral vessels which occur in a little group on the inner face of the xylem strand, embedded in a delicate, non-lignified living parenchyma, which frequently contains chlorophyll. The walls of these spirals are not pitted; their bore is almost capillary, i. e., much less than that of the pitted vessels; and they are of great length, probably by means of splicings extending as open tubes the whole length of the vine. That they are of more fundamental importance to the plant than are the pitted vessels, appears from the fact, that they are the only tubular parts of the xylem to be found in the smaller roots, and are also the only xylem-vessels passing out of the stems into the peti-

oles and ramifying in the veins of the leaves. It seems to follow from this that whatever be the path of the water current in the stem itself, it can enter the body of the plant in quantities sufficient for transpiration purposes only along the pathway of the spirals, and can reach the leaves only through the same channels.

The pitted vessels are probably sometimes nearly full of water, and at other times nearly empty, the amount depending on the quantity in the soil and on the activity of transpiration. Owing to the number of very thin places or actual perforations in their walls, they undoubtedly contain air at all times and probably often in large quantities. I regard these vessels as water reservoirs. In this capacity they appear to be admirably adapted to serve the needs of a class of plants which (on account of the extent and unprotected nature of their transpiring surface) often make sudden and very large demands on the stem for water,—demands greater than can be met by the immediate activity of the roots. There is, however, nothing against the supposition that when they are not full of water, they may also serve as aerating organs, the stems being alive and chlorophyll-bearing clear to the center. The function of the spiral vessels, according to my conception, is quite different. They also contain a greater or lesser quantity of water, according to the activity of transpiration and the amount procurable from the soil or from the neighboring reservoirs (the pitted vessels), but unlike the pitted vessels, they are surrounded by a living, non-lignified, non-lacunose parenchyma, and there is no free access of air to their interior, but, on the contrary, so far as we can judge from the anatomical structure, this part of the plant has been developed with special reference to keeping it out. When the spirals are not full of water, they probably contain rarefied air. The very thin walls of these spiral vessels bear on their inner face lignified annular or spiral thickenings, which are probably of great service in strengthening the delicate walls, so that they may be strong enough to resist the collapsing tendency of the vacuum pull due to the osmotic pressure, and yet remain thin enough to readily allow water to filter into

them or out, as the case may be. Such, roughly sketched, is the nature of the bundle, the xylem part of which contains 5 or 6 spirals and from 12 to 15, or more pitted vessels. The cucumber stem, exclusive of the hypocotyle, usually contains 9 such bundles, the 5 larger ones forming an interrupted ring or cylinder in the central part of the stem, and the four smaller ones alternating with the larger ones nearer the surface of the stem, the fifth bundle of the outer series being usually wanting in this species. These bundles are separated from each other by thin-walled, living cells which are nearly iso-diametric. The central portion of this parenchyma and that between the bundles, may be designated as medullary tissue, and that farther out as cortical parenchyma, although all of this fundamental tissue bears chlorophyll, and is used to store starch in prior to the development of the fruit. Outside of the bundles, and not far from the surface of the stem, is a compact tissue formed of numerous elongated, thick-walled, flexible, strengthening cells. These are the bast fibres, forming collectively, the stereomatic sheath. This sheath is several rows of cells thick and forms an broken or nearly unbroken cylinder in the young stem, but is afterwards ruptured longitudinally into a dozen or more strands by the growth of the stem in thickness. Between these strands of stereome, the cortical parenchyma finds its way to the epidermis, except where the latter is specially strengthened by sub-epidermal strands of collenchyma. The stem appears to have so developed as to secure every advantage to be derived from a combination of lightness with flexibility and strength.

To indicate the movement of the water in the stems and leaves, various aniline stains were tried, e. g., eosine, soluble nigrosene, methyl green, methyl orange, acid fuchsin, etc. Eosine proved by far the most satisfactory, none of the other stains moving with anything like the same rapidity, and some of them causing copious precipitates in the vessels. None of the substances in the sap of the cucumber vessels cause any precipitate with eosine, and it is probable that dilute solutions of this substance, while clearly poisonous to the plant, move with the same rapidity as pure water, at least at first.

1. Upward Movement of One Per Cent. Eosine Water Through Cut Stems.

(No. 9). This vine was 215 centimeters long and bore a number of small leaves and 17 large ones, 10 of which averaged 20 cm. in breadth. March 21, 2:30 p.m. The stem near the earth was cut under water and put at once into 1 per cent. eosine water.[1] 2:43 p.m. The stain is now distinct in all of the principal veins of a leaf only 15 cm. from the end of the stem, i. e., it has passed up the stem a distance of two meters in less than 13 minutes; probably in 10 to 12 minutes. 2:47 p. m. The red stain is now distinct in the veins of the small undeveloped uppermost leaves of the stem. 3:25 p. m. Slight droop of the foliage, but much less than in No. 10 (a similar vine in 10 per cent. eosine water). Foliage decidedly less red than that of No. 10. 4:35 p. m. Leaves drooping very decidedly. The leaves of No. 10 are flabbier and redder, but much less fluid has passed up the stem. 5:10 p. m. About 21 cc. of the eosine water has passed up the stem in 2 hours and 40 minutes. March 22, noon. Leaves, tendrils and surface of the young fruits reddish. The stain does not make its way readily into the coiled tips of the tendrils. Many of the leaves are dry shriveled, so that they crackle on touch. Stem not shriveled. Most of the petioles are still turgid and but little stain is visible in them, except in a few toward the top of the vine. 4:00 p. m. Not nearly so red as No. 10. Stem quite green and not noticeably shriveled. The stem of No. 10 in the 10 per cent. eosine has shriveled decidedly to-day. March 23, 12:25 p. m. About 10 cc. of the stain has passed up the stem since last night. 4:30 p. m. About 10 cc. of the stain has gone up the stem since the last record. March 25, 12:30 p. m. About 20 cc. of the stain has passed up the stem since the last record. Most of the leaves are crisp dry, but the terminal ones are still moist, although shriveled and soft like old rags, the parenchyma being yellow and the veins bright red. Most of the petioles are bright red, and all of them are limp and hang straight down; the stem has shriveled and become reddish, except the

[1]Distilled water containing Dr. Grübler's "Eosine Soluble in water."

submerged part, which has kept its turgor and resists diffuse staining better than the parts in the air. The plant is dead. March 26, 2:40 p. m. About 12 cc. of the eosine has passed up the stem since yesterday p. m.

In this plant over 40 cc. of the eosine water passed up the stem during the first 24 hours, and in the next four days an additional 45 cc., part of which after the plant was dead.

Vine No. 1 which was 188 centimeters long, also took up the eosine water after it was dead. This absorption of the stain continued long after the leaves had become dry-shriveled, and did not entirely cease until all parts of the bright red stem became bone-dry. This vine was under observation 14 days, during which time about 150 cc. of 1 per cent eosine water passed up the stem, only 57 cc. of which went up during the first 49½ hours.

(No. 25). This was a young vine, measuring 100 centimeters above the cut surface. It bore 17 leaves, the largest 6 averaging 13 cm. in breadth. March 28, 11:56 a. m. The stem was cut under water and put at once into an alkaline eosine water, made by putting 1 gr. eosine into 100 cc. of $\frac{N}{10}$ caustic soda (the solution stood in the laboratory over night and became darker colored). 12:01 p. m. The red stain is distinctly visible in the veins of all the leaves, even the uppermost ones, i. e., it has gone straight up a distance of one metre in 5 minutes. It is sunny and windy, and transpiration is active. The dry bulb registers 22° C.; the wet bulb 17.3° C. 12:10 p. m. The foliage begins to droop. 12:40 p. m. Foliage wilting very badly. 2:10 p. m. About 5 cc. of the stain have passed up the stem. The lower leaves have begun to crisp at the margin. March 29, 2:30 p. m. About 7 cc. of the stain have passed up the stem since the last record. The blades of the leaves are crisp and the petioles are bright red. March 30. Fluid quite dark; an additional 4 to 5 cc. has gone up the stem. Stem and petioles much brighter red than yesterday. April 3, 11 a. m. The entire stem and all of the petioles have become extremely bright red, the eosine water (20 cc. of it) having continued to pass up the dead stem since the last record. The leaves appear to have taken up no stain since March 29.

They are not now crisp, but feel limp like old rags. The veins are bright red, but the parenchyma is yellowish-white. The surface of the stem feels moist and stains the fingers red when rubbed.

Similiar results were obtained with a 1 per cent. solution of sodium chloride containing 1 per cent. eosine. Acidulated waters (1 per cent. citric acid and 1 per cent. hydrochloric acid) also passed up the cut stems rapidly and in large quantity, and after the stems were dead. The 1 per cent. hydrochloric acid proved much more poisonous to the plant than did the 1 per cent. citric acid. Similar experiments were made with hydrant water. In the latter, after a few days, the plants reduced their foliage to a minimum, and then lived on for many days, i. e., in case of a plant used for comparison with No. 1, until long after the latter was dead and dry.

To sum up the results of these experiments, of which the preceding are only examples, we have the following propositions:

(1). The rate of movement of the water current in cucumber stems during active transpiration is at least 10 to 12 meters an hour. (2). Absorption of water and transpiration continues in dead stems for some time, i. e., until they have become dry. (3). Large quantities of fluid passed through the cut stems during the first few days. (4). When the cut stems were plunged into water tinged with eosine, sufficient of this stain was taken up to color all the tissues of the plant bright red, including parenchyma, sclerenchyma, collenchyma and epidermis; the first parts to show the stain being the spiral vessels.

(To be Continued.)

ON THE MISSISSIPPI VALLEY UNIONIDÆ FOUND IN THE ST. LAWRENCE AND ATLANTIC DRAINAGE AREAS.

BY CHAS. T. SIMPSON.

The entire Mississippi drainage area is peopled by a peculiar Unione Fauna.[1]

The species are exceedingly numerous. and many of them attain great size, or become very solid at maturity. A large number are characterized by strong sculpture in the form of knobes, pustules or plications, or by striking outlines, and the species in general are more richly colored externally or internally than those of any other part of the globe.

The Atlantic drainage area, including a considerable part of the St. Lawrence River system, is occupied by a very different Naiad fauna. As a rule the species are moderate in size and conform nearly to the ordinary oval or oblong-oval Unione type; they are of light structure, without sculpture or strong angularities and lobes, and are plain colored in nacre and epidermis.

The dividing line between these two Unione faunas is not directly on the Height of Land, which separates the St. Lawrence and some of the other Atlantic drainage systems from that of the Mississippi, but it is considerably to the northward and north-eastward of it.[2]

To the westward the Red River of the north, the Saskatchewan and Mackenzie are largely inhabited by Mississippi Valley Uniones, and they are found abundantly in all the great lakes, the southern peninsula of Michigan, the streams in Wisconsin, Indiana and Ohio that drain into these lakes, and well up into Eastern Canada, Lake Champlain and

[1] See paper by the writer "On the Relationships and Distribution of the North American *Unionidæ*" in Am. Naturalist, *XXVII*, p, 353.

[2] This matter will be discussed in a paper by the writer, which will soon be published in the Proc. N. S. National Museum "On the Classification and Distribution of the Naiades,"

the Hudson River, in some places mingling with the forms belonging to the Atlantic drainage area proper, in others occupying the waters exclusively.

I think we may safely take it for granted that the only way in which the Mississippi Valley *Unionidæ* could have entered these northern and north-eastern river systems was by migrating along connecting fresh water. As there is no such connection to-day between these systems the question as to how they reached their present distribution becomes an extremely interesting one.

If the theory of the Ice Age as held by most glacialists is a true one I think it will fully explain the present remarkable distribution of these extra-limital Mississippi Valley Naiades. And at the same time I believe the evidence of these fresh water mussels is strongly corroborative of the glacial theory. It is held that at the close of the Ice Age a great cap of ice of immense thickness covered North America east of the Rocky Mountains, down to about Latitude 40°. That with the coming on of warm weather it gradually melted away at its southern extremity, and that when this thawing was continued north of the height of land great lakes were formed whose southern shores were the slope of the land which raised towards the south, and whose northern borders were the slowly dissolving wall of ice. On account of the ice to the northward this water could only drain into the Mississippi system, or to the Southeastward, and several old channels are found through which it is believed that it flowed. One of these is the Red River of the North, which almost connects by means of Traverse Lake at its head with Big Stone Lake at the head of the Minnesota River. There is still a broad channel near the western end of Lake Superior which connects with the St. Croix River, and at Chicago there was no doubt an overflow from Lake Michigan into the Des Plaines River, and Lake Erie is believed to have had its outlet into the Wabash through the Maumee which nearly connects with it. The two streams are connected over a very flat country by an old channel not less than a mile and a half wide, and having an average depth of 20 feet. For 25 miles this character continues, and there is

very little fall either way. To the northeast this channel opens out into an ancient lake, and at the southwest it touches bed rock at Huntington, and then descends more rapidly.[3]

It will be noticed on the map that the St. Josephs, St. Mary's, and Auglaize Rivers, tributaries of the Maumee, flow in the direction of the Wabash, that the two former join at Fort Wayne and flow partly backward as the Maumee; the whole looking like a tree with its branches broken down, and hanging against its trunk. If the river was continued into the Wabash, and the water all flowed to the southwest it would form a natural looking system. It is quite within the bounds of probability that there were old overflows from the St. Lawrence drainage to the eastward of this through the Oswego River into the Mohawk, or by way of the Sorel into the Hudson, and possibly through eastern Lake Erie into the Alleghany system.

Now if the water from this region north of the Height of Land flowed over into the Mississippi drainage area at various places it would be almost certain that the *Unionidæ* of this system would migrate up these overflows and into the northern lakes, that in this region they would obtain a foothold and flourish, for the reason that at the time of their entrance it is quite probable that all freshwater life of this area was destroyed by the grinding and crushing of the great ice cap. It is possible that a few of the Naiades of the eastern drainage system might have survived in the St. Lawrence Valley but it is more likely that such as are now found there have since reached that region by migration from the overflows through the Mohawk and Oswego Rivers, or the Sorel. There has probably been at some time since the close of the Glacial Epoch a connection between the Hudson River and Lake Champlain, as the latter is largely peopled with Mississippi Valley *Naiades*. These forms, most likely, entered Lake Erie through the old Maumee Channel, or by some connection with the Upper Ohio system, passed into Lake Ontario, thence through the Oswego

[3] See a paper "On the Ancient Outlet of Lake Michigan," by Prof., W. M. Davis. Pop. Science Monthly, XLVI, No. 2, p. 217. Also a paper on this old system by G. K. Gilbert, in the first volume of the Ohio Geological Survey.

and Mohawk Rivers into the Hudson, and across into Lake Champlain; or they may have gone down the St. Lawrence and up the Sorel. If by a subsidence since that time Lake Champlain has been connected with the ocean, as is now believed, the Naiads of that lake no doubt retreated up the small streams flowing into it, and returned after the elevation of the land when its waters again became fresh.

I think I am not making too sweeping an assertion when I say that all the Mississippi Valley species of Naiades that have entered the St. Lawrence, or in fact any part of the Atlantic drainage areas, have become changed in some of their characters. As a rule, though not in every case, they have become smaller, and simpler in their outlines; the sculpture is less pronounced or is almost obliterated; in many cases the shells are thinner, the nacre has lost its brilliancy, and instead of the bright epidermis, often painted beautifully with rays or a wonderful pattern of rich greens, yellows, and olives we have mostly dull, livid, ashy or rusty reddish or brownish exteriors, and they are very often somewhat distorted. This is not, as I believe, in any great measure due to climate or colder water, for these same species are as vigorous and finely developed in parts of Wisconsin drained into the Mississippi, Minnesota and Dakota as in any part of their area; besides *Anodonta edentula* under the name of *A. undulata*, and *Unio* (*Margaritana*) *marginata* when found in Maryland, Virginia, and probably even south of that are so dwarfed and stunted as to be scarcely recognizable. This changing of characters has been well illustrated in a lot of *Unionidæ* recently submitted to me for examination by Prof. B. W. Everman of the U. S. Fish Commission, which was collected mostly from the Maumee basin by Dr. Philip H. Kirsch, of Columbia City, Indiana. This region lies in Lat. 41° to 41½°, the most southerly part of the St. Lawrence drainage. *Unio. luteolus* Lam., *U. subrostratus* Say, *U. circulus* Lea, *U. phaseolus* Hild., *U. multiplicatus* Lea, *U. multiradiatus* Lea, and *Anodonta grandis* Say, are so dwarfed and stunted, and changed in color as to be scarcely recognizable, while the same species from the Wabash, from which these have no doubt all been derived, are as vigorous and finely developed as any in the Mississippi Valley.

This great change in size, form and coloring has caused students to bestow many specific names on what I believe are merely northern races or varieties of common Mississippi Valley species. Thus Anthony's *Anodonta subangulata* and Lea's, *A. footiana*, *A. marryattana* and *A. benedictii* are merely dwarfed and slightly changed forms of Say's, *A. grandis*. Anthony's *A. subinflata* is probably a form of *A. corpulenta* Cooper, and *A. subcylindracea* Lea, is the northern manifestation of Lea's well known *A. ferussaciana*. Say's *Anodonta edentula* becomes in Michigan *Alasmodonta rhombica* of *Anthony*, and further east and southeast *A. undulata* of Say; Lea's *Unio circulus* of the central Mississippi area changes in Lake Erie to the dwarf *U. leibi* of the same author; his *U. canadensis* is only an altered over *U. ventricosus* of the western States, and A. Gray's *U. borealis* is a very much changed form of the common *U. luteolus*, while *U. hippopæus* Lea, of Lake Erie is, I believe, only a stunted *U. plicatus* that has almost entirely lost its plications, and has assumed a dirty, reddish or olive color.

Some of these are possibly valid species; most of them would certainly be considered so, together with a number of other northern manifestations of Mississippi Valley species were it not that so many intermediate links are found.

It sometimes happens that specimens of a given species are found in the Mississippi area, growing, no doubt, under unfavorable conditions, that so closely imitate the same species found in northern waters as to be indistinguishable from it. Thus Lea has in his collection what he called *Anodonta footiana*, a Michigan form, from Illinois, and depauperate *Unio plicatus* are sometimes found in the Mississippi area that are almost exactly like *U. hippopæus*. And on the other hand occasionally fine specimens of *Unio rectus*, *U. rubiginosus*, *Anodonta ferussaciana* and *A. grandis* are found in the St. Lawrence drainage that are perfectly normal. Yet as a rule an expert can tell at a glance whether a specimen grew in the Mississippi area or was extra-limital.

Anodonta simpsoniana Lea, is, I believe, a good species, although it is probably an altered and dwarfed *A. grandis*.

It is possible that here we have an opportunity to make some kind of an estimate as to the time required in developing species and varieties among the *Unionidæ.* It is well known that the Laramie strata of the northwest, belonging perhaps to the upper cretaceous or earlier Tertiary systems contain the remains of a large number of Unios which appear to be very closely related to existing Mississippi Valley forms, and are probably their progenitors. Some of these old fossils are so much like certain recent species that they might easily be taken for them by an expert, and nearly or quite all of them can be placed in existing groups.

Yet it is more than probable that the great variety of changes that have been produced in the Mississippi Valley forms which now inhabit the St. Lawrence drainage area have taken place since the Ice Age began to draw to a close, because it is almost certain that all fluviatile and lacustine life under the ice sheet was destroyed, and that any forms closely allied to those of the Mississippi Valley now found north of the Height of Land migrated there since. It is held by most glaciologists, I believe, that the Glacial Epoch reached down probably to within from 10,000 to 20,000 years of the present. This amount of time might probably be taken as the age of these peculiar forms of St. Lawrence Mississippi Naiades.

Unio radiatus, ochraceus, cariosus, heterodon, tappanianus, and *Margaritana undulata,* which are found in the Atlantic drainage south of the line of the ice cap, and which are all closely related to common Mississippi Valley forms are probably older, and may have been derived from some migration made from the western to the eastern drainage at a much earlier date. At any rate I believe that all the Uniones which belong properly in the Atlantic drainage system were derived at one time and another from Mississippi Valley species; that some peculiarity of environment common to this entire region has had a tendency to dwarf them, to simplify their forms and dull their colors.

EDITOR'S TABLE.

Naturalists need not feel unkindly just now towards representative Dingley of Maine, who introduced a bill for the destruction of the seal herd of Behring Sea, which has passed the lower house of Congress. From the point of view of the lover of nature this bill appears to be an atrocity, but everything does not appear on the surface. The sole object is to destroy the commercial value of the herd, so as to put a stop to the slaughter by reckless Canadian poachers. A sufficient number will be preserved to serve as a basis of a new herd, whenever the British and Canadian Governments are ready to join hands with us in the effort to preserve it. The Dingley bill is really a plan for preserving the herd and not destroying it. The fact is that our neighbors across the border have been running up a bill of small accounts against themselves, which will in the aggregate prove burdensome to them some day if continued. It is poor policy for a weak party to make itself unpleasant, especially when the stronger party is desirous of friendly relations. Canadians and Americans are really one people, and we ought to combine not only to protect the seals, but to increase theirs numbers, and develop the industry which depends on them.

Some naturalists think it is quite the proper thing to protest that it is of absolutely no importance whether they receive credit for a discovery or not, and it is more than intimated in print from various quarters from time to time, that interest in such questions is quite inconsistent with the lofty aims of science. We must confess to having become somewhat weary of this alleged elevation of sentiment, for we find human nature to be in scientific investigators not so very different from that which is common to the rest of mankind. Under the circumstances these protestations savor of cant. The naturalist like other men must live. In order to live he must be known; hence necessity forbids that he hide his light if he have any, under a bushel. And in fact the majority of naturalists do not do so. They understand the value of honest advertising. The product of a laborer should be labelled, first for his own advantage, and second for the information of others, who know his personal equation. What we want is honest goods with honest labels, and for these no protestations of pseudomodesty, or depreciation on the part of unpractical idealists, is in place.

We are pleased to notice the excellent scientific work which is being done by the Field Museum of Chicago. The management has called

to its aid a number of able scientific men, and is publishing the result of their work in suitable style. The papers of Hay on the Vertebral Column of Amia, and the skeleton of Protostega, are important contributions to knowledge. We hope soon to give an abstract of the illustrated paper of Holmes on the Yucatan ruins. It seems that the Museum is not to be merely a show place, but is to be a center of original research, worthy of the great city in which it is sitnated.

Perhaps a year ago we objected in rather caustic terms to the proposed publication by the Filson Club of Louisville, Kentucky, of the life and bibliography of Rafinesque. We are at the time under the impression that the club was a scientific body, and we were then of the opinion, as we are now, that such a society might easily find better use for its money than the publication of such a work. The fact is, however, that the object of the society is the preservation of historic records, and not of the results of scientific research. Hence the publication in question was precisely within its scope, and Prof. Call, the author, conferred a benefit on us all in writing the book. The history is a very curious one, and will interest even the non-scientific reader. Manuscripts in the possession of the U. S. National Museum show that Rafinesque had a skillful pencil, and that the figures which accompany his printed works do him injustice.

President Cleveland deserves well of his fellow countrymen for various reasons, but he deserves least, of his scientific constituency. His latest appointment, that of the U. S. Commissioner of Fish and Fisheries, was made in spite of different recommendations of the scientific men of the country, and for reasons which are to this class quite inscrutable. The new appointee was, as we are informed, retired from the navy on account of rheumatism. He has no scientific knowledge or experience of the habits of fishes or the conduct of fisheries, and would seem to be physically incapacitated from learning. Doubtless the President has told him as the old lady told her daughter who asked her if she might go in to swim; father may I the fishes save from thoughtless cruel slaughter? yes, yes my son, save every one, but don't go near the water.

RECENT LITERATURE.

Geological Survey of New Jersey.[1]—The Annual Report of the State Geologist for the year 1894 contains an account of the progress made in the study of the surface geology, by R. D. Salisbury; a report on the artesian wells in southern New Jersey, by L. Woolman, and a statement of the results of the surveys made with reference to ascertaining the forest area of the state, by C. C. Vermeule.

Mr. Salisbury makes an especial point of the influence that "stagnant ice" has had upon the deposition of the stratified drift of the valleys of the northern part of the state. In his description of Flat Brook Valley he remarks that "the form of topography characteristic of this valley, and of stagnant ice deposits in general, is the following: A broad and somewhat swampy flood plain in the axis of the valley is bordered on one or both sides by a strongly-marked kame belt a few rods in width. This kame belt is lowest near the axis of the valley. It rises in the opposite direction, and finally grades into a flat-topped terrace." These terrace differ from normal river terraces primarily in the fact that the slopes which face the axis of the valley are not erosion slopes.

Mr. Woolman's report confirms the conclusions of former observations, that the principal water-bearing horizons are found in Cretaceous strata.

The forestry report includes a paper on the forest conditions of south Jersey, by John Gifford. The interest of this paper centers in the practical suggestions it contains as to the treatment of forest lands, both for their preservation, and for pecuniary return for money and labor spent in their care. The paragraphs on Forest Influences, and Forest Economics should, in the interest of the people, be quoted in every local paper of the State.

Nine page-plates are used for illustrations, and a geological map of the valley of the Passaic—topographic sheet 6 in envelope, accompanies the Report.

Annual Report, Vol. VI, Geological Survey of Canada.[2]—This volume comprises the summary reports on the operations of the

[1] Annual Report of the State Geologist of New Jersey for the year 1894. Trenton, N. J. 1895.

[2] Annual Report (new series) Geological Survey of Canada, Vol. VI., 1892-93. Ottawa, 1895; Dr. H. R. C. Selwyn, Director.

survey for the years 1892 and 93, by the Director; reports on the Geological investigations conducted in central Ontario and southwestern Nova Scotia by F. D. Addens and L. W. Bailey respectively; a contribution to the knowledge of the minerals of Canada, as shown by chemical analyses, by G. C. Hoffman; and a report on mineral statistics and mines, by E. D. Ingall and H. P. H. Brumell.

The Director's report includes much valuable information concerning the hitherto practically unexplored regions of the Labrador peninsula, and the western coast of Hudson's Bay.

Sketch maps of southern Keewatin, and of the south-western part of Nova Scotia accompany the reports on those regions, and a number of statistical diagrams show the progress of the mining indutries.

Elementary Physical Geography.[3]—A new text book of physical geography has been long needed, so that this work of Mr. Tarr's is well timed. The author divides the subject into three parts the Air, the Ocean, the Land, giving the physiographic side more prominence than is customary in works of this kind. The language is clear, the illustration apt, and the information up to date. Each chapter is supplemented by a list of reference books and an appendix contains descriptions of meteorological instruments, apparatus and methods of use, suggestions to teachers, and questions upon the text.

The text is usually well illustrated with diagrams and reproductions of photographs many of them new, while the addition of 29 plates and charts completes a most attractive volume. We can recommend it for use as the best text book for colleges before the public.

Guide Zoologique.[4]—A reference book, published for use during the meeting of the International Congress of Zoology at Leyden in 1895. Brief accounts are given of the zoological courses offered in the various schools of Holland, also of the Zoological institutions, gardens, and societies. The fauna of the country is summarized by specialists, the history of the domestic animals revewed, and a short account of the fishing industry closes the zoological part of the volume. The final chapter is devoted to the climate of Holland.

The many maps and plates which are distributed through the book, its convenient size, and the clear, concise language of the text, combine to make an admirable guide book,

[3] Elementary Physical Geography. By R. S. Tarr. New York and London, 1895. Macmillan & Co.

[4] Guide Zoologique. Communications diverses sur les Pays Bas. Leyde, Septembre, 1895.

Marshall and Hurst's Practical Zoology.[5]—The fourth edition of this work being called for, the work of revising and editing it has devolved upon Mr. Hurst, to bring the work up to date numerous changes have been made, the most important of which, perhaps, are in the chapter on Amphioxus.

The work as originally written was intended to give the junior students of Owens College, Manchester, England, a practical acquaintance with animal morphology, and the present revised edition will be found a useful laboratory text book for any one who wishes to acquire an insight into the leading facts of Animal structure, and a technical knowledge of the principal methods of research.

The illustrations are intentionally few, as it is expected that the student will make drawings from his own dissections. These are, however, of excellent quality.

Works of this class are of utility in the laboratory, but they do not take the place of general text books as guides to the larger problems of zoology.

Elementary Lessons in Zoology.[6]—In the hands of a competent teacher this book will be of value in giving a student a fair start in the study of zoology. It is in reality a Laboratory Manual. Four simple types of animal structure are given to familiarize the student with the meaning of the terms, *cell, protoplasm, tissue, differentiation sexuality,* etc. Considerable attention is given to insects; then follow in turn common forms of Crustaceans, Worms, Molluscs and Vertebrates. The study of the animal alive, and in its biological relation to its environment, is made a prominent feature. To this end methods of observation are given with suggestions as to the facts to be ascertained. In this way the student acquires a practical knowledge of the life histories of the animals studied.

An appendix contains directions for the preparation of material for study.

The illustrations are intended as guides to identification, and in a very general way, they answer the purpose.

Chats about British Birds.[7]—The depiction of bird life in this volume is quite a vivid and interesting as was that of insect life, by

[5] A Junior Course in Practical Zoology. By A. Milnes Marshall and C. Herbert Hurst. Fourth Edition revised by Mr. Hurst. New York, 1895. G. P. Putnam's Sons.

[6] Elementary Lessons in Zoology. By James G. Needham. New York, 1895. American Book Co.

[7] Chats about British Birds. By J. W. Tuft, London, Geo. Gill & Sons.

the same author, in *Rambles in Alpine Valleys.* Members of thirty three families are described in an easy, gossipy fashion, with special reference to their food and nesting-habits. No opportunity is lost for pointing out that in general, birds are the farmers best agents for protecting crops from insects and worms. The fruit eating proclivities of the Thrush and the Black bird in the late summer are excused for the wholesale destruction in early spring of insects, worms, slugs and snails.

The book is intended to interest young people in the study of Ornithology, but from the facts set forth, it may also be of use in creating among farmers a better appreciation of the service rendered them by birds, and lead them to see the necessity of organized protection for the feathered race.

Check List of North American Birds.[8]—The American Ornithologist's Union have issued a second edition of the Check-list published in 1885. The new addition includes the numerous additions and nomenclature changes made in the several supplements to the Check List since the publication of the original edition, together with a revision of the "habitats" of the species and subspecies, but omitting the Code of Nomenclature.

Species whose status as North American birds is doubtful are listed separately under the heading "Hypothetical," and the fossil birds are likewise separately classified.

As an authoritative nomenclator this book has much value, but it could be rendered more authoritative if the A. O. U. would insist on correct orthography in all cases where this is ascertainable. In several instances the list adheres to obvious misspelling and typographical errors; such as hasitata for hæsitata; cincinatus for cincinnatus; Leptatila for Leptoptila; Ammodramus for Ammodromus, etc.; Greek spellings instead of Latin are retained wherever the original authors used them, and some bad examples of the *vox hybrida* are perpetuated.

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[8] The A. O. U. List of North American Birds. Second Edition. New York, 1895.

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General Notes.

PETROGRAPHY.[1]

Ancient Volcanics in Michigan.—In an area in Michigan covered by Townships 42 to 47 N. and Ranges 30 to 34 West, is a succession of granites and gneisses overlain by a thickness of some 3000 feet of volcanic rocks, embracing acid and basic flows and tuffs. Among the basic rocks Clements[2] finds porphyrites and melaphyres, and among the acid ones quartz-porphyries and devitrified rhyolites. The melaphyres and porphyries are described under the names apo-basalts and apo-andesites, because they are altered forms of basalts and andesites. Some of the andesites are amygdaloidal, and nearly all show the effects of pressure. Andesitic and basaltic tuffs are both present. They exhibit no special peculiarities. The quartz porphyries among the acid flows are notable for the existence in them of corroded phenocrysts of quartz in which there has been developed a well marked rhombohedral cleavage. The groundmass of these rocks is sometimes micro-granitic and at other times is micro-poicilitic. The latter structure is peculiar in that it is produced by a reticulating net work of uniformly oriented quartz, between the meshes of which are irregularly shaped areas of orthoclase. The other acid lavas and the acid tuffs are similar to corresponding rocks elsewhere. The series is interesting as affording another illustration of a typical volcanic series of Pre-Cambrian age. It is one of the oldest accumulations of volcanic debris and lavas thus far described.

Gneisses of Essex Co., N. Y.—In a recent bulletin on the geology of Moriah and Westport Townships, Essex Co., N. Y., Kemp[3] gives a general account of the petrography of the gneisses, limestones, black schists, gabbros, anorthosites and dyke rocks of these regions. Most of these rocks have already been described in more detail in other papers. The gneisses are of several varieties. The most common is a member of the basement complex underlying the other rocks of the district. It is a biotite gneiss composed of quartz, micro-perthite, orthoclase, plagioclase and brown biotite, all of which minerals exhibit evidences of dynamic metamorphism. Near iron ore bodies the gneiss becomes more basic, abundant green or black hornblende, green

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Journal of Geology, Vol. III, p. 801.

[3] Bull. N. Y. State Mus., Vol. 3, No. 14, 1895, p. 325.

augite and a large quantity of plagioclase taking the places of the usual gneissic constituents.

Volcanic Rocks in Maine.—In a preliminary notice on the rocks of the Flox Islands, Maine, G. O. Smith[4] gives a brief account of the association of lavas and breccias on North Haven and Vinal Haven Islands. On North Haven the series consists of beds of porphyrites and of coarse volcanic breccias and conglomerates, layers of tuffs and sheets of quartz-porphyry. The porphyrites are sometimes olivinitic. The conglomerates and breccias are composed of fragments of the porphyrites cemented by a porphyritic matrix. The quartz-porphyry possesses no unusual features. On Vinal Haven the rocks are predominantly acid, comprising many banded and spherulitic felsites that were originally glassy rocks. The spherulites are felsitic or fibrous and are certainly original structures, since transitions from the felsitic into brecciated rocks may be traced, in the latter of which occur spherulites that were formed prior to the brecciation. The acid layers of the series are younger than the basic beds.

Spotted Quartzites, S. Dakota.—The Sioux quartzites in Minnehaha Co., S. Dakota, grade upward into variously colored quartz slates that are composed of quartz grains, iron oxides and mica in an argillaceous matrix that has crystallized in part as sericite, kaolin and chlorite. Many of the slates are marked by spots that are lighter than the body of the rocks. These spots are essentially of the same composition as the groundmass in which they lie, except that they contain less iron oxide. Their lighter color is due to bleaching out of the iron salt through the acid, probably of decomposing organic matter.[5]

The Gneisses and 'Leopard Rock' of Ontario.—The gneisses interstratified with the limestones in the Grenville series, north of Ottawa, Canada, vary much in character.[6] The predominant variety is a granitoid aggregate of reddish orthoclase and grayish-white quartz, a little or no mica, and sometimes garnets. Its bedding is very obscure. When the mica is abundant in the rock foliation is distinct. One variety of the rock is called by Gordon syenite-gneiss. It includes the 'leopard rock' of the Canadian geologists. The rock occurs as dykes cutting quartzites and pyroxenites. All the phases of the gneisses show the effects of pressure. The 'leopard rock' consists of

[4] Johns Hopkins Univ. Circulars, No. 121, p. 12.

[5] Beyer: Ib., No. 121, p. 10.

[6] Bull. Geol. Soc. Amer., Vol. 7, p. 95.

ellipsoidal or ovoid masses of feldspar and a little quartz, separated from each other by narrow anastomosing partitions of green interstitial substance composed of pyroxene and feldspar. When the ellipsoids are flattened by foliation the rock becomes a streaked gneiss. Under the microscope, in sections of the coarse grained gneisses, large crystals of pyroxene, microcline and quartz are seen to be imbedded in a fine grained aggregate of microcline and quartz. In the ellipsoidal varieties the ellipsoids are composed mainly of microcline grains and the interstitial mass is a fine grained mosaic of feldspar, quartz and augite. In the streaked gneiss the augite is partially changed to green hornblende, while crystalloids of idiomorphic hornblende indicates that some of this component is an original crystallization. The rocks are evidently sheared pyroxene-syenites. The author discusses the use of the term 'gneiss' and suggests that the term 'gneissoid' be restricted to the description of foliated eruptive rocks whose structure is due to magma motions, that 'gneiss' be used as a suffix to the name of any rock that has assumed the typical gneissic structure since its original consolidation, as diorite gneiss, etc., and that the ending 'ic' be used with reference to the mineralogic composition of a foliated rock whose origin is unknown—a dioritic gneiss, in this sense indicates a folaited rock whose present composition is that of a diorite.

Petrographical Notes.—In thin sections of sandstone inclusions that have been melted by basalts, Rinne[7] finds the remains of quartz grains surrounded by rims of monoclinic augite, cordierite, spinel, etc. In some of the glasses formed by the melting of the sandstone are trichites and crystallites of orthorhombic pyroxene. While this substance is found abundantly as a contact mineral in the sandstones enclosed in the basalts of Sababurg, the Blauen Kuppe and Steinberg, the author nevertheless regards it as a comparatively rare product of the contact action between these two rocks.

Bauer[8] declares that the rubies, sapphires, spinels and other gem minerals from northern Burma occur in a metamorphosed limestone on its contact with an eruptive rock whose nature is not known.

Penfield[9] obtains a heavy solution for the separation of mineral powders whose densities range between 4.6 and 4.94 by melting together silver and thallium nitrates in different proportions. The molten mass

[7] Neues Jahrb. f. Min., etc., 1895, II, p. 229.

[8] Sitzb. d. Ges. z. Beförd der gesamnet Naturw. Marburg, 1896, No. 1.

[9] Amer. Journ. Sci., Dec., 1895, p. 446.

attacks sulphides, but otherwise is of much value in separating mixtures of heavy minerals.

La Touche[10] describes an apparatus to be used in connection with diffusive columns of methylene iodide for the purpose of determining the density of minute fragments of minerals.

GEOLOGY AND PALEONTOLOGY.

Geology of the French Congo.—The Congo region belonging to France is greater in extent than the parent country. Through the enterprise of different explorers and the researches of scientific men, notably geologists and paleontologists, much information concerning this great tract of country has been acquired since 1871. M. Barrat has systematized the facts on record and publishes an interesting paper under the title "Geology of the French Congo," in which he embodies also the results of his own explorations in the valley of the Ogoove.

The observations of M. Pouel, made during a stay of many years in the region under discussion, confirm the conclusions of M. Barrat as to the great stratigraphic uniformity of the Congo. Furthermore, the formation of the basin after the uplift of the African plateau is explained by the progressive draining of a series of reservoirs, more or less depressed, placed at different altitudes and discharging from one to another toward the ocean. The limits of these reservoirs are in relation to the ancient rock ridges now hidden beneath the sandstone but occasionally laid bare by erosion.

Around the border of the basin, the formations plainly demonstrate that they were elevated as early as the carboniferous epoch, and although greatly leveled since, still show the primary reliefs. One of the most interesting is the formation of Adamaona; its substratum is granitic and metamorphic like the Crystal Mountains and the Mouamba Mountains and the region of Katanga, and there are also rocks which are similar to the Devonian of the Lower Congo. The whole formation has been intersected and to a great extent covered by erupted material, probably Cenozoic, and by outflows from volcanoes of undoubtedly much more recent age.

The structure of the plateaus of Adamaona is somewhat analogous to that of the central plateau of France. (Extr. Ann. des Mines liv. d'Aril, 1895.)

[10] Nature, Jan., 1896, p. 198.

Geology of the Nile Valley.—In a paper on the geology of the Nile Valley, Prof. E. Hull calls attention to the two great periods of erosion in this region, the first during the Miocene period, after the elevation of the Libyan region at the close of Eocene times, and the second during a "pluvial" period extending from late Pliocene times into and including the Plistocene. In the second part of the paper the terraces of the Nile Valley are described and full details given of the characters of a second terrace, at a height varying from 50 to 100 feet above the lower one, which is flooded at the present day. This second terrace is traceable at intervals for a distance of between 600 and 700 miles above Cairo. Two old river channels are also described, one at Kom Ombo and the other at Assuan itself. The author discusses the mode of origin of the second terrace and the old river valleys, and believes them to be due to the former greater volume of the river and not to the subsequent erosion of the valley. He gives further evidence of the existence of meteorological conditions sufficient to give rise to a "plnvial" period, and points out that other authors have also considered that the volume of the Nile was greater in former times. (Nature, March, 1896.)

The Antarctic Continent.—Mr. C. Hedley has published the data to date concerning the forms of life held as common stock by the converging land masses of the southern continent, together with the conclusions reached by several naturalists as to a former antarctic land area and the continuity of the southern land masses. The author states that the evidence collected tends to show Antarctica as an unstable area, at one time dissolving into an archipelago, at another resolving itself into a continent. From the distribution of the pond snail Gundlachia, he argues a narrow land connection during Mesozoic time between Tasmania and Terra del Fuego across the south pole, and that New Zealand at that time reached sufficiently near to this Antarctic land to receive by flight or drift many plants and animals (Proceeds. Roy. Soc. N. S. Wales, 1895).

Two Epochs in Vegetable Paleontology.—A late number of Science contains a tribute by Lester Ward to the memory of two eminent paleontologists, Marquis de Saporta and Professor William C. Williamson. In the author's judgment, the most important contribution of the former to science is the conclusion that the most important subdivisions of the geological scale must be drawn at different points for plant development from those at which they are commonly drawn for animal development. For example, the Mesophytic age properly

ends with the Jurassic instead of with the Cretaceous, while the tertiary for fossil plants closes with the Miocene instead of with the Pliocene.

Of the many important discoveries made by Williamson, the most valuable is the demonstration of the existence of exogenous structure in the Carboniferous Pteridophytes. (Science, Vol. II, 1895.)

The Appalachian Folds.—The faults and folds in Pennsylvania Anthracite beds are most admirably shown by Dr. Benjamin Smith Lyman in a paper illustrated by thirty-three page plates containing 177 sections. These sections were prepared by the author from the valuable cross-section sheets of the State Geological Survey, and are accompanied by a key map showing where the sections are made. From a comparison of these cross sections Dr. Lyman draws the following conclusions:

"Steep northerly dips in the Pennsylvania anthracite region are much less prevalent than was formerly supposed; nearly half the basins and saddles are about symmetrical; nearly three-fourths of the subordinate ones are so in the Western Middle field; less than quarter of the main ones are so in the Southern field. Again, the subordinate folds throughout the region are confined to subordinate groups of beds of inferior firmness, and are not parellel to the main folds, but probably at uniform profile distances from the main axes, so as to descend the flanks of a sinking anticlinal. Further, that the faults are most invariably longitudinal or reversed faults, occasioned by the overtraining of subordinate folds, and corresponding in three-fourths of the cases to an overturned southerly dip, with the upthrow to the south; such broken subordinate folds, whether dipping southerly or northerly, ride in equal number on the northerly dipping and southerly dipping sides of the main folds; the stratigraphic throw averages only about 62 feet, and never exceeds 160 feet; the displacement averages 72 feet, and never exceeds 240 feet." (Trans. Amer. Institute Mining Engineers, 1895.)

The Ancestry of the Testudinata.—In the NATURALIST for 1885, I advanced the hypothesis that the order of the Testudinata arose from the Paleozoic order of the Theromora. In the latter I included at that time the forms I afterwards distinguished as an order under the name of Cotylosauria. In 1892 (Transac. Amer. Philosoph. Society, p. 24) I specified that the Testudinata must have been derived from this latter order. It is now possible to bring positive evidence that this view is correct, since the anticipation so expressed is

now verifiable. Parts of the skeletons of a new form of Cotylosauria from the Permian bed has come into my hands, which represents a new family of the order, and one which may well have been ancestral to the Testudinata. (See NATURALIST, 1896, April for a description of the order). The family may be defined as follows:

OTOCŒLIDÆ fam. nov. Cranial roof excavated laterally behind, forming a large meatus auditorius. Teeth present, in a single row, not transversely expanded. Ribs immediately overlaid by parallel transverse dermoössifications, which form a carapace.

To this family I refer two new genera, viz., Otocœlus and Conodectes, which differ as follows:

Suspensorium directed anteriorly, except at free extremity; nostrils lateral; *Otocœlus.*

Suspensorium directed posteriorly; nostrils vertical; *Conodectes.*

OTOCŒLUS, has the following characters: Intercentra present. Teeth subconical. Mandible not projecting beyond quadrate. Clavicle expanded at both extremities, overlapping the episternum. Scapula with a proscapular lamina. Ribs transversely expanded, not united by suture with each other, alternating with the dermal bands. Limbs well developed.

The type species of the genus Otocœlus is the

OTOCŒLUS TESTUDINEUS sp. nov. The skull is short wide and flat, and the orbits are large and are situated near the auricular excavations. Surface roughly sculptured with small pits and ridges. Malar and mandibular bones shallow. Teeth small, compressed conic smooth, and without serrations. Scapular arch without sculpture of the inferior surface. Humerus with widely expanded head and narrow shaft. Bands of carapace of moderate transverse extent, and roughly sculptured with pits and tubercles. Width between auditory meatuses 74 mm.; do. between orbits 32 mm.; do. between auditory sinus and orbit 16 mm.; transverse diameter of orbit 30 mm.; depth of mandibular ramus below middle of orbit, 28 mm.; width of carapace 80 mm.; length of clavicle 80 mm.; transverse width of head of humerus 35 mm.; length of femur 67 mm.; length of vertebral centrum 10 mm.; width of do. 19 mm.; width anterior rib distally 11 mm.

A second species is the *O. mimeticus* Cope. But one species of Conodectes is known, the *C. favosus* Cope.

This form is of especial interest since it constitutes, with the genus Dissorhophus which I described in the NATURALIST for 1895, p. 998, a remarkable example of homoplassy. It is doubtful whether the carapaces of the two forms could be distinguished externally, but Dissorhophus is a Stegocephalian Batrachian with rhachitomous vertebræ,

while the present form is a Cotylosaurian reptile. Although so similar superficially, the carapaces of the two differ as follows. In Dissorhophus transverse expansions of the neural spines of the vertebræ support the transverse dermal bands below, and the ribs are free, and only reach the border of the carapace by their extremities. In Otocœlus the neural spines are not expanded, and the dermal bands rest immediately on the ribs.—E. D. Cope.

The Extent of the Triassic Ocean.—In a short note contributed to the Paris Academy of Sciences on December 30, Prof. Iness calls attention to the striking geographical results of the researches of his Vienna colleagues on the marine Triassic fauna. While to English geologists the Trias is the typical example of an unfossiliferous land deposit, the work of Mojsisovics on the contemporaneous deposits of the Alpine region has been the starting point for a series of discoveries in many parts of the world. A rich marine Triassic fauna is now known, extending from Spain to Japan and California, and from Spitzbergen to New Zealand. Yet among the thousands of these fossils gathered together in Vienna from all parts, there is not a single marine fossil from the regions bordering the Atlantic or Indian Oceans. The conclusion is obvious, thas the regions of these modern oceans were not covered by sea in Triassic times. On the other hand, all the districts bordering the Pacific and Mediterranean yield the marine forms, as does a great stretch of land extending from the Mediterranean to the Pacific through Central Asia, and another extending from the Pacific through Eastern Siberia to the Arctic Ocean. Thus the Pacific Ocean was the main ocean in Triassic times, and stretched out two arms across the continental region—the one called the Tethyan ocean, of which the Mediterranean is the last remnant, the other, the Arctic branch. This distribution of the Triassic seas strikingly agrees with that of the structural features of modern coast lines indicated by Neumayr: the oceans bordered by lands with marine Trias are the oceans of the *Pacific type*, of which the coasts are determined by the convex margins of earth folds; while the oceans of *Atlantic type*, of which the margins cut across the mountain folds, are those around which the only fresh water Triassic strata are found. Thus is confirmed the opinion that the latter oceans are of comparatively recent origin, and have been produced by a process of wholesale depression, which has cut off the three great triangular upstanding masses (or *horsts*) of Greenland, Africa and India, which form so striking a feature on the surface of our planet. (Nature, Jan., 1896.)

The Ancient Beaches of Erie and Ontario.—In his correlation of the moraines of western New York with the Raised Beaches (Crittenden and Sheridan) of Lake Erie, Mr. Leverett makes the following statement in regard to the Lake Outlets:

"The evidence is clear that during the formation of the upper two beaches an outlet was found to the Wabash, past Fort Wayne, Indiana. At the time the third or Belmore beach was formed (and its probable continuation, the Sheridan beach) this outlet had been abandoned. It is thought that the ice sheet had retreated so far from the Huron and Michigan basins as to open a lower outlet through these basins than past Fort Wayne. It seems improbable that an eastward outlet was then open, for the district south of Lake Ontario was apparently still occupied by the ice sheet. It is evident that no outlet to the east could have existed until the ice sheet had withdrawn from the Lockport moraine sufficiently for a passage eastward along its southern margin. If my interpretations are correct, the Crittenden beach had been for a long time occupied by the lake before an eastward outlet was opened, a time sufficient, not only for the Lockport moraine, but for several other slightly older minor moraines to be formed. During that time, the lake in all probability discharged westward through the Huron and Michigan basins past Chicago. When the gates to the eastward were opened by the withdrawal of the ice sheet there was probably a brief period in which the lake discharged through the Seneca Valley into the Susquehanna. But soon the lower outlet by the Mohawk was opened and the lake fell rapidly to that level, leaving but feeble traces of beach or wave action in its intermediate stages." (Amer. Jour. Sci., Vol. L, 1895.)

Geological News.—GENERAL.—The periods of volcanic activity in New Brunswick, as stated by Wm. D. Matthews, are:

1. *Huronian.*—Southern New Brunswick and the northern watershed.

2. *Silurian and Early Devonian.*—Passamaquoddy Bay, Baie Chaleur, etc.

3. *Sub. Carboniferous.*—Borders of the central plain, Grand Lake, Blue Mountains of the Tobique.

4. *Triassic.*—Quaco, Grand Manan. (Bull. Nat. Hist. Soc., New Brunswick, No. XIII, 1895.)

PALEOZOIC.—The "shists lustrés" of Mont Jovet are shown by Dr. Gregory to be older than the Trias (1) by the occurrence of fragments of the schists in the Trias; (2) by the discordance of strike between the two series; (3) by the occurrence of masses of dolomite resting unconformably on the flanks of the shists; and (4) by the fact that the Trias has escaped the metamorphism which the schists have undergone. (Quart. Journ. Geol. Soc., 1896.)

Mr. C. H. Gordon's investigations of the St. Louis and Warsaw formations in southeastern Iowa furnishes evidence in favor of Calvin's

conclusion that dolomites are essentially offshore products. (Journ. Geol., Vol. III, 1895.)

CENOZOIC.—Certain data accumulated by H. B. Kümmel indicate the glaciation of Pocono Knob and Mts. Ararat and Surgar Loaf in Wayne County, Pennsylvania. It has been hitherto held by the Pennsylvania State Geologists that these peaks were nunataks. (Amer. Journ. Sci., 1876.)

According to Dr. Grossmann, glacial phenomena in the Færoes comprise roche moutounées, glacial striæ, glacial mounds and boulder clay. The author states that there is no doubt that the islands had a glaciation of their own, a conclusion which is inconsistent with the hypothesis of a big northern ice cap. (Geog. Journ., Vol. VII, 1896.)

M. Harlé has identified the canine tooth and phalangeal bones of a lion, two molar teeth of a reindeer, and a molar tooth of an elan (Alces) in the fragmentary specimens from the Tourasse caverns in the southwestern part of France. From the evidence of other remains, M. Regnault fixed the age of this cave as intermediate between late Paleolithic and Neolithic. The presence of lion remains at Tourasse shows that this carnivore lingered long in the Pyrenees. (L'Anthropol., 1894.)

CAVE EXPLORATIONS IN TENNESSEE.—We discovered the tapir-peccary layer in the cave breccia together with a later fauna in a layer of cave earth, associated with Indian remains, in Zirkel's Cave near Mossy Creek, east Tennessee. Prof. Cope, our informant, had previously found the former in 1869.—H. C. MERCER.

BOTANY.[1]

The Conifers of the Pacific Slope.—Every botanist who is interested in the Conifers (and what *botanist* is not?) will be pleased with the pocket edition of Mr. J. G. Lemmon's "Hand-book of West American Cone-bearers," which appeared somewhat less than a year ago. It is a duodecimo volume of about a hundred pages, and includes seventeen half-tone plates, from photographs, of the foliage, cones, and other characteristic features. The text consists of brief descriptions of the genera and species, interspersed with notes, discussions and narra-

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

tion. In a prefatory note we are informed that this is but a prodrome to a complete work which the author has in preparation, in which full scientific and popular descriptions are to be given. The little volume before us with its modest price of but one dollar should find its way into the library of every botanist, and all will look with expectation to the completion of the larger work.—CHARLES E. BESSEY.

Still another High School Botany.—It will not be the fault of the book-makers if the young people of the country are not versed in Botany, for one scarcely takes up a scientific journal nowadays without finding an announcement of some forthcoming book, or of one just issued. It is a sign of much botanical activity in the public schools, for it is very certain that the publishers are bringing these books out in response to what they regard as a sufficient demand. The last one on our table is the Elements of Botany prepared by J Y. Bergen, instructor in Biology in the English High School of Boston. It is, we are told in the preface, "for the most part an expansion of the manuscript notes which have for some years formed the basis of the botany teaching in the Boston English High School." The book is thus to a large extent a growth; and not a creation. It looks usable, and what is more it has every appearance of being a profitable book to the user. An importance feature of the work is found in its many physiological experiments and observations which are to be made by the pupil. The whole work has a strong physiological bias which will be of much value in leading the pupil to the study of the plant in action, rather than to the identification of species.

Still with all its excellence the book presents the elements of botany in a fragmentary way. After over two hundred pages given to flowering plants, we find but twenty-seven pages given to "Some Types of Flowerless Plants." The pupil will imbibe the notion from this book that the flowerless plants are of less importance than those which receive so much more attention. The book should be called the Elements of the Anatomy and Physiology of the Flowering Plants, and thus restricted it is admirable; but the author was not warranted in calling it the elements of Botany, that is of the whole science, for it certainly does not present the elements of the *science* of Botany. We are glad to note in the very much abridged Flora at the end of the book a departure from the usual sequence of families, but we regret to see that the Gymnosperms, while given their proper place below the Angiosperms, are described in accordance with the old views as to their morphology. When we describe the staminate cones of the pine as catkins of monandrous flowers, and the ovaliferous cones as catkins of

"spirally arranged carpel scales," we must be consistent, and put the Gymnosperms where Bentham and Hooker, Gray and Watson put them, as the simplest of the Apetalæ.—CHARLES E. BESSEY.

Popular Botany.—We frequently deplore the ignorance of people in general as to the main facts of botanical science, and sometimes we berate them for not taking more interest in what we find so attractive. Yet when we are asked to recommend a book to a non-botanical friend we are sorely puzzled. It is true that we may name Kerner's Natural History of Plants, which no doubt if well read would be greatly edifying, but it costs so much, and is so big a book that few can afford the time or money it demands. We know that it is regarded by many botanists as quite the thing to sneer at Grant Allen's books on plants, but we are not of these, and on the contrary have always admired his ability to state scientific facts—dry facts too—in a way which makes them readable and even entertaining. In his latest booklet—The Story of the Plants—he maintains his reputation for entertaining and at the same time instructive writing. We commend it to the non-botanical who wish to get some general notions of plants, and may we also make bold to suggest that our severely critical and truly scientific botanists run it through. It may be suggestive to them, even.

The author pleasantly tells us "How plants began to be," "How plants came to differ from one another," "How plants eat," "How plants drink," "How plants marry," "Various marriage customs," "The wind as a carrier," "How flowers club together," "What plants do for their young," "The stem and branches," "Some plant-biographies," "The past-histories of plants." That he makes slips here and there may well be granted, but not more, we venture to say, than are made by authors of some more ambitious works.—CHARLES E. BESSEY.

Notes of Botanical Papers.—Edward C. Jeffrey in the December *Annals of Botany* figures and describes polyembryony in *Erythronium americanum*, in which *four embryos* developed in each ovule by the division of the fertilized oosphere.—The freshwater Chlorophyceæ of Northern Russia, are enumerated by O. Borge, in a 40 page paper, accompanied by three plates, the latter mainly of new species or varieties.—The Adirondack Black Spruce is treated fully, both economically and scientifically by Wm. F. Fox the superintendent of state forests for New York, in the Report of the Forest Commission for 1894. This paper has been issued under its title as a separate book of eighty-two pages. It is illustrated by many half-tone and two colored plates. —A. P. Morgan continues his studies of North American Fungi in the

Journal of the Cincinnati Society of Natural History (April–July, 1895) and describes some new genera and species. Three plates accompany the paper, giving illustrations of every species.—In *El Barbareno* (Santa Barbara, California), Mrs. Ida Blochman writes pleasantly and instructively about the California wild flowers. Such papers will do much to help acquaint busy people with the plants about them. It would be well if botanists elsewhere were to imitate Mrs. Blochman.—The recent death of Julien Vesque July 25, 1895) brings to us a series of necrological papers by Dehérain, Bonnier, Duclaux, Schribaux and Bertrand, accompanied by a photogravure of the lamented investigator. Julien Vesque was born in Luxembourg, April 8, 1848, educated in the Grand Ducal Atheneum of Luxembourg, studied in Berlin (under Braun and Kny) and afterwards in Paris with Brongniart, Duchartre and Decaisne. He was early made a member of the Institut Agronomique, in which he was an active worker at the time of his death. The collected titles of his botanical papers number sixty-seven, covering twenty-two years (1873–1895).—Lewis's Leaf-Charts promise to be very useful. They consist of very accurately drawn life-size drawings of characteristic leaves of North American trees. Their moderate price (50 cents per chart, 22 x 28 inches) should warrant their being placed in many of the public schools.—Century III of C. L. Shear's New York Fungi is now in course of distribution. It will prove to be of more than usual interest containing as it does several new or recently described species. This distribution of fungi has met with unusual success, every copy of Century I having long since been taken, no doubt due to the excellence of the specimens. It should be mentioned that the author has removed to Lincoln, Nebr. where he is engaged in botanical studies.

VEGETABLE PHYSIOLOGY.[1]

Change in Structure of Plants due to Feeble Light.—The evidence that new species of plants are developed directly and rapidly out of old ones by changes in the environment is becoming more and more conclusive each year. Plants put into markedly different surroundings either perish or become rapidly modified to meet the changed demands made by the new conditions. One of the most recent and

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

interesting pieces of evidence is that brought forward by M. Gaston Bonnier in a long article,—Influence de la lumière électrique continue sur la forme et la structure des plantes,—running through four numbers (78, 79, 80 and 82) of the *Revue générale de Botanique,* Paris, 1895. In a previous study (Les plantes arctiques comparées aux mêmes espèces des Alpes et des Pyrénées, *Rev. générale,* VI, 1894, p. 505) M. Bonnier had shown that arctic plants differ noticeably from the same species growing in alpine regions, e. g., in the greater thickness and simpler structure of the leaves, and had attributed this to the feebler light of the arctic region and to the greater degree of moisture. By means of feeble electric lighting and a moist cool temperature he has now been able to produce these differences synthetically in Paris, i. e., to take alpine plants and convert them into arctic ones. He has also shown by experiments on a great many plants, details of which are given, that feeble continuous electric lighting for a period of six months causes decided histological and morphological changes in nearly all of them, except such as grow in the water. Many plates are given in connection with this paper showing morphology and histology of normally grown and continuously lighted plants and the changes in the structure of the latter are frequently so great that no one would believe the sections to have been derived from the same species. About 75 species were experimented on and these belonged to many different families. The structural changes obtained in *Helleborus niger, Fagus silvatica, Pinus austriaca, Picea excelsa,* and *Pteris tremula* are particularly striking. To illustrate, in the needles of Pinus the characteristic arms or folds of the cortical parenchyma disappear entirely and there are several other equally striking changes. In Pteris the petiolule under the influence of the continuous electric light takes on an epidermis which is clearly distinct from the subjacent cells, and the cells of which are elongated perpendicularly to the surface of the petiole and are much larger than the neighboring layers of cortical tissue while their walls are not thickened; the cell layers immediately under the epidermis (sclerenchymatic tissue) do not become thick-walled and are rich in chlorophyll; the intercellular spaces in the cortical parenchyma have wholly disappeared; and, finally, there is no endodermis, although it is well developed in the normally lighted plant. The palisade tissue was imperfectly formed in bright electric light and in many cases entirely disappeared in feeble electric light thus confirming what has been believed for some time on other grounds, namely that the development of the palisade tissue of leaves stands in direct relation to the intensity of the light. Additional experiments seemed to indicate that most of

the results obtained were due not to the kind of light but to the grade of intensity. The whole paper will repay careful perusal. The author's main conclusions are given in the following paragraphs, as nearly as possible in his own phraseology: *Modifications due to continuous electric light.* The organs completely developed in continuous light have the following characters: (1) The chlorophyll is more abundant and is more uniformly distributed in all the cells which contain it under normal lighting. Moreover, chlorophyll grains may appear even in elements which do not contain them in a normal state, in the bark clear to the endodermis, or even in the medullary rays, in the pith, sometimes even to the central cells of the pith. (2) The structure of the blade of the leaf is simplified; the palisade tissue is less distinct or disappears entirely, the epidermis has cells with thinner walls, and the cortical cells lose their special differentiations (transformation into sclerenchyma of the petioles of the fern, reduplication of the membrane of the cortical cells of the needles of the pine, etc.). (3) The structure of the stem is simplified; the bark is less clearly divided into two different zones or even has all its elements alike; the cork is tardy or but little developed, the endodermis is less well defined, or is no longer distinct from the neighboring cells; the cortical tissue, the tissue of the medullary rays, and that of the pith are composed of elements which more nearly resemble each other; the sclerification and the lignification of the pericycle or of the wood fibres is diminished or disappears entirely; the interior calibre of the vessels is often greater; the perimedullary zone and the libre are less differentiated.

It may be added that the structure in discontinuous electric light approaches more nearly the structure in normal light than that in the continuous electric light. Finally, it should be noted that this latter is intermediate between the normal structure and that in obscurity, except the greening. The simplification of structure under continuous feeble electric lighting is, therefore, to be ascribed partly to the continuity of the light and partly to its feebleness. To sum up, a sort of *green etiolation* is produced by continuous electric lighting, for the two principal characteristics of the changes obtained are the superabundance of chlorophyll and the simplification of the structure.

Somewhat similar results may be obtained by growing plants for some time in weak daylight in the middle of a room and then comparing their structure with that of the same species cultivated in the bright light of a window. Modifications of form and cell structure are still more pronounced if the same plants are grown in total darkness. Anatomical characters are sometimes used in classification and M.

Bonnier suggests that the electric light may be used to determine which of these are most constant.—ERWIN F. SMITH.

A Graft Hybrid.—At a meeting of the Biological Society in Christiana, Nov. 21, 1895, Prof. N. Wille, the well known algologist, exhibited the fruit and leaves of a so-called graft hybrid which is said to have resulted from the working of a pear upon a white thorn (*Cratægus oxyacantha* L.). This tree stands in the Hofe Torp in Borge Kirchspiel in south east Norway. According to the statement of Herr Apotheker Johns. Smith, of Fredriksstad, the tree is about twenty years old and stood for fifteen years in an unfavorable place without blossoming. It wa then set in a better place and has blossomed and borne fruit for five years. The flowers are like those of the pear tree but somewhat smaller and borne in corymbs like those of *Cratægus*. The pedicels and the fruit are smooth, but the calyx lobes are triangular and woolly hairy with the tips somewhat bent back. The small fruits (1·5 to 3 cm. long by 1·3 to 2 cm. broad) are pear-shaped but with the color of *Cratægus* fruits. The fruits are five-celled and usually with two sterile seeds in each compartment; the pericarp is somewhat firmer than the flesh of the fruit and recalls the so-called stone of the *Cratægus* fruit, but is by no means so hard. The taste of the flesh is insipid and lies between the taste of the pear and that of the white thorn. All the fruits examined by Prof. Wille contained only sterile seeds, but Herr Apotheker Smith stated to him that he once found a single perfect seed. The leaves of the tree have retained the appearance of pear leaves and do not appear to be changed, but out of the wild stem below the point of union shoots of the white thorn now and then grow out and these have the characteristic leaves of that tree. This account is taken from *Biologisches Centralblatt*, Bd. 16, No. 3, Feb. 1, 1896. It would add much to the credibility of this case if it could be learned when, by whom, and from what sort of pear tree this white thorn was grafted. A sceptical pomologist suggests that the top of this tree may possibly be the Japanese *Pirus Toringo*, or some allied species.—ERWIN F. SMITH.

Ustilaginoidea. The following note should have appeared in the March number of this journal, p. 226, after BIOLOGY OF SMUT FUNGI, in connection with which it should be read.

NOTE:—Since the above was written, Dr. Brefeld has succeeded in discovering the full life history of *Ustilaginoidea*. The sclerotia, after lying on damp sand for six months, developed an ascus fructification closely resembling *Claviceps*. Dr. Brefeld's last paper on the subject

may be found in a recent number of *Botanisches Centralblatt*, Bd. 65, No. 4, 1896. It is entitled, Der Reis-Brand und der *Setaria*-Brand, die Entwicklungsgleider neuer Mutterkornpilze.—ERWIN F. SMITH.

ZOOLOGY.

Respiration of Trilobites. Dr. C. E. Beecher comments as follows on the probable method of respiration of the trilobite genus, Triarthrus. "No traces of any special organs for this purpose have been found in this genus, and their former existence is very doubtful, especially in view of the perfection of details preserved in various parts of the animal. The delicacy of the appendages and ventral membrane of trilobites and their rarity of preservation are sufficient demonstration that these portions of the outer integument were of extreme thinness, and therefore perfectly capable of performing the function of respiration. Similar conditions occur in most of the Ostracoda and Copepoda, and also in many of the Cladocera and Cirripedia."

"The fringes on the exopodites in Triarthrus and Trinucleus are made up of narrow, oblique, lamellar elements becoming filiform at the ends. Thus they presented a large surface to the external medium, and partook of the nature of gills." (American Journal Science, April, 1896.)

A Criticism of Mr. Cook's Note on the Sclerites of Spirobolus.—I have read with some interest Mr. Cook's description[1] of certain lines found upon the rings of a specimen of *Spirobolus marginatus*, but I am unable to agree with him in the conclusions drawn from them, nor with his remarks relative to the diplopod segment in general. It seems somewhat surprising that Mr. Cook made no examination of the musculature, either of the specimen described or of any other, to determine whether the lines discovered coincided in any way with lines of muscular attachment, an examination that is necessary to give his conclusions more than a very superficial footing. Had he made the examination, it is extremely doubtful whether he would have found this necessary data, since in more or less closely related forms no lines of attachment corresponding to his lines are to be found.

[1] This journal, p. 333.

Indeed there are many facts that he either ignores or of which he is unaware that are far from lending support to his interpretation of the lines. Some of these I have pointed out elsewhere[1] when considering the subject of the diplopod segmentation. Mr. Cook seems unfortunate in thinking of the greatly overgrown dorsal plate in the diplopod ring as the segment or somite, and in drawing his comparison from the geophilids. Had he examined the conditions occuring in the pauropods and those in *Lithobius*, *Scutigera* and scolopendrids, and taken into account some of the ontogenetic facts known regarding diplopods, he doubtless would have plainly seen indications of alternate plates (not segments) having disappeared and of the remaining plates over-growing the segments behind them, so as to give rise to the anomalous double segments. There would then have been no reason for bringing forward the most decidedly unprogressive supposition, namely, that the double or apparently double condition of the diplopod segment is a condition *sui generis* unexplainable upon general morphological principles.

With reference to his supposition that alternate leg pairs have disappeared even in the geophilids, the case that he has in mind in mentioning the Chilopoda, I must say there is no evidence whatever. To adduce the geophilid condition as evidence is to adduce the thing to be explained. Therefore, I at least am not able to agree with him in saying that this view is no more fantastic than the old fusion idea of Newport, since the latter has some real ground and many favorable appearances in its support, even though it be incorrect.

—F. C. KENYON.

The Sight of Insects.—M. Felix Plateau has been conducting a series of experiments to settle the question as to whether an insect in flight will go through a net the size of whose meshes would offer no obstruction to the passage of the insect. The question has a bearing upon the difference of vision of Insects and Vertebrates. Mr. Plateau's recent experiments would seem to confirm the statement made by him in 1889 that the vision of insects is obscure as to form, and is adapted more to the perception of movements.

The data upon which the paper is based were acquired by means of ingeniously contrived nettings of various shapes, with meshes 26 to 27 millimeters and 1 to 2 centimeters in size. These nets were placed over attractive lures, such as flowers that insects frequent and in other cases decaying animal matter. The results of the author's observations are given in the following conclusions:

[1] The morphology and classification of the Pauropoda, Tufts College Studies No. 4.

"1. A net extended does not arrest the flight of insects in every case."

"2. During flight the insects act as if they did not see the meshes of the net."

"3. A direct passage by flying is always rare. In the great majority of cases the insect hurls itself upon the net where it rests on one of the threads, and then passes through as any other animal would go through an opening which it discovers."

"4. The only explanation possible for these facts rests on the defective vision due to the compound eyes of Insects. The threads of the net produce in the insect an illusion of a continous surface, just as the cross-hatchings of an engraving do for a human eye. The Arthropod believes itself to be confronted by an obstacle, more or less translucent, in which it can perceive no openings." (Bull. Acad. Roy. Sciences Bruxelles, Nos. 9-10. 1895.)

Dr. Baur on my Drawings of the Skull of Conolophus subcristatus Gray.—In the No. of the Naturalist for April (last p. 238), Dr. Baur criticises Steindachner's drawings of the skull of the above species and my copies of them published in the Naturalist for February, p. 149. He says of the former: "These drawings have not been made to show the detailed relations of the different elements of the skull. Especially the regions copied by Cope are drawn quite insufficiently. The sutures between the different elements can not be made out." To this I have to remark that the sutures between the quadrate and adjacent bones are distinctly drawn, and can be made out perfectly well by any one familiar with the subject, but some of the others are less distinct. Dr. Baur then goes on to say that "Prof. Cope's drawing are not exact tracings from Steindachner for he has drawn sutures which do not exist at all in Steindachner's figure. There is no such suture between the postorbital. Pob, and his supertemporal, St., in the actual specimen, nor in Steindachner's drawing. * * In Prof. Cope's figure the outer and upper portion of the distal end of the paroccipital process separates the parietal process from the prosquamosal (supratemporal Cope.) This is not the case; the parietal process is always united with the prosquamosal. * The prosquamosal (supratemporal Cope) is also drawn quite incorrect; besides, its true relations cannot be made out at all from Steindachner's figures * *"

It will be noticed that in the above criticism nothing is said about the articulation of the quadrate with the exoccipital, which is the

question at issue between us; I alleging that the articulation exists, and Dr. Baur denying it. Dr. Steindacher's figures show conclusively that the articulation exists, as it does in nearly all other Lacertilia, and Dr. Baur has not alleged that this plate is wrong in this particular, or that my tracing of it is not an exact copy. On comparing my tracing with the original again, I find that it is an exact copy, and that if any errors exist they are altogether irrelevant to the question at issue. The separation of the parietal process from the superatemporal is shown in Steindachner's plate, but it may be erroneously, as Baur alleges. The suture separating the postorbital from the supratemporal in my drawing may also be an error, but it represents a feature of Steindachner's drawing, which he did not perhaps intend for a suture, although it looks like it. These two points are obscure to the eye without close examination, and it is probable that Baur is right as to their condition in nature. They however do not discredit the accuracy of the conspicous features of the articulations of the elements with the quadrate, which I find to agree with other Iguanidæ.

Dr. Baur's assumption as to what I "really believe," is not quite correct, as can be easily seen by reading my previous articles. What I have endeavored to show is that until the character of the paroccipital (squamosal Baur) of the Pythonomorpha is explained, I hold that the determination of that element as squamosal as is made by Baur, is premature. I am agnostic, and am open to conviction, but Dr. Baur has not yet convinced me.—E. D. Cope.

Zoological News. The Tokio Zoological Magazine, for 1895, Vol. VII contains an account by R. Mitsukuri of a Japanese species of Hariotta, for which he proposes the name *H. pacifica.* The type species of this remarkable chimaeroid genus is now in the U. S. Natl. Mus. It was found in deep water off the coast of Virginia and described by Goode and Bean under the name *H. raleighana.* See Naturalist 1895 p. 375 Plate XIX. The Pacific form agrees with the Atlantic one in general appearance, especially in the elongate muzzle and feeble claspers, but differs in five essential points which are enumerated by the author. The occurrence of this interesting genus in both the Atlantic and Pacific Oceans is an interesting fact.

Recent explorations in the Gulf of California along the coast of Sinaloa have resulted in a collection of fishes, which while yielding 232 species, by no means exhausts the richness of that locality. The collection was sent to Prof. Jordan for identification. Thirty new species were found among the specimens, all of which are described

and figured in the proceedings of the Calif. Acad. Sci. Vol. V. 1895.

Among the new fishes described during the past year is *Razania makua* from the Hawaiian Islands. The species is very rare, only two specimens being known. It is a deep-sea fish by habit, and is especially remarkable for its rapidity in swimming. A colored plate accompanies the description given by Mr. O. P. Jenkins in the Proceeds. Calif. Acad. Sci. Vol. V., 1895.

Two new genera and species of fishes, belonging to the family Percophidæ, are reported from Australia, by J. D. Ogilby. They are described by him under the names *Centropercis nudivittis* and *Tropidostethus rothophilus*. The latter are surf-fishes, never descending to the bottom, but swimming a few inches beneath the surface of the water. (Proceeds. Linn. Soc. N. S. W. (2) Vol. X. Pt. 2, 1895.)

In an examination of 52 specimens of *Vipera berus* from Denmark, Mr. Boulenger finds a wide range of individual variation. The differences observed are in the shape of the snout, the scaling of the head, body and tail, size and coloration. The observations as to color confirm those previously made by Geithe in Germany. (Zoologist, 1895.)

The same author in a recent classification of the American Box Tortoises in the British Museum, adopts Baur's definitions of species and distinguishes six of which he gives a synopsis. He holds to the generic name of Cistudo although it has been shown that Terrapene has priority. Ann. Mag. Nat. Hist. 1895.)

ENTOMOLOGY.[1]

A New Diplopod Fauna in Liberia.—From the west coast of Africa large numbers of Diplopoda are already known, and yet very little of the vast extent of territory has been thoroughly searched for members of this group. In connection with an attempted exploration of Liberia under the auspicies of the New York State Colonization Society, there has been an opportunity for careful collecting in the western part of that country, some of the results of which are here offered. The majority of Liberian Diplopoda belong to the suborder Polydesmoidea. The only other families represented are the Polyxenidæ, Stemmatoiulidæ, Spirostreptidæ and Spirobolidæ, and these offer no very remarkable novelty in structure or form. This is in strong contrast to the great number and variety of Polydesmoidea; indeed it

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

has proved necessary to establish genera and families in attempting to properly recognize their structural novelty and diversity. Some of these new groups have already received names,[1] but their characters have been only formally indicated.

Family AMMODESMIDÆ.

Two minute Glomeroid genera were discovered, one of which, Ammodesmus, is the smallest member of the suberder, if not of the entire class. The only species, *Ammodesmus granum*, is less than two millimetres long, and about half a millimetre broad. A single specimen was secured while collecting minute Oniscidæ, but diligent and repeated search failed to find another. It did result, however, in three specimens of a very distinct, though evidently allied, genus which it is proposed to name Cenchrodesmus. Both genera have the habit of coiling into a sphere. The second segment is enormously enlarged so as to completely conceal the head and first segment when viewed from the side, as well as to cover the space left between the decurved carinæ of the other segments when the creatures are coiled. Ammodesmus has the dorsum roughened by a transverse row of large papilliform tubercles rising from the posterior part of each segment, while *Cenchrodesmus volutus* has the segments nearly smooth. The surface of Ammodesmus is rough and dusted with earth. When disturbed it coils up and lies motionless, and is then perfectly concealed, having exactly the appearance of a grain of sand. My specimen would certainly not have been seen had it not been crawling. Cenchrodesmus is pinkish in life and mottled with pale horn-color in alcohol. Both genera live on the ground under decaying wood or leaves.

Family CAMPODESMIDÆ.

This also contains two genera similar in size and general shape, yet evidently distinct, in that Campodesmus has the segments ornamented with two conspicuous clusters of coarse tubercles, while *Tropidesmus jugosus* has two transverse rows of short longitudinal carinæ, a form of sculpture previously quite unknown in Polydesmoidea. The carinæ are depressed in both genera, and the dorsal surface is very rough with fine granules and tubercles. Pores are visible on the fifth and seventh segments, but I have been unable to find them on the others. Both forms are denizens of the deepest forests, where the light is so deficient that they are sure to be overlooked unless specially sought for. They are very sluggish in their movements, and are seldom found crawling. When disturbed they coil up into a spiral, and also assume that posi-

[1] Proc. U. S. Nat. Museum and Annals N. Y. Acad. Science, 1895.

tion in alcohol. The first segment is not enlarged to conceal the head, nor are the anterior segments larger than the others. The general appearance is strikingly different from that of other Diplopoda, the resemblance being rather to certain lepidopterous larvæ.

Familly COMODESMIDÆ, new.

The type of this family is a small, reddish-brown, subcylindrical form, very rare, and also inhabiting the denser parts of the forest. The pore-formula is unique: 5, 7, 9, 12, 15, 17, 18. The pores are located in the front part of the posterior subsegments. The dorsal surface is beset with conic piliferous granules, giving a wooly appearance. The last segment is scarcely produced beyond the anal valves, but is rounded off at apex as in many Iulidæ. The head is not concealed by the first segment, which is narrower than the second and somewhat included between the carinæ of the latter, much as in *Scytonotus granulatus* (Say). Two other allied genera, also granular and hairy, are found in similar situations in Liberia, but both have the normal pore-formula as in Polydesmus. Thelydesmus is nearly black, larger and much more abundant than Comodesmus. The generic name alludes to the fact that the females are in a large preponderance. Although about a hundred females were taken, careful and extended collecting resulted in only four males. The remaining genus is minute and very rare, cylindrical, and without carinæ. The posterior subsegments are abruptly thicker than the anterior, giving the appearance of a series of of discs laid together, whence the generic name, Discodesmus. In the Berlin Museum is another form evidently allied to Thelydesmus, but with broader carinæ and more resemblance to the Pterodesmidæ, to be noted later on. It was collected in the German Colony of Togo by Dr. K. Büttner, and may be known as *Xyodesmus planus.*

In addition to the above genera there may be referred to this family Cylindrodesmus Pocock, from Christmas Island. It is even more evidently allied to Comodesmus than the other genera mentioned. There is also in my collection a new generic type from the mountains of Java, not closely related to the other genera, but evidently belonging to the same family group.

Family PREPODESMIDÆ, new.

Under this name it is proposed to arrange West African forms hitherto referred either to Paradesmus or to Oxydesmus, from the latter of which they differ in having the apex of the last segment narrow and bituberculate. The affinities of the group seem to be with the Oxydesmidæ, although no connecting links have yet turned up. In a

large number of forms the poriferous segments are wholly or partly red or yellow, while the remainder of the body is nearly black, giving a most striking appearance. Prepodesmus includes several such forms, all with the anterior corner of the second segment greatly produced and embracing the first segment. Tylodesmus has the corner rounded and not produced. Cheirodesmus is similar to the last in general shape, but is more slender and with the male genitalia resembling in shape a gloved hand. Anisodesmus is peculiar in that the fourth segment is distinctly, though slightly, narrower than the third or fifth. The species are uniform dark red in color and the type is closely allied to *Polydesmus erythropus* Lucas. Isodesmus is evidently related, but with the fourth segment not narrowed, and remarkable in that the pores are not borne on a distinct callus as in the other genera of the group. The copulatory legs are also very peculiar being deeply divided into several laciniæ. In all these genera the dorsal surface is finely and evenly granular, though differing somewhat in other respects. The family is probably distributed along the entire West Coast, and I have seen two forms from South Africa, one of which, Lipodesmus, is in the Berlin Museum.

Family Oxydesmidæ.

The Liberian forms which belong to this family in the more limited sense[3] are all referable to the genus Oxydesmus, and belong to three species, *O. grayii* Newport, *O. medius* and *O. liber*, both new. The first is a very striking form, black in color with a narrow median stripe of bright vermillion. The other species are also black, *O. liber* with bright yellow submarginal ridges.

Family Polydesmidæ.

Of Liberian species referable to this family in its stricter sense there seem to be but two; small pinkish-red forms, similar in general appearance to some species of Brachydesmus. The dorsal elevated areas are each supplied with a clavate hair. The antennæ are strongly clavate, though rather slender, and the second pair of legs is crassate in the

[3] The African forms having the apex of the last segment broad, the femora spined, and the carinæ with a submarginal ridge, constitute the family Oxydesmidæ. There are five genera now known, two confined to the east side of the continent, three to the west. Of the east coast forms, Orodesmus includes those with strongly tuberculate segments, Mimodesmus those with the body slender and the dorsum nearly smooth. Of the west coast genera Oxydesmus has three rows of dorsal tubercles and surrounding areas; Scytodesmus has five or six rows, while Plagiodesmus resembles Oxydesmus, but has the submarginal ridges very broad and oblique, and the copulatory legs large and exposed.

male. For this genus the name Bactrodesmus is proposed; it will probably be found to be next related to the form described from Ceylon by Humbert as *Polydesmus cognatus*, but which is generically different from the European *P. complanatus*, and may be denominated Nasodesmus.

Family PTERODESMIDÆ, New.

This family is proposed for *Polydesmus gabonicus* Lucas and its African relatives, by more recent writers referred to Cryptodesmus. I have examined the type of *Cryptodesmus olfersii* in the Berlin Museum. The diversities seem to be of family importance. The African forms are very curious, the development of the lateral carinæ being carried to its greatest extent. They are very much flattened, elliptical in outline, and only four or five times as long as broad. They never coil into a spiral, even when placed in alcohol. At least five genera are found in Liberia.

All the African forms yet known to me have repugnatorial pores, and we may expect to find these in the others, notwithstanding the statements of several writers to the contrary. The location of the pores is, however, very unusual. They are far remote from the lateral margin, in the *anterior* part of the carinæ, in some cases so far ahead as to be concealed by the posterior margin of the preceeding segment. An even more remarkable condition obtains in *Pterodesmus brownellii*, the type of the genus and family. The fifth segment has no pore! The Liberian forms are further peculiar in that all are more or less pruinose. Pterodesmus is the largest of the Liberian genera. It is pure white when young, but mature individuals are usually dusted with earth which adheres to the pruinosity and gives them the advantage of protective coloration. Gypsodesmus, on the other hand, is pure white, even when mature. Lampodesmus is partly pruinose and appears to be black and white when alive, though it is brown in alcohol. It is structurally peculiar in that the sternum of the sixth segment bears two hollow processes fringed along their apical edges with long hairs. These may be of use as a protection to the copulatory legs. Compsodesmus is the broadest of the Liberian forms. When alive it is one of the most varied and brilliant of Diplopoda. A large median area of the dorsal surface of each segment is dark brown, while the space between it and the posterior margin on each side is nearly white or bright yellow. Carinæ tinted with bright orange or pink, or both. Below, except near the edges of the carinæ, the body is covered with a pure white bloom or chalky powder. Last segment nearly white. Motions very sluggish.

From the German colony of Togo comes a genus evidently allied to the last, but distinct by reason of the more slender body and narrower carinæ, which are also scarcely produced at the posterior corners. From Lampodesmus it is distinct in the absence of the process from the sternum of the sixth segment, and in the form of the copulatory legs.

A small horn-brown or yellowish creature with remarkably agile movements it is proposed to name *Choridesmus citus.* The first segment is pure white, pruinose, and abruptly different in color from the remainder of the body. The pores are large, and are located in the middle of the carinæ, remote from the margins. The quick, jerky movements remind one strongly of Polyxenus.

Family STRONGYLOSOMATIDÆ.

Of this group there are two genera in Liberia, both new, though probably not confined to the West Coast. *Scolodesmus grallator* represents the usual Strongylosoma type, with long legs and antennæ. It is dark wine-color, nearly black. *Habrodesmus lætus* is a rare species apparently confined to the darkest forests. It is exceedingly quick and agile, very graceful in form and brilliantly colored. The legs are orange and pink, and the segments have the posterior margin yellow, shading through orange and brown to black on the remainder of the segment.

In gardens at Monrovia *Orthomorpha vicaria* (Karsch) is not uncommon; it is probably not indigenous.

Family STYLODESMIDÆ.

The type of this family is a bizzare creature named *Stylodesmus horridus.* The generic name alludes to the fact that the pores are borne on long stalks placed near the lateral margins of the broad, decurved carinæ. The pore-formula is the usual one, 5, 7, 9, 10, 12, 13, 15–19. The whole dorsal surface of the animal is setose and coal-black. There is almost always an incrustation of dirt which furnishes a completely protective coloration. The head is completely concealed under the flabelliform, anteriorly lobed, first segment, and the last segment is reduced, included in, and concealed by the penultimate. The most striking feature is that each of the segments except the last bears dorsally a pair of long slender processes. Those of the anterior and posterior segments are close together and show a tendency to unite at the base. These processes are also rough and setose, and almost always so incrusted with dirt as to appear several times their actual size. If segments of Stylodesmus had been found in fossil condition they would probably have been looked upon as allied to some of the

Archipolypoda, so much greater is the general resemblance to the fossils than to previously known extant genera. Yet there are in Liberia at least three other genera which have evident affinities with Stylodesmus. In all the pores are located on special processes or tubercles, and the first segment is enlarged and scalloped in front. *Udodesmus telluster* differs from Stylodesmus in being much more slender and without the long dorsal processes. These are replaced by two longitudinal crests of two or three large tubercles. The body is very rough, setose, and incrusted with earth. *Hercodesmus aureus* is a beautiful little species, more slender than Udodesmus, and usually without a covering of earth. In Stiodesmus the dorsal ridges of tubercles are not much more prominent than the numerous large, rounded tubercles with which the whole surface is beset. The result is a creature which on first view might be supposed to have affinities with Scytonotus.

Besides these, the present family will contain four East Indian genera, Pyrgodesmus and Lophodesmus, described by Pocock, and two new ones from Java. In the Canary Islands is a beautiful and evidently allied form inhabiting the nests of ants, and called Cynedesmus, on account of the form of the first segment. The Stylodesmidæ do not coil up into a close spiral; they usually remain nearly straight, even when in alcohol. Though there is no close resemblance in form or structure between the Stylodesmidæ and Campodesmidæ, yet both are strikingly different from other Diplopoda. That two groups of such remarkable creatures should inhabit the same locality seemed at first very strange, but as the various new and equally interesting forms continued to be found it was soon apparent that we were really in the presence of a new fauna.

That the new families are not all confined to Africa is shown by recent papers, notably those of Mr. Pocock. As yet, however, the Ammodesmidæ and Campodesmidæ are known only from African representatives. Of the larger, long known forms the Oxydesmidæ and Prepodesmidæ, appear to be confined to Africa. In East Africa is another family of several genera, none of which has yet been reported from the West Coast. Indeed, speaking with regard only to families and genera, there are four very distinct diplopod faunæ in the African continent, the northern, southern, eastern and western parts having little in common. The species are, of course, even more local. I have examined the collections of the Berlin Museum and the British Museum, as well as the literature of the subject, and with the exception of *Oxydesmus grayii* and *Orthomorpha vicaria*, collected at Sierra Leone, know of no Liberian diplopod from any other part of the West Coast.

We are thus assured of an African fauna of surpassing richness, not a tithe of which has yet been revealed.—O. F. Cook.

Entomological News.—Prof. Clarence M. Weed of the New Hampshire College spent several weeks in December and January, studying the Bermuda Islands. Many species not before recorded from there were collected.

EMBRYOLOGY.[1]

The Sense Plates, the Germ of the Foot, and the Shell or Mantle Region in the Stylommatophora.[2]—To our knowledge of these subjects, Dr. Ferdinand Schmidt contributes the results of his numerous observations upon the embryos of *Succinea*, *Limax* and *Clausilia*. Concerning the sensory plates he shows that immediately behind the budlike rudiments of the future egg-bearing and the simple tactile tentacles, in *Limax* where the development is most easily followed, there arises a third pair of buds like the first two pairs in all respects except in size. From these buds arises the so called oral lobes, subtentacular lobes, or labial tentacles. They have no relation to the velum whatever, since they arise in a pre-velar region. This is completely at varience with the observations of Jayeux-Laffuie on *Onchidium* and those of Ray Lankester on *Limnæus* in which the subtentacular lobes are asserted to arise from the velum or a rudiment of the same. Should further studies upon these forms substantiate the assertion, we would then have two groups of oral lobes, one in which they arise from the velum and to be homologized with the oral lobes of the lammellibranchiata, where they undoubtedly have such an origin, and the other in which they arise from the sensory plates and are homologous with the tentacles.

In his account of the development of the foot in *Succinea* he supports the conclusion long since put forth by Lankester, namely, that the typical form of the blastopore is an elongated cleft on the ventral side of the embryo, from which arises in some cases the mouth, in others the anus, according as the cleft persists anteriorly or posteriorly. This form of a blastopore is certainly important, considering his con-

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Beitrage zur Kenntniss der Entwicklungsgeschichte der Stylomatophoren, with 9 text figs. Zool. Jahrbücher, VIII, 318.

clusions with regard to the foot. This, he says, is to be distinguished very much earlier than has hitherto been recorded and, as one would naturally expect from Patten's study of *Patella*, which he quotes, it arises from a pair of folds and not from a single one as has generally been stated for related forms. In *Succinea* these two folds appear close behind the blastopore between the region of the mouth and anus, and approaching one another fuse in the median line forming an oval area. A median furrow persists for some time as evidence of the union as in *Patella*. This last fact gives some meaning to the similarly furrowed appearance occuring in *Limnæus*, *Planorbis* and *Ancylus*.

A study of over 100 embryo showed him that this paired origin is the rule, although examples were found where the elevation was unpaired, forming then a broad disc. In one apparently pathological case the blastopore had retained its supposed primitive elongated form and the beginning of the foot had the form of a horseshoe embracing its hinder end.

His conclusion that the foot represents the fused lips of the elongated blastopore removes the possibility of the organ being some kind of secondary formation, and makes it out to be a metamorphosed very ancient structure: and if the conclusion is correct, the molluscan foot is not quite such an anomalous structure as it has hitherto seemed.

A few remarks concerning the podocyst and the so called "Nackenblase" are of interest in that they show that the latter structure is not an organ at all, and that the contracting motions that have been observed in it are due to the contractions of the podocyst which acts as an organ of circulation. For in *Succinea* where the structure in question has an enormous development and where no podocyst occurs there are no such movements to be seen. The structure is, he says, a mass of endoderm cells swollen with albumen, the embryonal liver and the outer body epithelium.

With regard to the shell gland, Schmidt substantiates, in the main, the early observations of Gegenbaur or *Clausilia* and shows Korschelt's doubt concerning them to be unfounded. A large series of *Clausilia* embryos gave ample opportunities for study, and as a result it appears that very early the shell gland arises as an invagination of the outer epithelium, and closing up, becomes completely cut off from its parent layer. Sections show it to be completely surrounded by mesoderm. The hollow vesicle thus formed becomes flattened out so that he distinguishes in it an outer and an inner layer of cells separated by a narrow space. The outer layer remains more or less un-

changed, but the cells of the inner one proliferate and begin to lay down the shell, which may be distinguished in sections as a very thin lamina. At about this time observations of embryos by reflected light show a small invagination or hole near the center of the newly formed shell, which is thus laid bare. The hole then is of secondary formation and not, as Korschelt supposes, something that has persisted from the original invagination.

It appears then that the internal formation of the shell, as it has been generally recognized in the so called naked pulmonates is not an exception to a rule but the rule itself, and that the condition obtaining in *Limax* and others differs from that in the rest of the pulmonates only in so far as a rudimentary condition is retained in the adult animal.—F. C. KENYON, Ph. D., Clark University, Worcester, Mass.

PSYCHOLOGY.

Physical and Social Heredity.—The great courtesy of the Editor of this journal in reprinting one of my paper from *Science* preliminary to replying to it encourages me to ask him for a page or two of comment on his reply. This is the more needful since the second of my papers which he criticises may not have been seen by the readers of the NATURALIST, and the third has only just appeared in *Science*, (March 20 and April 10, 1896).

The main question at issue is the relation of consciousness or intelligence to heredity; the other matter, that of the relation of consciousness to the brain, being so purely speculative that I shall merely touch upon it at the end of this note.

Prof. Cope[1] says: " there is no way short of supernatural revelation by which mental education can be accomplished other than by contact with the environment through sense-impressions, and by transmission of the results to subsequent generations. The injection of consciousness into the process does not alter the case, but adds a factor which necessitates the progressive character of evolution." Both of these sentences I fully accept, except that the word " transmission " seem to imply the Lamarkian factor, which I think the presence of consciousness renders unnecessary. Using the more neutral word " conservation " instead of " transmission," I may refer to three points on which Prof. Cope criticises my views: first, conservation of intelligent acquisitions from genera-

[1] AMER. NAT., April, 1896, p. 343.

tion to generation; second, "the progressive character of evolution;" and third, "mental education" or acquisition.

First, agreeing as we do on the fact of mental acquisition or "selection through pleasure, pain, experience, association, etc.," Prof. Cope cites my second paper (*Science*, Mar. 20th) in which I hold that consciousness makes acquisitions of new movements by such selections. He then says, if so, then I admit the Lamarkian factor. But not at all; it is just the point of my article to refute Romanes by showing that adaptation by intelligent selection makes the Lamarkian factor unnecessary. And in this way, i. e., this sort of adaptation on the part of a creature *keeps that creature alive* by supplementing his reflex and instinctive actions, so *prevents the operation of natural selection* in his case, and gives the species time to get congenital variations in the lines that have thus proved to be useful (see cases cited). Farthermore, all the resources of Social Heredity—the handing down of intelligent acquisition by maternal instruction, imitation, gregarious life, etc.—come in directly to take the place of the physical inheritance of such adaptations. This influence Prof. Cope, I am glad to see, admits; although in admitting it, he does not seem to see that he is practically throwing away the Lamarkian factor. For instead of limiting this influence to human progress, we have to extend it to all animals with gregarious and family life, to all creatures that have any ability to imitate, and finally to all animals which have consciousness sufficient to enable then to make conscious adaptations themselves: for such creatures will have children that do the same, and it is unnecessary to say that the children must inherit what their fathers did by intelligence, when they can do the same things by their own intelligence. As a matter of fact Prof. Cope is exactly the biologist to whose Lamarkism this admission is, so far as I can see, absolutely fatal; for he more than all others holds that adaptations all through the biological scale are secured by consciousness.[1] If so, then he is just the man who is obliged to extend to the utmost the possibility of the transmission also of these adaptations by intelligence, which, as I said, rules out the need of their transmission by physical heredity. At any rate he is quite incorrect in saying that "he [I] both admit and deny Lamarkism."

To this argument of mine Prof. Cope presents no objection that I see except one from analogy. He says: "I do not see how promiscuous variation and natural selection alone can result in progressive psychic evolution, more than in structural evolution, since the former is condi-

[1] And in this I think he is right: see chaps. VII and IX of my *Mental Development* (Macmillans, 2d. ed.).

tioned by the latter." As to the word "progressive," I take up that question below; but as to the analogy with structural evolution, two answers occur to me. In the first place, Prof. Cope is, as I said, the very man who holds that all structural evolution is secured by direct conscious adaptations. He says: "mind determines movements and movements have determined structure or form." If this be true how can psychic be conditioned by structural evolution? Would not rather the structural changes depend upon the psychic ability of the creature to effect adaptations? And then, second, at this point Prof. Cope assumes the Lamarkian factor in structural evolution. Later on he makes the same assumption when he says: "But since the biologists have generally repudiated Weismannism," etc. This is a curious saying; for my impression is that even on the purely biological side, the tendency is the other way. Lloyd Morgan has pretty well come over; Romanes took back before he died many of his arguments in favor of the Lamarkian factor; and here comes a paleontologist, Prof. Osborn, —if he is correctly reported in *Science*, April 3rd, p. 530—to argue against Prof. Cope on this very point with very much the same sort of argument as this which I have made.[3] And while Prof. Cope will agree with me that this sort of *argumentum ex autoritate* is not very convincing, yet he will not object to my balancing off his dictum with the following from a letter which just comes to me from another distinguished biologist, Prof. Minot: "Neo-Lamarkism seems to me an impossible theory."

But Prof. Cope goes on to say that I "both admit and deny Weismannism;" on the ground that "his [my] denial of inheritance only covers

[3] Since writing this I have heard Prof. Osborn read a paper which confirms the agreement between him and me which I supported in the text above. I reached my conclusion independently and one of my *Science* articles gives report of it as expressed in a criticism of Romanes before the New York Academy of Science on Jan. 31st, 1896. Prof. Osborn's expression "ontogenic variations" i. e. those brought out by "environment (which includes all the atmospheric, chemical, nutritive, motor, and psychical circumstances under which the animal is reared)" seems to make these adaptations after all *constitutional*. As Prof. Osborn says this will not do for all cases; and I think it will not do for instinct, where constitutional variations without the aid of *intelligence* would never suffice (as Romanes says) to keep the animal alive while correlated variations are being perfected phylogenically. But it seems to answer perfectly where intelligent or other adaptations supplement the constitutional variations—and that is just the point made in my *Science* paper. As to the way these ontogenic variations or adaptations are brought about in the individual creature, see the remarks on "organic selection" below. I am printing in the next issue of this journal (June) a full statement of the entire position.

the case of psychological sports." But I do not see the connection. If Prof. Cope means denial of the inheritance of acquired characters then I deny it equally of sports and other creatures; but I do not deny that the native "sportiveness" (!) of sports tends to be transmitted. In my view the "massiveness of front" which progress shows (and which Prof. Cope accepts) shows that in social transmission the individual is usually swamped in the general movement as the individual sport is in biological progress. As a matter of fact, however, the analogy from "sports" which Prof. Cope makes does not strictly hold. For the social sport, the genius, is *sometimes* just the controlling factor in social evolution. And this is another proof that the means of transmission of intelligent adaptations is not physical heredity alone, but that they are socially handed down. I do not see, therefore, what Prof. Cope means by saying that I "admit and deny Weismannism," for I have never discussed Weismannism at all. I believe in the Neo-Darwinian position plus some way of getting "determinate variations." And for this latter I think the way now suggested is better than the Lamarkian way. Like many of the biologists (e. g., Minot) I see no proof of Weismannism (just as I protest mildly against being sorted with Mr. Benjamin Kidd!); yet I have no competence for such purely biological speculations as those which deal in plasms!

Second, the question as to how evolution can be made "progressive." Prof. Cope thinks only by the theory of "lapsed intelligence" or "inherited habit." Admitting that the intelligence makes selections, then they must be inherited, in order that the progress of evolution may set the way the intelligence selects. But suppose we admit intelligent selection (even in the way Prof. Cope believes); still there are two influences at work to keep the direction which the intelligence selects apart from the supposed direct inheritance. There is that of social handing down, imitation, etc., or Social Heredity, which I have already pointed out; and besides there is the survival by natural selection of those creatures which have variations which intelligence can use. This puts a premium on these variations and their intelligent use in following generations. Suppose, for instance, a set of young animals some of which have variations which intelligence can use for a particular adaptation, thus keeping these individuals alive, while the others who have not these variations die off; then the next generation will not only have the same variations which intelligence can use in the same way, but will also have the intelligence to use the variations in the same way, and the result will be about *the same as if the second generation had inherited the adaptations directly*. The direc-

tion of the intelligent selection will be preserved in just the same sense. I think it is a great feature of Prof. Cope's theory that he emphasizes the intelligent direction of evolution, and especially that he does it by appealing to the intelligent adaptations of the creatures themselves; but just by so doing he destroys the need of the Lamarkian factor. Natural selection kills off all the creatures which have not the intelligence nor the variations which the intelligence can use; those are kept alive which have both the intelligence and the variations. They use their intelligence just as their fathers did, and besides get new intelligent adaptations, thus aiding progress again by intelligent selection. What more is needed for progressive evolution?[4]

Third. We come now to the third point—the method of intelligent selection—and on this point Prof. Cope does not understand my position, I think. I differ from him both in the psychology of voluntary adaptations of movement, and in the view that consciousness is a sort of force directing brain energies in one way or another (for nothing short of a force could release or direct brain energy). The principle of Dynamogenesis was cited in my article in this form: i. e., "the thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement." This principle covers two facts. First, that no movement can be thought of effectively which has not itself been performed before and left traces of some sort in memory. These traces must come up in mind when its performance is again intended. And second (and in consequence of this) that no act, whatever, can be performed by consciousness by willing movements which have never been performed before. It follows that we can not say that consciousness by selecting new adaptations beforehand can make the muscles perform them. The most that psychologists (to my knowledge) are inclined to claim is that by the attention one or other of alternative movements which have been performed before (or combinations of them) may be performed again; in other words, the selection is of old alternative movements. But this is not what Prof. Cope seems to mean; nor what his theory requires. His theory requires the acquisition of new movements, *new adaptations to environment*, by a conscious selection of certain movements which are *then carried out the first time* by the muscles.[5]

[4] I keep to "intellegent" adaptations here; but the same principle applies to *all adaptations made in ontogenesis*. I am using the phrase "Organic Selection" in the article to appear in this journal to designate this "factor" in evolution (see the next heading below).

[5] "Conscious states do have a causal relation to the other organic processes." I do not find, however, that Prof. Cope has made clear just how in his opinion the "selection" by consciousness does work.

It may very justly be asked; if his view be not true, how then can new movements which are adaptive ever be learned at all? This is one of the most important questions, in my view, both for biologists and for psychologists; and my recent work on *Mental Development* is, in its theoretical portion (chap., VIIff), devoted mainly to it, i. e., the problem of *ontogenetic accommodation.* I can not go into details here, but it may suffice to say that Spencer (and Bain after him) laid out what seems to me, with certain modifications urged in my book, the only theory which can stand in court. Its main thought is this, that all new movements which are adaptive or "fit" are selected from *overproduced movements* or *movement variations*, just as creatures are selected from overproduced variations by the natural selection of those which are fit. This process, as I conceive it, I have called "organic selection," a phrase which emphasizes the fact that it is the organism which selects from all its overproduced movements those which are adaptive and beneficial. The part which the intelligence plays is "through pleasure, pain, experience, association, etc., to concentrate the energies of movement upon the limb or system of muscles to be used and to hold the adaptive movement, "select" it, when it has once been struck. In the higher forms both the concentration and the selection are felt as acts of attention.

Such a view extends the application of the general principle of selection through fitness to the *activities of the organism.* To this problem I have devoted some five years of study and experiment with children, etc., and I am now convinced that this "organic selection" bears much the same relation to the doctrine of special creation of ontogenetic adaptations by consciousness which Prof. Cope is reviving, that the Darwinian theory of natural selection bears to the special creation theory of the phylogenetic adaptations of species. The facts which Spencer called "heightened discharge" are capable of formulation of the principle of "motor excess": "the accommodation of an organism to a new stimulation is secured—not by the selection of this stimulation beforehand (nor of the necessary movements)—but by the reinstatement of it by a discharge of the energies of the organism, concentrated, as far as may be, for the excessive stimulation of the organs (muscles, etc.), most nearly fitted by former habit to get this stimulation again,"[6] in which the word "stimulation" stands for the condition favorable to adaptation. After several trials with grotesquely excessive movements, the child (for example) gets the adaptation aimed at more and more perfectly, and the accompanying excessive and use-

[6] *Mental Development*, p. 179. Spencer and Bain hold that the selection is of purely chance adaptations among spontaneous random movements.

less movements fall away. This is the kind of "selection" that consciousness does in its acquisition of new movements. And how the results of it are conserved from generation to generation, without the Lamarkian factor, has been spoken of above.

Finally, a word merely of the relation of consciousness to the energies of the brain. It is clear that this doctrine of selection as applied to muscular movement does away with all necessity for holding that consciousness even directs brain energy. The need of such direction seems to me to be as artificial as Darwin's principle showed the need of special creation to be for the teleological adaptations of the different species. This necessity of supposed directive agency done away in this case as in that, the question of the relation of consciousness to the brain becomes a metaphysical one; just as that of teleology in nature became a metaphysical one; and science can get along without asking it. And biological as well as psychological science should be glad that it is so—should it not?

I may add in closing that of the three headings of this note only the last (third) is based on matters of my private opinion; the other two rest on Prof. Cope's own presuppositions—that of intelligent selection in his sense of the term, and that of the bearing of Social Heredity (which he admits) upon Lamarckism. In another place I hope to take up the psychology of Prof. Cope's new book in some detail.

J. MARK BALDWIN.

Observations on Prof. Baldwin's Reply.—In order to comprehend the question at issue, it is necessary to state certain fundamental principles of evolution. This process consists in the development of the heterogeneous from the homogeneous as Spencer expresses it; or in more specific language, evolution consists in the development of specialized structures from generalized material. Primitive organic or living beings consist of protoplasm which is, as compared with higher organisms, generalized. That is, they are without distinct muscular, nervous, or digestive organs, etc. How are psychic conditions related to this process of specialization? Prof. Baldwin states that an animal is able to "select through pleasure, pain, experience, association, etc., from certain alternative complex movements which are already possible for the limb or member used." This means that under guidance of a form of consciousness, certain existing muscles are selected to perform certain movements, while other muscles are neglected. Now if this be possible to a muscular system specialized into discrete bundles, it is also possible to a primitive contractile protoplasm which is not yet differ-

entiated into discrete muscular and other bodies. In other words it is possible to contract that part of the homogeneous protoplasm which is necessary for the production of a certain movement, and leave that part of the protoplasm which is not necessary to produce the movement, uncontracted. And this is exactly what undifferentiated animals (Protozoa) do, and it is what is done at all stages of differentiation of the muscular system, so far as the differentiation which that muscular system has attained, will permit. It is the sentence which I have quoted above from Prof. Baldwin which induced me to say that he admits the Lamarkian factor. For there is no doubt that it has been this habitual contraction of certain parts of undifferentiated protoplasm which has produced muscular bands, sheets, etc., as distinguished from other histological elements of the organism. If this be true, there is no necessity for the hypothesis of "overproduced movements" as the source of new habits, since those habits may be produced by the direct effect of the selective power of the animal over its own protoplasm. It is not intended by this expression to claim anything more than simple sensation for simple forms of life, or that anything higher than hunger, reproduction temperature, etc., constitute their pleasures and pains.

The theory of natural selection from "overproduced movements" as a *source* of new movements stands on the same basis as all the other theories of natural selection as explanations of the *origin* of anything new. They are impossible in practice, and inaccurate in logic, since in my opinion, following that of Mr. Darwin, they demand of Natural Selection a function of which it is by its definition incapable. That natural selection regulates the survival of movements after they have originated, goes without saying. It is evideut that "overproduced movements" must on Prof. Baldwin's "Organic Selection" theory, include the adaptive one which is destined to survive. The question then is as to the origin of this particular "overproduced" and adaptive movement. The explanation has been given above; i. e. that it is a direct response to the stimulus supplied. The location in the organism of the responsive movement depends on the location of the stimulus, a fact testified to by the close local connection of motor with sensory nerves of general sensation. In the case of responses to special sensation, we may suppose that the responses only became exact as to locality after a period of trial and error, the new movement always having a local relation to the point of stimulus. The beast bites his wound, before he has traced the pain to his enemy. As already pointed out, this process would result in a perfected mechanism which would be inherited. No one can yet explain the mechanism of the control of a mental state

over a contraction of protoplasm. It is one of the ultimate facts of the universe. When Prof. Baldwin admits that an animal can select which of two muscles it will use, or when he admits that an animal can contract any muscle under the stimulus of "pleasure, pain, etc., he admits this ultimate fact, but does not explain it.

As to the scope of Social Heredity as a factor in psychic evolution, it appears to me to be, like that of the higher intelligence, mainly restricted to the higher animals and to man. Maternal instruction among all but the higher animals probably has no existence. Imitation may be supposed to be possible to animals a little lower in the scale. But both factors are to my mind only supplementary to the more vigorous education furnished by the environment, with its wealth of stimuli to "pleasure, pain, experience, association, etc." In regarding Social Heredity as the sole factor of psychic evolution, Prof. Baldwin temporarily loses sight of the intimate connection between mind and its physical basis. The inheritance of mental characteristics is as much a fact as the inheritance of physical structure, and for the reason that the two propositions are identical. One does not believe in either education or imitation as a cause of the repetition of insanity in family lines. We rather believe in a defective brain mechanism, which is inheritable, though fortunately not always inherited. The doctrine of Weismann that acquired characters are not inherited, if true, would furnish the physical conditions for the theory that Social Heredity is the only psychic heredity, but it is impossible to believe that Weismann's doctrine is true. Hence while Social Heredity is true as far as it goes, Lamarkism is also true, and expresses the more fundamental law. The fact that no adequate physical explanation of the inheritance of acquired characters has been reached does not disprove the fact.

E. D. COPE.

ANTHROPOLOGY.[1]

Indian habitation in the Eastern United States.—Mr. Thomas Wilson of the Smithsonian Institution in a recent letter refering to a discussion in Washington as to the shape of Indian habitations east of the Mississippi, says, that while certain of the disputants "agreed that the Plains Indians of the present or modern times used wigwams made with poles fastened together at the top and spreading out in a circle at the bottom after the fashion of a Sibley tent, they

[1] This department is edited by Henry C. Mercer, University of Penna, Phila.

denied that any such structures were used by Indians, in the East. They insisted that these wigwams were confined to the plains and to the prairies and treeless countries, and did not exist, or were not found, and had never been used in the timbered countries—that in the timbered country Indian houses were made of wooden logs with upright sides and a flat or sloping roof. While I knew that many of these were made among the Iroquois of the East, and that this form was adopted in making the long houses (as they were called), I doubted whether they were so built among the nomadic and wandering tribes of Pennsylvania and the West Ohio, Indiana, etc. Can you give me any enlightenment thereon? If so, I will be obliged."

While it is not improbable that the shape of "wigwams," like burial customs varied considerably among the forest Indians, and while any camper out feels that a shelter often temporary, framed in the woods with available boughs, would vary in shape according to circumstances and suggest variation in more permanent structures, no one need hope to speak with final authority upon this subject, who has not ransacked the records of explorers, the narratives of individuals captured by Indians, the *relations* of the Jesuits, and the significant sketches of travellers in the last two centuries.

Dr. Daniel G. Brinton informs me that certain of the Brazilian forest Indians use the tepee form, and speaking of the Lenni Lenape, and quoting Nelson's History of New Jersey, writes: "William Penn describes the dwellings of the Delaware Indians as 'houses of mats, or barks of trees, set on poles, hardly higher than a man.' Pastorius states that 'young trees would be bent towards a common centre and the branches interlaced and fastened together as a frame work, and covered with bark.' Wassenaer says, 'they would construct a circular matted hut, with either angular or rounded top, thatched or lined with mats, a rent hole in the top serving for the escape of smoke.' This last description is strictly that of a tepee and shows that the angular pointed hut was in use by the Mohigan and Lenape Indians. Wassenaers' History is printed in Vol. III New York Documentary History."

The above quotation from Penn, however, if given correctly in Watsons' Annals of Philadelphia Vol. II. p., 153 reads distinctly against the tepee form. "Their houses were made of mats or barks of trees set on poles, *in the fashion of an English barn*, but out of the power of the winds for they are hardly higher than a man." And we find a rectangular structure again ascribed to the work of a band of Lenapes squatting in the suburbs of Philadelphia about 1770-80, in

Watson Vol. II. p. 31 where a person 80 years old in 1842 relates that he well remembers seeing colonies of Indians of twenty or thirty persons, often coming through the town (Germantown) and sitting down in Logan's woods, others in the present (1842) open field south-east of Griggs' place. They would make their huts and stay a whole year at a time and make and sell baskets, ladles and tolerably good fiddles. He has seen them shoot birds and young squirrels there with their bows and arrows. Their huts were made of four upright saplings with crotch limbs at top. The sides and tops were of cedar bushes and branches. In these they lived in the severest winters. Their fire was on the ground and in the middle of the area."

As the barn structure with its ridge pole would take six upright crotched saplings, this rectangle set up by half civilized indians with only four, was not barn shaped but single sloped like the simplest form of shed. The form described above by Pastorius judging from the tendency of elastic saplings when pulled together at the top to bow outward, would probably have resulted in a round roofed structure of the bee hive pattern if round at the base, or if rectangular, in such a building as De Brys' picture made in 1690 refers to Virginia Indians (Contributions to N. A. ethnology Vol. IV) or Captain John Smith carefully draws over the head of the sitting Powhatan in the upper left hand corner of his map of Virginia (see Narr. and Critical History of Am., III, 166.) But if we believe Wassenaer who distinctly describes the Sioux Tepeè we must allow the latter form to the Delawares.

Too much importance need not be ascribed to the minute realistic outlines of habitations made to stand for Indian villages upon certain old maps drawn on a large scale as for instance in Dumont de Montigny's map of Louisiana (1746), when all Indian villages are marked with tepee like points from the Illinois River to New Orleans and from the Mobile to the Misissippi Rivers. On the other hand Du Prats, in a similar map (1758) gives the barn shape.[2] In other maps the structures seem too carefully and designedly drawn to be without archæological value. As when Father Abrahams Almanac Map 1761 (Narrative and Crit. Hist. V, 497) marks seven indian towns in the tepee shape near the junction of the Allegheny and Monongahela Rivers, and Hennepin in his map (1740) of the Mississippi valley and lakes (Narr. and Crit. Hist. IV 252 and 249) and again in his map of the lake region (1683) clearly shows pointed wigwams about the head waters of the Missiesippi, as against small rectangular figures for the lower valley. Hawkins describes a communal Indian house seen in Florida as

[2] Narrative and Critical History of America Vol. V. p. 66.

like a great barn in strength, not inferior to ours. Lescarbot's map of Montreal 1609. (Narr. and Crit. His. IV 304) shows the palisaded Indian village of Hochelaga with barn-shaped round-roofed rectangular structures as in John Smith's cut, and in a map of Lake Ontario and the Iroquois Country 1662-63, (from one of the Jesuit *relations*) the indian villages are barn-shaped and with pointed roofs. La Hontain suggests the same shape in his map of the lake region 1709 (Narr. Crit. Hist. IV 281-261-258) and several Indian lodges of the circular bee-hive pattern surrounded by cultivated enclosures are given by Champlain in his map of Plymouth Harbor 1605. (Narr. and Crit. History IV 109). While not only the round bee-hive pattern, but also the long rectangle with round roof, as in Smith, are carefully drawn by the same explorer in his map of Nauset Harbor, 1604-05 (Land fall of Leif Erickson by Eben Norton Horsford p. 78).

More interesting is the direct evidence of the Indians themselves. The Lenape Stone, found in the Lenape region in 1872, and whose authenticity after ten years observation I have been unable to doubt, shows three pointed figures near trees, unmistakably referring to tepee shaped habitations in the right of the drawing, and another figure similarly outlined on the reverse, (See the Lenape Stone or the Indian and the Mammoth by H. C. Mercer, Putnam, N. Y. 1885). Another stone figured by me from the same locality. (See Lenape Stone p. 94) seems again to be inscribed with three tepee like forms.

No less explicit is the tepee figure upon the so called Winnipeseogee Stone found on the shores of Lake Winnipeseogee. (See Abbotts' Primitive Industry p. 362). George Copway (See Bureau of Ethnology Report 1888-89 p. 493 and 242) shows us Ojibway drawings which doubtless refer to the same pointed form of habitation.

That the sides of the barn shaped structures when built as by the Iroqois were invariably made of logs, is not to be supposed from the statement above quoted from Wm. Penn., and the drawing by Captain John Smith. All things considered, we have reason for supposing, subject to correction from documentary investigation, that though the barn shaped and round roofed rectangular structures were common, not only the bee hive, but the true tepee form were in use by Indians in the Pre-Columbian forest east of the Mississippi.

HENRY C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Novia Scotian Institute of Science.—The 13th of April.—The following papers were read: Preliminary Notes on the Orthoptera of Nova Scotia. By Harry Piers, Esq. Notes on the Newt (D. *viridescens*) and on the Ring-Necked and Garter Snakes (D. *punctatus* and E. *sirtalis*.) By A. H. MacKay, Esq., LL. D., F. R. S. C., Superintendent of Education. On the Calculation of the Conductivity of Mixtures of Aqueous Solutions of Electrolytes having a common ion. By D. MacIntosh, Esq., Physical Laboratory, Dalhousie College.—Harry Piers, *Secretary*.

Boston Society of Natural History.—March 18th.—The following paper was read: Prof. Charles R. Cross, "The X rays." With experimental illustrations.

April 1st.—The following paper was read: Prof. William Libbey, "The Hawaiian Islands."

April 15th.—The following papers were read: Mr. M. L. Fuller, "A new occurrence of Carboniferous fossils in the Narragansett Basin. Prof. Alpheus Hyatt, "The evidence of the descent of man from the ape. A discussion upon the subject of Prof. Hyatt's followed, Prof. Thomas Dwight, Prof. C. S. Minot, and others participating.—Samuel Henshaw, *Secretary*.

American Philosophical Society.—March 20th.—An obituary of Rev. W. H. Furness, by Jos. G. Rosengarten, was presented; Mrs. Cornelius Stevenson read a short paper on "Remains of Libyan Invaders of Egypt," discovered in 1895 by Mr. Flinders Petrie.

April 10.—Prof. Cope made some observations on the figures on a tablet from Nippur, pointing out the physical characters of the men and animals represented.

Academy of Natural Science, Philadelphia.—A meeting of the Anthropological Section was held the 13th of March.—The following papers were read: Prof. F. Edge Kavanagh, addressed the Section on "Right Handedness," was the subject discussed by Drs. Mills, Allen and Brinton, Professors Witmer, Culin, Jastrow and Gudeman.

Anthropological Section was held at the Academy on Friday, April 10th.—The following paper was read: Prof. Lightner Witmer on "Psycho-physical Measurement."—Charles Morrris.

New York Academy of Sciences.—Biological Section, March 9th.—Mr. F. B. Sumner read a paper on "The Descent Tree of the Variations of a Land Snail from the Philippines," illustrated by a lantern slide. Mr. Sumner described the range in variation in size and markings in the shell, and arranged the varieties in the form of a tree of three branches diverging from the most generalized type. It was shown that these several varieties occupy the same geographical region and Mr. Sumner was of the opinion that their occurrence could not be explained by natural selection since if the colorations were supposed to be protective it would be impossible to explain the evolution of these three types. Prof. Osborn, in discussion, was inclined to take the same view. Dr. Dyar, however, thought the explanation by natural selection not necessarily excluded, since the variations seemed analogous to the dimorphism in sphinx larvae, which has been shown by Poulton to be probably due to this factor.

The other paper was by Dr. Arnold Graf on "The Problem of the Transmission of Acquired Characters."

Dr. Graf discussed the views of the modern schools of evolutionists and adopted the view that the transmission of acquired characters must be admitted to occur. He cited several examples which seemed to support this view, and especially discussed the sucker in leeches as an adaptation to parasitism and the evolution of the chambered shell in a series of fossil Cephalopods.

Prof. Osborn remarked in criticism of Dr. Graf's paper that this statement does not appear to recognize the distinction between *ontogenic* and *phylogenic* variation, or that the adult from any organism is an exponent of the stirp, or constitution. The Environment. If the environment is normal the adult would be normal, but if the environment (which includes all the atmospheric, chemical, nutritive, motor and psychical circumstances under which the animal is reared) were to change, the adult would change correspondingly; and these changes would be so profound that in many cases it would appear as if the constitution, or stirp, had also changed. Illustrations might be given of changes of the most profound character induced by changes in either of the above factors of the environment, and in the case of the motor factor or animal motion, the habits of the animal might, in the course of a life time, profoundly modify its structure. For example, if the human infant were brought up in the branches of a tree as an arboreal type instead of as a terrestrial, bi-pedal type, there is little doubt that some of the well known early adaptations to arboreal habit (such as the turning in of the soles of the feet, and the grasping of the

hands) might be retained and cultivated, thus a profoundly different type of man would be produced. Similar changes in the action of environment are constantly in progress in nature since there is no doubt that the changes of environment and the new habits which it so brings about far outstrip all changes in constitution. This fact which has not been sufficiently emphasized before, offers an explanation of the evidence advanced by Cope and other writers that change in the forms of the skeletons of the vertebrates first appears in ontogeny and subsequently in phylogeny. During the enormously long period of time in which habits induced ontogenic variations it is possible for natural selection to work very slowly and gradually upon predispositions to useful correlated variations, and thus what are primarily *ontogenic variations* become slowly apparent as *phylogenic variations* or congential characters of the race.—C. L. BRISTOL, *Secretary.*

The Academy of Science of St. Louis.—March 16th.—Mr. Trelease presented some of the results of a recent study of the poplars of North America, made by him for the Systematic Botany of North America, and exhibited specimens of the several species and recognized varieties. Specimens were also exhibited of an apparently undescribed poplar from the mountains of northern Mexico, which he proposed to characterize shortly, and, for comparison, specimens of the two other species of poplar known to occur in Mexico, and of the European allies of the supposed new species, were laid before the Academy. The paper was discussed by Drs. Green, Glatfelter, and Kinner, Mr. Winslow, and Professor Kinealy.

The Academy, in co-operation with the joint committee of the scientific societies of Washington, adopted resolutions favoring the appointment of a permanent chief for the scientific work of the United States Department of Agriculture.

April 6th.—Prof. C. R. Sanger spoke on the commercial synthesis of acetylene, illustrating the flame procurable from this gas when burned with a proper proportion of air.

Prof. Sanger also presented the results of a preliminary biological and chemical examination into the ice supply of St. Louis, and exhibited a device for melting the ice in such examinations without danger of contamination from atmospheric ammonia, etc.

The Secretary presented for publication, by title, a paper by Mr. Charles Robertson, entitled "Flowers and Insects."

Mr. William H. Roever presented a paper on the geometry of the lines of force from an electrified body, in which it was shown that:

(a.) the curve representing a line of force proceeding from a system consisting of two parallel electrified lines, is the locus of the intersection of two straight lines, rotating in the same plane about these two parallel lines as axes with uniform but different angular velocities. (b.) the curve representing a line of force proceeding from a system consisting of two electrified points, is the locus of the intersection of two straight lines, rotating, in the same plane about parallel axes passing through those points, in such a manner that the versines of their angles of inclination to the plane of the axes change at uniform but different rates.

April 20th.—Dr. C. M. Woodward presented the results of a study of certain statistics of school attendance, from which it appeared that the average age of withdrawal from the public schools in three cities compared was as follows: Boston, 15.8; Chicago, 14.6; St. Louis, 13.7.

Professor J. H. Kinealy exhibited and gave a mathematical discussion of the Stang planimeter, an interesting and simple instrument of Danish invention, but improved in the United States.

WILLIAM TRELEASE, *Recording Secretary.*

U. S. National Academy of Sciences.—April 21, 1896.—The following papers were read: The Geological Efficacy of Alkali Carbonate Solutions, E. W. Hilgard; On the Color Relations of Atoms, Ions and Molecules, M. Carey Lea; On the Characters of the Otocœlidæ, E. D. Cope; Exhibition of a Linkage whose motion shows the Laws of Refraction of Light, A. M. Mayer; Location in Paris of the Dwelling of Malus, in which he made the discovery of the Polarization of Light by Reflection, A. M. Mayer; (1) On Experiments showing that the X-Rays cannot be Polarized by passing through Herapathite; (2) The Density of Herapathite; (3) Formulæ of Transmission of the X-Rays through Glass, Tourmaline and Herapathite, A. M. Mayer; On the X-Rays from a Statical Current produced by a Rapidly Revolving Leather Belt, W. A. Rogers and Frederick Brown; Biographical Memoir of James Edward Oliver, G. W. Hill; Biographical Memoir of Charles Henry Davis, C. H. Davis; Biographical Memoir of George Engelmann, C. A. White; Legislation Relating to Standards, T. C. Mendenhall; On the Determination of the Coefficient of Expansion of Jessop's Steel, between the limits of O° and 64° C., by the Interferential Method, E. W. Morley and W. A. Rogers; On the Separate Measurement, by the Interferential Method, of the Heating Effect of Pure Radiations and of an Envelope of Heated Air, W. A. Rogers; On the Logic of Quantity, C. S. Peirce; Judgement in Sensation and Perception, J. W. Powell; The Variability in Fermenting Power of the Colon

Bacillus under Different Conditions, A. W. Peckham (Presented by J. S. Billings); Experiments on the Reflection of the Röntgen Rays, O. N. Rood; Notes on Röntgen Rays, H. A. Rowland; Some Studies in Chemical Equilibrium, Ira Remsen; The Decomposition of Diazo-compounds by Alcohol, Ira Remsen; On Double Halides containing Organic Bases, Ira Remsen; Results of Researches of Forty Binary Stars, T. J. J. See; On a Remarkable New Family of Deep-sea Cephalopoda and its bearing on Molluscan Morphology, A. E. Verrill; The Question of the Molluscan Archetype, an Archi-mollusk, A. E. Verrill; On some Points in the Morphology and Phylogeny of the Gastropoda, A. E. Verrill; Source of X-Rays, A. A. Michelson and S. W. Stratton; The Relative Permeability of Magnesium and Aluminum to the Röntgen Rays, A. W. Wright; The State of Carbondioxide at the Critical Temperature, C. Barus; The Motion of a Submerged Thread of Mercury, C. Barus; On a Method of Obtaining Variable Capillary Apertures of Specified Diameter, C. Barus; On a New Type of Telescope Free from Secondary Color, C. S. Hastings; The Olindiadæ and other Medusæ, W. K. Brooks; Budding in Perophora, W. K. Brooks and George Lefevre; Anatomy of Yoldia, W. K. Brooks and Gilman Drew; On the *Pithecanthropus erectus* from the Tertiary of Java, O. C. Marsh.

C. D. Walcott and R. S. Woodward were elected members.

SCIENTIFIC NEWS.

Prof. Charles L. Edwards of the University of Cincinnati is to open a biological station this summer at Biscayne Bay, Florida. The place is well situated for the study of the tropical and sub-tropical flora and fauna, while its situation upon the continent makes it more readily accessible than the West India Islands. There will be opportunity for investigation while less mature students will have lectures and laboratory instructions. The session begins June 22d, and continues six weeks. A laboratory fee of $25,00 covers tuition, use of apparatus, reagents, etc., and Prof. Edwards estimates the total necessary expenses of each student, including board, railroad fares, etc., at from $100 to $125. It is also proposed to open a department of laboratory supply and to furnish all available material properly prepared at reasonable rates. For further information address Prof. Edwards at the University of Cincinnati.

Among the recent appointments to honorary membership in Learned Societies we notice, Sir W. H. Flower, by the Swedish Academy of Science; Prof. E. Ray Lankester, by the Russian Academy of Science; A. N. Beketow, Prof. Jas. Hall, Charles D. Walcott and Dr. G. Retzius by the St. Petersburg Academy of Science.

Dr. G. Lawson, botanist, of Halifax, N. S., died December 10th, 1895. It was owing to a confusion in names that the report of the death of the Canadian geologist, G. Dawson, arose.

The French Association for the Advancement of Science held its meeting this year at Tunis, from April 1 to 11. The Botanical Society of France, met at the same time and place.

Dr. George Baur, of the University of Chicago, will spend the summer in Munich, his former home, where he will study the rich paleontological collections of the University.

An expedition started, the middle of March to explore the interior of New Guinea. Dr. Lauterbach the leader takes charge of the botany, Dr. Kersting of the zoology.

The report of the death of the botanist K. Wilhelm, of Vienna is an error, caused by a confusion of names, his brother G. Wilhelm having died Nov. 30th, 1895.

Dr. H. M. Ward, of Cooper's Hill, England, accepts the Professorship of Botany in the University of Cambridge as successor to the late Professor Babbington.

Prof. K. G. Huefner, of Tübingen, has been called to the University of Strasburg where he succeeded the late Prof. Hoppe Seyler in the chair of Physiological Chemistry.

Prof. F. von Sandberger, who recently celebrated his fifty year Doctor-jubilee, has retired from the Professorship of Mineralogy in the University of Würzburg.

Prof. W. A. Locy, for several years Professor of Biology in Lake Forest University goes to Northwestern University, Evanston, Ill., as Professor of Zoology.

H. A. Miers, assistant keeper in the British Museum, goes to the University of Oxford as Professor of Mineralogy, succeeding the late Professor Maskelyne.

Dr. H. Schauinsland, of Bremen, has gone to the Island of Laysan for a ten month's exploring expedition, intending to study both the flora and fauna.

Dr. Looss, for several years docent in the University of Leipzig, has been advanced to the position of Extraordinary Professor.

Dr. E. Sickenberger, Professor of Botany and Chemistry in the medical school of Cairo, Egypt, died December 10th, 1895.

Dr. L. Edinger, of Frankfort, A. M. well known for his researches on the brain, has been honored with the title of Professor.

Dr. F. Saccardo, has been appointed Professor of Plant Pathology in the school of Oenology and Viticulture at Avellino.

Dr. P. Tauber, of Berlin, has sailed for South America intending to study the plants of Brazil, Venezuela and Guinea.

Dr. G. Wagener, Professor of Anatomy in the University of Marburg, died February 10th, 1896, at the age of 70.

Dr. F. Hochstetter, formerly of Vienna, goes to the University of Innsbruck, as ordinary Professor of Anatomy.

Dr. Katzer, has been elected Director of the Mineralogical-Geological section of the Museum of Para, Brazil.

Dr. L. Neumann has been appointed Ordinary Professor of Geography at the University of Freiberg.

Dr. E. Topsent, of Rheims, has been called to the chair of zoology in the Medical School at Rennes, France.

Dr. Seidentopf, of Bremen, has been appointed Assistant in Minerology in the University of Göttingen.

Dr. G. Horvath of Budapesth has been appointed Director of the Royal Hungarian Museum, zoological section.

Lieut H. E. Barnes, well known through his studies of Asiatic ornithology, died recently at the age of 48.

Dr. A. Schadmberg, an investigator of the flora and ethnology of the Philippines, died recently in Manila.

Count J. von Bergenstamm, the well known student of the Diptera, died January 31, 1896 in Vienna.

Dr. A. Zimmermann, becomes Private docent in Vegetable Physiology in the University of Berlin.

Dr. L. Buscalone, of Turin, goes to the University of Göttingen as Assistant in Plant Physiology.

G. C. Druce has been elected Custodian of the Fielding herbarium of the University of Oxford.

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AMERICAN NATURALIST

VOL. XXX. June, 1896. 354

A NEW FACTOR IN EVOLUTION.

BY J. MARK BALDWIN.

In several recent publications I have developed, from different points of view, some considerations which tend to bring out a certain influence at work in organic evolution which I venture to call "a new factor." I give below a list of references[1] to these publications and shall refer to them by number as this paper proceeds. The object of the present paper is to

[1] References:

(1). *Imitation: a Chapter in the Natural History of Consciousness, Mind* (London), Jan., 1894. Citations from earlier papers will be found in this article and in the next reference.

(2). *Mental Development in the Child and the Race* (1st. ed., April, 1895; 2nd. ed., Oct., 1895; Macmillan & Co. The present paper expands an additional chapter (Chap. XVII) added in the German and French editions and to be incorporated in the third English edition.

(3). *Consciousness and Evolution, Science*, N. Y., August, 23, 1895; reprinted printed in the AMERICAN NATURALIST, April, 1896.

(4). *Heredity and Instinct* (I), *Science*, March 20, 1896. Discussion before N. Y. Acad. of Sci., Jan. 31, 1896.

(5). *Heredity and Instinct* (II), *Science*, April 10, 1896.

(6). *Physical and Social Heredity, Amer. Naturalist*, May, 1896.

(7). *Consciousness and Evolution, Psychol. Review*, May, 1896. Discussion before Amer. Psychol. Association, Dec. 28, 1895.

gather into one sketch an outline of the view of the process of development which these different publications have hinged upon.

The problems involved in a theory of organic development may be gathered up under three great heads: Ontogeny, Phylogeny, Heredity. The general consideration, the "factor" which I propose to bring out, is operative in the first instance, in the field of *Ontogeny*; I shall consequently speak first of the problem of Ontogeny, then of that of Phylogeny, in so far as the topic dealt with makes it necessary, then of that of Heredity, under the same limitation, and finally, give some definitions and conclusions.

I.

Ontogeny: "Organic Selection" (see ref. 2, chap. vii).—The series of facts which investigation in this field has to deal with are those of the individual creature's development; and two sorts of facts may be distinguished from the point of view of the *functions which an organism performs in the course of his life history.* There is, in the first place, the development of his heredity impulse, the unfolding of his heredity in the forms and functions which characterize his kind, together with the congenital variations which characterize the particular indiual—the phylogenetic variations, which are constitutional to him; and there is, in the second place, the series of functions, acts, etc., *which he learns to do himself in the course of his life.* All of these latter, the *special modifications which an organism undergoes during its ontogeny,* thrown together, have been called "acquired characters," and we may use that expression or adopt one recently suggested by Osborn,[2] "ontogenic variations" (except that I should prefer the form "ontogenetic variations"), if the word variations seems appropriate at all.

[2] Reported in *Science,* April 3rd.; also used by him before N. Y. Acad. of Sci., April 13th. There is some confusion between the two terminations "genic" and "genetic." I think the proper distinction is that which reserves the former, "genic," for application in cases in which the word to which it is affixed qualifies a term used *actively,* while the other, "genetic" conveys similarly a *passive* signification; thus agencies, causes, influences, etc., and "ontogenic phylogenic, etc.," while effects, consequences, etc, and "ontogenetic, phylogenetic, etc."

Assuming that there are such new or modified functions, in the first instance, and such "acquired characters," arising by the law of "use and disuse" from these new functions, our farther question is about them. And the question is this: How does an organism come to be modified during its life history?

In answer to this question we find that there are three different sorts of ontogenic agencies which should be distinguished—each of which works to produce ontogenetic modifications, adaptations, or variations. These are: first, the physical agencies and influences in the environment which work upon the organism to produce modifications of its form and functions. They include all chemical agents, strains, contacts, hindrances to growth, temperature changes, etc. As far as these forces work changes in the organism, the changes may be considered largely "fortuitous" or accidental. Considering the forces which produce them I propose to call them "physico-genetic." Spencer's theory of ontogenetic development rests largely upon the occurrence of lucky movements brought out by such accidental influences. Second, there is a class of modifications which arise from the spontaneous activities of the organism itself in the carrying out of its normal congenital functions. These variations and adaptations are seen in a remarkable way in plants, in unicellular creatures, in very young children. There seems to be a readiness and capacity on the part of the organism to "rise to the occasion," as it were, and make gain out of the circumstances of its life. The facts have been put in evidence (for plants) by Henslow, Pfeffer, Sachs; (for micro-organisms) by Binet, Bunge; (in human pathology) by Bernheim, Janet; (in children) by Baldwin (ref. 2, chap. vi.) (See citations in ref. 2, chap. ix, and in Orr, *Theory of Development*, chap. iv). These changes I propose to call "neuro-genetic," laying emphasis on what is called by Romanes, Morgan and others, the "selective property" of the nervous system, and of life generally. Third, there is the great series of adaptations secured by conscious agency, which we may throw together as "psycho-genetic." The processes involved here are all classed broadly under the term "intelligent," i. e., imitation, gregarious influences, maternal in-

struction, the lessons of pleasure and pain, and of experience generally, and reasoning from means to ends, etc.

We reach, therefore, the following scheme:

Ontogenetic Modifications.	*Ontogenic Agencies.*
1. Physico-genetic.	1. Mechanical.
2. Neuro-genetic.	2. Nervous.
3. Psycho-genetic.	3. Intelligent.
	Imitation.
	Pleasure and pain.
	Reasoning.

Now it is evident that there are two very distinct questions which come up as soon as we admit modifications of function and of structure in ontogenetic development: first, there is the question as to how these modifications can come to be adaptive in the life of the individual creature. Or in other words: What is the method of the individual's growth and adaptation as shown in the well known law of "use and disuse?" Looked at functionally, we see that the organism manages somehow to accommodate itself to conditions which are favorable, to repeat movements which are adaptive, and so to grow by the principle of use. This involves some sort of selection, from the actual ontogenetic variations, of certain ones—certain functions, etc. Certain other possible and actual functions and structures decay from disuse. Whatever the method of doing this may be, we may simply, at this point, claim the law of use and disuse, as applicable in ontogenetic development, and apply the phrase, "Organic Selection," to the organism's behavior in acquiring new modes or modifications of adaptive function with its influence of structure. The question of the method of "Organic Selection" is taken up below (IV); here, I may repeat, we simply assume what every one admits in some form, that such adaptations of function—"accommodations" the psychologist calls them, the processes of learning new movements, etc.—*do occur.* We then reach another question, second; what place these adaptations have in the general theory of development.

Effects of Organic Selection.—First, we may note the results of this principle in the creature's own private life.

1. *By securing adaptations, accommodations, in special circumstances the creature is kept alive* (ref. 2, 1st ed., pp. 172 ff.). This is true in all the three spheres of ontogenetic variation distinguished in the table above. The creatures which can stand the "storm and stress" of the physical influences of the environment, and of the changes which occur in the environment, *by undergoing modifications of their congenital functions or of the structures which they get congenitally—these creatures will live; while those which cannot, will not.* In the sphere of neurogenetic variations we find a superb series of adaptations by lower as well as higher organisms during the course of ontogenetic development (ref. 2, chap. ix). And in the highest sphere, that of intelligence (including the phenomena of consciousness of all kinds, experience of pleasure and pain, imitation, etc.), we find individual accommodations on the tremendous scale which culminates in the skilful performances of human volition, invention, etc. The progress of the child in all the learning processes which lead him on to be a man, just illustrates this higher form of ontogenetic adaptation (ref. 2, chap. x–xiii).

All these instances are associated in the higher organisms, and all of them unite to *keep the creature alive.*

2. By this means *those congenital or phylogenetic variations are kept in existence, which lend themselves to intelligent, imitative, adaptive, and mechanical modification during the lifetime of the creatures which have them.* Other congenital variations are not thus kept in existence. So there arises a more or less widespread series of *determinate variations in each generation's ontogenesis* (ref. 3, 4, 5).[3]

[3] "It is necessary to consider further how certain reactions of one single organism can be selected so as to adapt the organism better and give it a life history. Let us at the outset call this process "Organic Selection" in contrast with the Natural Selection of whole organisms. . . . If this (natural selection) worked alone, every change in the environment would weed out all life except those organisms, which by accidental variation reacted already in the way demanded by the changed conditions—in every case new organisms showing variations, not, in any case, new elements of life-history in the old organisms. In order to the latter we would have to conceive . . . some modification of the old reactions in an organism through the influence of new conditions. . . . We are, accordingly, left to the view that the new stimulations brought by changes in the environment

The further applications of the principle lead us over into the field of our second question, i. e., phylogeny.

II.

Phylogeny: Physical Heredity.—The question of phylogenetic development considered apart, in so far as may be, from that of heredity, is the question as to what the factors really are which show themselves in evolutionary progress from generation to generation. The most important series of facts recently brought to light are those which show what is called "determinate variation" from one generation to another. This has been insisted on by the paleontologists. Of the two current theories of heredity, only one, Neo-Lamarkism—by means of its principle of the inheritance of acquired characters—has been able to account for this fact of determinate phylogenetic change. Weismann admits the inadequacy of the principle of natural selection, as operative on rival organisms, to explain variations when they are wanted or, as he puts it, "the right variations in the right place" (*Monist*, Jan., '96).

I have argued, however, in detail that the assumption of determinate variations of function in ontogenesis, under the principle of neurogenetic and psychogenetic adaptation, does away with the need of appealing to the Lamarkian factor. In the case i. g., of instincts, "if we do not assume consciousness, then natural selection is inadequate; but if we do assume consciousness, then the inheritance of acquired characters is unnecessary" (ref. 5).

"The intelligence which is appealed to, to take the place of instinct and to give rise to it, uses just these partial variations which tend in the direction of the instinct; so the intelligence *supplements* such partial co-ordinations, makes them functional, and *so keeps the creature alive.* In the phrase of Prof.

themselves modify the reactions of an organism. . . . The facts show that individual organisms do acquire new adaptations in their lifetime, and that is our first problem. If in solving it we find a principle which may also serve as a principle of race-development, then we may possibly use it against the 'all sufficiency of natural selection' or in its support" (ref. 2, 1st. ed., pp. 175–6.)

Lloyd Morgan, this prevents the 'incidence of natural selection.' So the supposition that intelligence is operative turns out to be just the supposition which makes use-inheritance unnecessary. Thus kept alive, the species has all the time necessary to perfect the variations required by a complete instinct. And when we bear in mind that the variation required is not on the muscular side to any great extent, but in the central brain connections, and is a slight variation for functional purposes at the best, the hypothesis of use-inheritance becomes not only unnecessary, but to my mind quite superfluous" (ref. 4, p. 439). And for adaptations generally, "the most plastic individuals will be preserved to do the advantageous things for which their variations show them to be the most fit, and the next generation will show an emphasis of just this direction in its variations" (ref. 3, p. 221).

We get, therefore, from Organic Selection, certain results in the sphere of phylogeny:

1. *This principle secures by survival certain lines of determinate phylogenetic variation in the directions of the determinate ontogenetic adaptations of the earlier generation.* The variations which were utilized for ontogenetic adaptation in the earlier generation, being thus kept in existence, are utilized more widely in the subsequent generation (ref. 3, 4). "Congenital variations, on the one hand, are kept alive and made effective by their use for adaptations in the life of the individual; and, on the other hand, adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarkian factor" (ref. 3). And in cases of conscious adaptation: "We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an adaptation, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses.* The principle of 'selective value' applies to the others or to some of them. So natural selection kills off the others; and the *future*

development at each stage of a species' development must be in the directions thus ratified by intelligence. So also with imitation. Only those imitative actions of a creature which are useful to him will survive in the species, for in so far as he imitates actions which are injurious he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Neo-Darwinian theory; and in this sense we may say that consciousness is a 'factor'" (ref. 4).

2. *The mean of phylogenetic variation being thus made more determinate, further phylogenetic variations follow about this mean, and these variations are again utilized by Organic Selection for ontogenetic adaptation.* So there is continual phylogenetic progress in the directions set by ontogenetic adaptation (ref. 3, 4, 5). "The intelligence supplements slight co-adaptations and so gives them selective value; but it does not keep them from getting farther selective value as instincts, reflexes, etc., by farther variation" (ref. 5). "The imitative function, by using muscular co-ordinations, supplements them, secures adaptations, keeps the creature alive, prevents the 'incidence of natural selection,' and so gives the species all the time necessary to get the variations required for the full instinctive performance of the function" (ref. 4). But, "Conscious imitation, while it prevents the incidence of natural selection, as has been seen, and so keeps alive the creatures which have no instincts for the performance of the actions required, nevertheless does not subserve the utilities which the special instincts do, nor prevent them from having the selective value of which Romanes speaks. Accordingly, on the more general definition of intelligence, which includes in it all conscious imitation, use of maternal instruction, and that sort of thing—no less than on the more special definition—we still find the principal of natural selection operative" (ref. 5).

3. *This completely disposes of the Lamarkian factor as far as two lines of evidence for it are concerned.* First, the evidence drawn from function, "use and disuse," is discredited; since by "organic selection," the reappearance, in subsequent generations, of the variations first secured in ontogenesis is ac-

counted for without the inheritance of acquired characters. So also the evidence drawn from paleontology which cites progressive variations resting on functional use and disuse. Second, the evidence drawn from the facts of "determinate variations;" since by this principle we have the preservation of such variations in phylogeny without the inheritance of acquired characters.

4. *But this is not Preformism in the old sense; since the adpatations made in ontogenetic development which "set" the direction of evolution are novelties of function in whole or part* (although they utilize congenital variations of structure). And it is only by the exercise of these novel functions that the creatures are kept alive to propagate and thus produce further variations of structure which may in time make the whole function, with its adequate structure, congenital. Romanes' argument from "partial co-adaptations" and "selective value," seem to hold in the case of reflex and instinctive functions (ref. 4, 5), as against the old preformist or Weismannist view, although the operation of Organic Selection, as now explained, renders them ineffective when urged in support of Lamarkism. "We may imagine creatures, whose hands were used for holding only with the thumb and fingers on the same side of the object held, to have first discovered, under stress of circumstances and with variations which permitted the further adaptation, how to make use of the thumb for grasping opposite to the fingers, as we now do. Then let us suppose that this proved of such utility that all the young that did not do it were killed off; the next generation following would be plastic, intelligent, or imitative, enough to do it also. They would use the same co-ordinations and prevent natural selection getting its operation on them; and so instinctive 'thumb-grasping' might be waited for indefinitely by the species and then be got as an instinct altogether apart from use-inheritance" (ref. 4). "I have cited 'thumb-grasping' because we can see in the child the anticipation, by intelligence and imitation, of the use of the thumb for the adaptation which the Simian probably gets entirely by instinct, and which I think an isolated and weak-minded child, say, would also come to do by instinct'" (ref. 4).

5. It seems to me also—though I hardly dare venture into a field belonging so strictly to the technical biologist—that *this principle might not only explain many cases of widespread "determinate variations" appearing suddenly, let us say, in fossil deposits, but the fact that variations seem often to be "discontinuous."* Suppose, for example, certain animals, varying, in respect to a certain quality, from a to n about a mean x. The mean x would be the case most likely to be preserved in fossil form (seeing that there are vastly more of them). Now suppose a sweeping change in the environment, in such a way that only the variations lying near the extreme n can accommodate to it and live to reproduce. The next generation would then show variations about the mean n. And the chances of fossils from this generation, and the subsequent ones, would be of creatures approximating n. Here would be a great discontinuity in the chain and also a widespread prevalence of these variations in a set direction. This seems especially evident when we consider that the paleontologist does not deal with successive generations, but with widely remote periods, and the smallest lapse of time which he can take cognizance of is long enough to give the new mean of variation, n, a lot of generations in which to multiply and deposit its representative fossils. Of course, this would be only the action of natural selection upon "preformed" variations in those cases which did not involve positive changes, in structure and function, *acquired in ontogenesis*; but in so far as such ontogenetic adaptations were actually there, the extent of difference of the n mean from the x mean would be greater, and hence the resources of explanation, both of the sudden prevalence of the new type and of its discontinuity from the earlier, would be much increased. This additional resource, then, is due to the "Organic Selection" factor.

We seem to be able also to utilize all the evidence usually cited for the functional origin of specific characters and groupings of characters. So far as the Lamarkians have a strong case here, it remains as strong if Organic Selection be substituted for the "inheritance of acquired characters." This is especially true where intelligent and imitative adaptations are

involved, as in the case of instinct. This "may give the reason, e. g., that instincts are so often coterminous with the limits of species. Similar structures find the similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which represents species, and the functional definition which represents instinct, largely keep to the same lines" (ref. 5).

6. It seems proper, therefore, to call the influence of Organic Selection "a new factor;" for it gives a method of deriving the determinate gains of phylogeny from the adaptations of ontogeny without holding to either of the two current theories. *The ontogenetic adaptations are really new, not performed; and they are really reproduced in succeeding generations, although not physically inherited.*

(*To be continued.*)

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

BY ERWIN F. SMITH.

(*Continued from page 378*).

2. UPWARD MOVEMENT OF ONE PER CENT. EOSINE WATER THROUGH CUT STEMS PLUGGED WITH GELATINE.

In all of these experiments a somewhat stiff gelatine was used (15 per cent.) to secure a relatively high melting point (about 27° C.) and this was tinged with India ink, so that the location of the gelatine plugs inside of the vessels could be determined accurately on cross section. Both substances being as far as has been determined inert to the plant, it is not likely that they could have in any way injured the carrying capacity of the walls of the vessels.[2]

[2]Recently Dixon and Joly (Annals of Botany, Sept., 1895, p. 403) have raised some objections to this view, but it cannot be said that they have fully established their case.

(No. 5). A much branched large vine, bearing many leaves, at least 60, the breadth of the best ones being 17 cm. The distance from the cut stem to the extremity of the longest shoot measured 218 centimeters. March 20, 1:52 p. m. The basal part of the stem was plunged into gelatine at 45° C., severed smoothly and left 40 minutes, the temperature of the gelatine when the cut stem was removed being 34° C. At 1:30 p. m. the dry bulb registered 18° C, and the wet bulb 16.5° C., and transpiration during the afternoon was probably not very active. On removing the cut stem from the melted gelatine, it was immediately plunged into water at 16° C., and kept there 10 minutes, i. e., until the gelatine was congealed. At 2:38 p. m. the stem was shortened about 3 millimeters and plunged into 1 per cent eosine water at a temperature of 16° C. A careful examination of the 3 millimeter segment showed that by far the larger number of the vessels of the stem (nearly all) were full of the black gelatine, but for unknown reasons, some of the spirals and a very few of the larger pitted vessels were not filled. 4:10 p. m. No trace of color in the veins of any of the leaves. 4:50 p. m. Not a trace of color in any of the leaves. The stem has now been in the eosine water 2 hours and 12 minutes. March 21, 11:30 a. m. The house is dryer than yesterday, and the demand on the plant for water is enormous. The foliage is shriveled or flabby, including the petioles,and hangs down, but not a trace of eosine is to be seen anywhere in any of the leaves, although the lowest leaf is within 24 centimeters of the cut end. There has also been no perceptible lowering of the level of the eosine water in which the stem rests. The sun shines hot through the glass, the temperature in the shade on a level with the bench being 24° C., while in the sun, four inches above, it is 29° C. 12:20 p. m. No trace of eosine visible externally in any part of stem or leaves, although it is nearly 22 hours since the stem was plunged into the stain. The stem was now removed and cut for examination with the following results: 2 cm. up.—There is a trifling stain. Nearly all of the vessels are full of gelatine, and in some of the bundles the stain shows *only in those spirals which did not fill.* 4 cm.

up.—Most of the vessels are full of gelatine. 8 cm. up.—About one-third of the vessels are full. 12 cm. up.—No gelatine in the vessels. Three of the nine bundles show no trace of stain. There is no fluid in the lumen of any of the vessels of the other 6 bundles, but the walls of the vessels and connecting tissues are partially stained in 4 bundles, and entirely in 2.30 cm. up, i. e., above several nodes.—The walls of a part of the vessels of each bundle are tinged with the stain, but less strongly than at 12 centimeters. The lignified parts of all of the spiral vessels show the stain, but part of the pitted vessels are free from stain, and all of the phloem, fundamental tissue, collenchyma and sclerenchyma. It would seem as if the spirals brought up the stain (the almost inappreciable quantity which passed by the gelatine plugs), and that from these it diffused out into the rest of the xylem. When the walls of the pitted vessels were stained, those of the connecting tracheids were also stained. 50 cm. up.—The red color is restricted to 4 inner and 2 outer bundles. In one of the two outer bundles and in all of the inner ones, the stain is confined to the region of the spirals and is barely visible in these. Longitudinal and oblique cuts also show in a striking way the restriction of the stain to the spirals. 60 cm. up.—Barest trace of stain in 3 bundles. 65 cm. up.—Only slightest trace of stain, restricted to the spirals of 2 bundles. 70 cm. up.—No trace of stain. 80 cm. up.—No trace of color. Two branches were given off just under the 50 cm. cut: In the lower there was a trace of stain in two bundles at 5 cm. from the main stem; in the other there was no trace of the stain, 2 cm. or 5 cm. out.

Here there was every opportunity for water to pass through the walls of the vessels, the osmotic pull probably amounting to a pressure of several atmospheres, but there is no conclusive evidence that even a single drop passed up in this way. The very slight amount of eosine which passed up the stem may have gone through the walls of the vessels in obedience to the law of surface tension or may have passed through the lumina, owing to the incomplete plugging of some of the vessels, those which showed no gelatine being probably plugged by air and inoperative.

(No. 15). An old, much branched vine which has borne fruit and is nearly past profitable culture. The principal stem is 200 centimetres long (measured from the cut near the earth); the longest branch is 105 cm.; the next longest is 65 cm. The vine has lost much of its foliage but bears about 60 medium sized leaves (10 to 15 cm. broad) and as many more smaller ones, mostly from short, lateral branches, so that the transpiration on a sunny, windy day, like this, must be very considerable. March 22, 2:05 p. m. The base of the vine, which had previously been dug up carefully by the roots, and put at once into water, was cut 30 cm. above the roots under gelatine at a temperature of 40° C. 2:30 p. m. Many of the leaves have begun to wilt, showing that the transpiration and negative pressure must be very great. The stem was now shortened under the gelatine one centimeter and the segment examined. Most of the vessels were full of gelatine but not all. A dozen or so of the pitted vessels were empty and more than that many spirals. The vessels of some bundles appeared to be completely full including the spirals. 2:40 p. m. The foliage now shows a decided droop. Stem cut again under the still fluid gelatine 4 cm. up. Fully one-third of the pitted vessels are free from gelatine (contain air), but most of the spirals seem to be full. The torn central stem cavity is also full of gelatine and was in those examined yesterday. 3:30 p. m. Marked droop of all the foliage. Stem removed quickly from the gelatine which has been kept at 40° C. and plunged into water at 19° C. 3:50 p. m. Stem shortened slightly and put at once into 1 per cent eosine water cooled down to 14° C. An examination of the segment just removed shows that nearly all of the vessels are full of the solidified gelatine, but not *all.* 4:10 p. m. No trace of stain in any of the leaves, although those nearest the cut are only 15 centimeters up. 5:00 p. m. The vine is drooping and needs water badly, but can get none either through the walls of the vessels or through the gelatine plugs. An hour and ten minutes has passed since the stem was plunged into the eosine and yet there is not a trace of stain in any leaf, although the eosine water would have gone to the end of the vine and been dis-

tinctly visible in the veins of every leaf in 15 minutes but for the gelatine plugs, as we have seen from the preceding experiments. March 23, 11:00 a. m. Two-thirds of all the foliage of this vine is now dry-shriveled, and the remainder is very flabby, but there is not a trace of eosine visible in any of the leaves, not even in those which are near the cut end and still living. Sun shining, hot, some wind outside. Temperature in the shade, 6 inches above the bench, 27° C.; in sun, 30° C.; dry bulb, 26.5° C.; wet bulb, 22° C. Active transpiration. 1:45 p. m. Nine-tenths of the foliage is crisp-dry. No trace of color in any part of the stem or foliage. The stem was now removed from the fluid and cut for examination with the following results: One-half centimeter up.—Most of the pitted vessels were full of the black gelatine, one showed a rim of gelatine with a central air bubble. Spirals mostly not full. Diffuse stain in the parenchyma. The stain has passed through the gelatine itself in many instances (owing perhaps to its liquefaction on a. m. of March 23, when a beaker of water, in which the eosine bottle rested, became lukewarm and a beaker of gelatine on the bench near by became fluid). Six cm. up.—Comparatively few pitted vessels have gelatine in them; some of these are full, others have only a rim of gelatine around a succession of air bubbles. Eight cm. up.—The torn central stem cavity, which is still visible, contains no gelatine. A few pitted vessels contain gelatine mostly as rims around the walls, air being in the center. Ten cm. up.—No gelatine. Stain very feeble, diffused somewhat into the parenchyma. About ¼ of the pitted vessels unstained; color restricted to the spiral vessels in one bundle. Twenty cm. up.—No gelatine. Stain slight, not entirely restricted to the bundles but diffused out into the parenchyma on one side of the stem. Forty-three cm. up.—Slight traces of stain in 7 bundles, restricted to the spirals in 4; in two other bundles, only the outer angle of the xylem wedge is stained, on one side or the other, *i. e.*, those vessels which frequently fill with bacteria in advance of the rest of the pitted vessels, when the plant suffers from cucumber wilt. Seventy-five cm. up.—Stain restricted to six bundles, and very slight; confined to the

spirals in three bundles and almost so in a fourth. Several nodes have been cut. The stain is deepest in these parts of the stem and more widespread in the walls of the pitted vessels. Ninety cm. up.—There is still a trace of stain in three bundles; in one it is restricted to the walls of the spiral vessels, in the other two it does not occur in the spirals but on one side of the xylem, midway out in one, and in the outer angle of the other. Some of the branches were also examined. The branch 65 cm. long, separated from the parent shoot only ten centimeters above the cut surface. At 20 cm. from its junction with the main shoot, 7 bundles showed a trace of stain in the xylem part; in two of these the stain was restricted to the spirals, and in the other five it seemed to have diffused outward from the spirals into the neighboring pitted vessels. Twenty-three cm. up.—Only slightest trace of stain, restricted to the spirals of one bundle. Twenty-five cm. up.—Not a trace of stain. The branch 105 cm. long, separated from the parent shoot 41 centimeters above the cut surface. It was first cut at the junction with the main stem. Here the stain was to be seen in 7 bundles, but very slight and almost wholly restricted to the spiral vessels. Thirty cm. from the junction, *i. e.*, past several nodes.—Stain in xylem of all of the nine bundles; restricted to the spirals in 6, diffused in 3. This cut was made just above a node. The stain appears to be more restricted to the spirals in the internodes than in the nodes. Vessels empty. Sixty cm. up.—Only the slightest trace of stain in one bundle, so slight as to be readily overlooked. Sixty-five cm. up.—Color still present but so slight that no one would recognize it unless informed that stain had passed through the stem. Seventy cm. up.—All trace of the stain has disappeared.

It will be remembered that this vine was in the 1 per cent eosine water nearly 24 hours, during which time there was no perceptible lowering of the liquid, consequently all of the internal stain is readily accounted for, especially when we remember the powerful tinctive character of eosine, by the few drops of stain which managed to get past the gelatine plugs. We are warranted, therefore, in concluding that not a drop

passed up through the walls of the vessels, or if this be too strong a statement we are at least safe in saying that not enough passed up the walls to serve even most inadequately, the transpiration purposes of a single leaf, and this, perhaps, is the better forms in which to leave the statement. Neither can it be said, by way of objection that the eosine behaved differently from ordinary water for we have already seen that 1 per cent eosine water passes up unplugged stems readily, even for days and long after the stem is dead. In this case, judging from the state of the atmosphere, the temperature, and the amount of transpiring surface, (approximately 1,500 sq. cm.) at least 25 and probably 50 cubic centimeters would have been taken up by the plant in the first 24 hours but for the gelatine plugs. No explanation is open, therefore, except that the transpiration water passes up through the lumen of the vessels in the stem of the cucumber, and presumably in all other stems of similar structure, unless we assume that the gelatine passed into the walls of the vessels and destroyed their conductive power, and no one has proved this to be possible or even set forth facts rendering it probable. Considering the fact that the walls of the vessels in most plants are solid lignified structures and that the vessels are long open tubes, comparable to water pipes and in many plants probably continuous through the whole length of the stem, it would seem strange that this other view, viz. that the water passes upward through the wall itself and not through the lumen of the tube, should have ever gained credence, did we not know how often, even in science, the weight of a great name carries everything before it.

(*To be Continued.*)

EXTENSIVE MIGRATION IN BIRDS AS A CHECK UPON THE PRODUCTION OF GEOGRAPHICAL VARIETIES.

By Thomas H. Montgomery, Jr.

Two problems have of late years received much attention from ornithologists, and deservedly, namely, the faunal distribution of species, and their ranges of migration. But to my knowledge, no one has raised the question of the possible existence of a relation between the extent of the periodic migration and the amount of geographical variation evinced by a species. The object of this paper then is to show that such a relation does exist, that extensive migration tends to act as a check upon the production of geographical varieties, or races so-called.

In the first place, on comparing the amount of faunal variation with the extent of the periodical migrations in a given species, it will be found to be usually, if not always, the case, that those species which undertake migrations of more than the average extent—migrating through 30° or more of latitude, have no tendency to give rise to geographical varieties. In order to see how far this law extends, and whether exceptions to it may be found, I have compared all the species of North American birds with regard to this relation existing between variation and range of migration, using Ridgway's excellent "Manual of N. A. Birds" as my authority for the amount of variation and extent of migration in these species. For our present purposes the North American species may be divided into three groups, based on the extent of their migrations: (1) species with exceedingly protracted migrations, but irregular as to the localities traversed; (2) species with more or less regular migrations, of 30° lat. or more in extent; and (3) species which undertake migrations less in extent than 30° lat., or species which do not migrate at all. We may now consider each of these groups in turn, with regard to the question at issue.

I. *Species with protracted but irregular migrations.*

Diomedeidæ.
Procellariidæ.
Phaethontidæ (?)
Fregatidæ (?)

II. *Species with a migration range of 30° lat. or more.*

Podicipidæ.
Urinatoridæ (all ?)
Stercorariidæ.
Laridæ (most).
Sulidæ (most ?).
Anatidæ.
Gruidæ.
Rallus virginianus.
Porzana.
Fulica americana.
Phalaropodidæ.
Recurvirostridæ.
Scolopacidæ (most).
Charadriidæ (most).
Aphrizidæ.
Columbidæ (most).
Cathartidæ (?).
Circus hudsonius.
Accipiter velox.
Falco (2 species).
Ceryle alcyon.
Sphyrapicus varius.
Micropodidæ.
Trochilus colubris.
Tyrannus (2 species).
Myiarchus crinitus.
Sayornis (2 species).
Contopus (3 species).
Empidonax (5 species).
Dolichonyx oryzivorus.
Icterus galbula.
Calcarius.
Zonotrichia (2 species).
Spizella (2 species).
Melospiza (2 species).
Habia ludoviciana.
Passerina cyanea.
Spiza americana.
Piranga (3 species).
Hirundinidæ.
Vireo (4 species).
Mniotiltidæ (most).
Motacillidæ (most).
Galeoscoptes.
Regulus (2 species).
Turdus (3 species).

Now the species enumerated in Lists I and II migrate periodically through an area of 30° lat., or more, that is, a migration range of considerable extent, and, with a few exceptions to be considered later, all are sharply defined species, and even though the breeding areas of most are very broad, none of them have a tendency to split into geographical varieties. Accordingly there must be be some relation existing between

the range of migration and the tendency to produce geographical races, for otherwise this coincidence could not be explained. So having found that those species undertaking long migrations do not, as a rule, tend to give rise to local varieties, we must conclude that the process of taking extensive migrations is a check upon the tendency to produce geographical varieties. But in order to round off further deductions, we must first determine whether species which do not migrate extensively have a greater tendency to geographical variation than those just considered; and this assumption will be strengthened by a comparison of the species in the following List III with those in Lists I and II.

III. *Species with a short or no migration range.*

Alcidæ.
Rhynchops.
Anhinga.
Phalacrocoracidæ (most).
Pelecanidæ.
Phœnicopterus.
Plataleidæ.
Ibididæ.
Ciconiidæ.
Ardeidæ (most).
Aramidæ.
Rallidæ (most).
Hæmatopodidæ.
Tetraonidæ.
Phasianidæ.
Cracidæ.
Falconidæ (most).
Strix pratincola.
Bubonidæ.
Psittacidæ.
Cuculidæ.
Trogonidæ.
Momotidæ.
Alcedinidæ (most).
Picidæ (most).
Caprimulgidæ (most).
Trochilidæ (most).
Cotingidæ.
Tyrannidæ (most).
Alaudidæ.
Corvidæ.
Icteridæ (most).
Fringillidæ (most).
{ *Euphonia.*
{ *Piranga* (most).
Ampelidæ.
Laniidæ.
Vireonidæ (most).
Cœrebidæ.
Mniotiltidæ (a few).
Cinclidæ.
Troglodytidæ (most).
Certhiidæ.
Paridæ.
Polioptila.
Turdidæ (most).

It is at once apparent that almost all the species of North American birds which are divisible into geographical varieties are classed in this third list, that is, that those species evincing the greatest tendency to geographical variation, are also those which undertake migrations of the least extent. Thus, for instance, *Melospiza fasciata* is usually resident in most localities throughout the whole year, and has become differentiated into a number of geographical races, while *Melospiza georgiana* is migratory, and though it breeds in an area nearly equal in extent to that of *fasciata*, has not produced local varieties; the non-migratory *Megascops asio* shows great geographical variation, while the migratory *Asio acciptrinus*, though almost cosmopolitan in its breeding area, shows no tendency toward such variation. And, in fact, an examination and comparison of List III with Lists I and II, will lead to the conclusion, that given any two species of equally extensive breeding areas, the one with the smaller range of periodic migration will, as a rule, evince a greater tendency to produce geographical varieties than will the species with the greater range of migration. This conclusion may be concisely formulated as follows; *it is the rule that the amount of geographical variation in species with more or less extensive breeding areas, stands in inverse ratio to the extent of its periodic migrations.* Naturally, this law is only applicable to species with extended breeding areas, since diverse conditions in different sections of this area are necessary, according to the theory of Natural Selection, for the production of geographical subspecies or varieties; and in a limited breeding area, throughout which the conditions of the environment are similar, there could be no cause to produce geographical varieties, irrespective of the migratory or non-migratory habits of the species.

I have not meant to imply, in the preceding pages, that species with migration ranges of 30° lat., or more, are all sharply definable, *i. e.*, that such species are never divisible into geographical varieties; but, on the contrary, that this tendency to produce geographical races is less in the species with extensive migrations, than in those with shorter ranges of migration. For it is usual, even in species with extensive migrations, whose

breeding areas are extraordinarily great, so as to include the whole of the arctic region, or northern America together with northern Eurasia, for them to subdivide into two geographical varieties, occupying respectively the eastern and western hemispheres. Thus, the eurasiatic *Colymbus nigricollis* is represented by a variety (*californicus*) in western North America; and to give other examples where an eurasiatic form, which undertakes long periodic migrations, is represented by a geographical variety in North America, may be mentioned one species of *Fratercula*, 1 *Uria*, 1 *Larus*, 1 *Hydrochelidon*, 2 *Aythya*, 1 *Glaucionetta*, 1 *Somateria*, 1 *Anser*, 1 *Tringa*, 1 *Limosa*, 1 *Charadrius*, 2 *Falco*, 1 *Pandion*, and others. But no species with extensive migration ranges shows any tendency to geographical variation, unless its breeding areas are also very large in extent. And the species with the least demonstrable tendency to produce local races, are those in which the wing power is greater, and the range of migration more extensive, than in any other species of birds, namely, those enumerated in List I. Further, we find it to be the rule, that in those avian families most of the species of which undertake long migrations, if species are present which are divisible into geographical varieties, that these latter are more restricted in their migrations than the former; examples are *Uria troile*, *Rissa tridactyla*, *Fulmarus glacialis* (only North American species of the family presenting geographical varieties), *Rallus longirostrus*, *Porzana jamaicensis*, *Aegialitis wilsonia* and *Ae. meloda*, and others. After the consideration of these facts it is certainly permissible to conclude that, as a rule, species which undertake annual migrations of comparatively great extent, through distances of 30° lat., or more, evince no tendency to give rise to geographical varieties, unless their breeding areas are very extensive; and, conversely, that species which do not undertake extensive migrations, owing to insufficient wing power or to some other cause, and which occupy broad breeding areas, have the tendency to produce geographical varieties. Consequently, also, extended migration acts as a check upon the production of varieties; and the extent of the range of migration will, therefore, stand in inverse ratio to the amount of geographical variation

evinced. Thus the postulate of Darwin, that wide-ranging species vary most, must be modified after a consideration of the facts given here. But to pass over to certain apparent exceptions to the rule. *Falco columbarius*, breeding chiefly north of the United States, and migrating in winter as far as South America, has a variety (*suckleyi*) on the Pacific coast from Sitka to California; *Helminthophila ruficapilla*, breeding as far north as Hudson's Bay, and migrating in winter as far as Guatemala, has a variety (*gutturalis*) from the Rocky Mts. to the Pacific coast, in winter to Mexico; and a number of similar cases could be mentioned, where the species, although it has a wide range of migration, and a breeding area which is not extraordinarily extensive, has, nevertheless, the tendency to geographical variation. But such apparent exceptions to the rule are, in fact, not valid objections, since in these cases the geographical variety is much more restricted in the range of its migration than the type species, or *vice versa*. And in any of these cases, the species, including the variety, is to be regarded as a number of individuals, some of which undertake extensive migrations, while others migrate not at all or through much shorter distances. Therefore, these are not true exceptions to the law, that the extent of the migration stands in inverse ratio to the amount of the tendency to produce geographical varieties; since a number of the individuals do not undertake extensive migrations. Real exceptions may, however, be found in such cases where the individuals of the type species as well as its varieties make prolonged periodic migrations; and after a careful examination of all the North American species and their varieties, I have found only four species which represent such exceptions to the rule: *Dendroica æstiva*, with its variety *morcomii*, *Sciurus noveboracensis*, with the subspecies *notabilis*, *Sylvania pusilla*, with the variety *pileolata*, and *Turdus ustulatus*, with its eastern variety *swainsonii*. These four species represent cases where, with not very extensive breeding area, both races of the species possess extensive migration ranges. But I think that the importance of these cases as exceptions to the rule is diminished, when we consider that in each case the migration route of the variety is different from

that of the species, one being west of, while the other is east of the Rocky Mts. And hence, since not only in its breeding area but also in its migration range, the variety is subjected to conditions of environment different from those influencing the type species, we would naturally expect that the species (as a whole) would become differentiated into two geographical races.

The reason for the law, that extensive migration acts as a check upon the production of geographical varieties, is not far to seek. The barn swallow, for instance, remains in its breeding area from four to five months each year, spending the remainder of its time, except that consumed by its actual migration to and fro, in its tropical winter quarters. Roughly speaking, we may say that it spends about half a year in its breeding area, and the remainder in its winter home. In other words, the swallow is subjected to one environment for half the time of its existence, and to a more or less different environment during the remainder of its life. The result of this on the organism is obvious: the action of the two environments during approximately the same length of time, would prevent it from becoming more particularly adapted to the one than to the other, and would lead to the production of more generalized characters, fitted to respond more or less equally to both environments. In this way individuals of the species could not become especially adapted to a certain portion of the breeding area, if such adaptations should be unfavorable for its existence in the winter quarters, and *vice versa*; in other words, the influence of the winter environment acts as a check upon the acquisition of adaptations suited alone to the summer environment. This is, to my mind, the only adequate explanation for the law that extensive migration exerts a check upon the production of geographical varieties. Species with wide-ranging breeding areas, on the other hand, but with none or only restricted migrations, may give rise to geographical varieties, suited respectively to the diverse conditions found in different portions of its habitat, since such species are influenced by the conditions of but one environment, owing to the absence or restriction of migration.

11 Sept., 1895.
West Chester, Penna.

THE PLANT-GEOGRAPHY OF GERMANY.

By Roscoe Pound.

In a recently published address Dr. Coulter speaks of a new movement in botany which is sending botanists "to the great laboratory of nature," and replacing collecting trips by biological surveys. "The old-fashioned collection of plants," he says, "will hold no more relation to the new field work than the old geology, with its scattered collection of fossils, holds to the topographic geology of to-day." Geographical botany as it is now understood is comparatively a recent development. Collectors and cataloguers for a long time have been gathering a portion of the bare facts upon which geographical botany must proceed, and the facts of plant-distribution have been more or less ascertained. But the systematic collating and grouping of these facts and the application of biological and physiological facts to them is a matter of the last few years and is still going on. At first localities were catalogued, and collectors were eager to add new and rare stations to those recorded for species; then came statistical comparison of families and genera, especially in relation to altitude and the media of plant-migration. The limits of distribution of species were ascertained, particularly of those which are characteristic and controlling in vegetation. Such work laid the foundations of geographical botany.

But the statistics as to the distribution of families, which have been worked out in one method and another, gave no promise of leading to important results. It was not until biological groups began to be made for the purpose of comparison, and statistics began to be applied to those groups, that such work acquired importance. It is apparent that a mere statement of the number of species of the various natural plant-groups occurring in a certain region tells us very little of the vegetation of that region except in the most general way. A group represented by comparatively few species may yet as far as the occupation of the soil is concerned be dominant and

controlling. To understand the vegetation of a region one must ascertain not only what are its physical, meteorological and geological features, but much more what sorts of plants control its water, meadow, plain, or forest vegetation. Directed towards the latter ends, statistics have a very different meaning. Such work is the aim of the new geographical botany. "When we hear of a district," say Schrœter and Stebler, "that it is covered with extended fields of turf-rush or of bromegrass, that tells us more of the nature of the region than long lists of meteorological data. It also tells us more than the mere occurrence of the species in question of itself"

A notable contribution to this department of the science is Dr. Drude's new work, "Deutschlands Pflanzengeographie," of which the first part appeared in January last. The sub title of the work gives a clue to its purpose. It is stated to be "ein geographisches Charakterbild der Flora von Deutschland." Much has been done in recent years towards such characterization of restricted districts, or for large areas as regards certain kinds of vegetation. But Dr. Drude in giving a complete picture of the vegetation of as large a country as Germany has, in one sense, made an epoch in geographical botany. Such a work demonstrates that the era of preparation is passed. A mere cursory examination of the work serves to convince the reader that the theory and system of plant-geography have been thoroughly worked out, and that henceforth workers will be busied chiefly with their application to other regions rather than with devising new methods.

As has been remarked, in order to be of value, statistics must be based not upon the systematic groups of plants but upon groups founded on biological considerations, so far as they indicate a positive role in the vegetation of the region in question. Such groups are called vegetation-groups. Dr. Drude points out also that the proportions of the number of representatives of the several orders, genera, or other systematic groups are not to be reckoned with the whole flora of a region as represented by a certain number of species, but with the biological plant-community of the region. Accordingly he constructs some thirty-five vegetation-groups for the flora of

Germany. The thoroughness of this may be judged from the fact that he begins with trees and ends with plankton-algæ.

Germany belongs to the Middle-European region which, bounded by the Pyrenees, the Alps, and the Balkan system, stretches along the northwest border of the Russian steppes to the arctic flora which extends over the north of Europe. The region includes also the wooded portions of the Scandinavian countries. Throughout this large region, as regards the distribution of families and genera, the same fundamental character prevails. Carrying the principles of division further, and observing on the one hand lesser influences of climate and physiognomy and on the other the division of the floral-elements into " Genossenschaften," subdivisions, or "Vegetationsregionen" are made. Germany and the neighboring regions of the Alps and Carpathians fall into five such divisions; the region of the north-Atlantic lowlands, the region of the south-Baltic lowlands and uplands, the region of the middle and south German highlands and lower mountain districts, the region of the higher mountain districts and subalpine formations, and the region of the higher mountain formations of the Alps and Carpathians. The region of central France and the west-Pontic region, to which belong the southwestern and southeastern neighbors of Germany respectively, include also isolated spots in Germany itself. Dr. Drude's maps show that the first two regions are continuous in extent. The first includes Holland and North Germany west of the Elbe and the western portion of the Danish peninsula, the second East-Prussia and Pomerania, being bounded roughly by the Oder on the west. Between the Elbe and the Oder is a neutral zone, transitional between the two regions. The whole of middle and south Germany to the Alps constitutes the third region. But along the northern borders of the Alps and here and there throughout south Germany, as for instance the Harz forest, the Thuringian forest, the Black forest, in isolated spots, we find the fourth region, the region of subalpine forests. Along the upper Rhine here and there are localities belonging to the region of central France, and in the southeastern portion are many localities belonging to the west-Pontic region.

But geographical botany today does not stop with the distribution of the wild flora. Cultivated plants, native useful plants, weeds, and the flora of waste places come in for their share of consideration and are treated in turn. The plants whose seeds are mixed with those of cultivated plants and are thus sowed and grown involuntarily are placed in the group of cultivated plants. But a more important group is formed by the species introduced and supported incidentally by the cultivation and occupation of the soil by man. A notable instance of this is a group of "saltpetre plants" due to the use of nitrate fertilizers.

It would become tedious to enumerate the many striking features of the work and the ideas which they suggest. The work is in some sort a summary of geographical botany as it now stands. So much material necessarily takes on a new aspect when brought together and digested, though we have been more or less acquainted with a large part of it in its scattered condition. As part of a whole, each fact seems something new. We may safely predict that a great impetus will be given to this kind of botanical work in regions remote from Germany by Dr. Drude's book, since it presents a practical outline which will not fail to be taken advantage of. Our own country furnishes many excellent opportunities which the various biological and botanical surveys now in progress are already beginning to seize. The example of such a geographico-botanical survey of a large country, on a large scale, will be a great inspiration.

Dr. Drude's book is most interesting reading, and as a compendium of the latest results in a growing and important department, as well as in its more immediate purpose, is of the highest value.

EDITOR'S TABLE.

THE bestiarians are still actively engaged in endeavoring to prevent humanitarians from prosecuting their good work of relieving human disease and suffering. Their latest move is to endeavor to get national legislation to suppress physiologic research by vivisection in the Dis-

trict of Columbia. There are various reasons why humanitarians should take especial pains to prevent this attempt to restrict human knowledge and prevent the dimunition of human suffering. They suppose that National legislation once secured, State legislation will be easily obtained. Perhaps they expect to get a national law forbidding such research in all parts of the United States! Such people must, however, present very clean hands in the cause of prevention of cruelty to animals before they appear as advocates of the suppression of the most important method known of reducing human suffering. Do any of them wear articles made from the furs of animals? Do they carry pocket-books or grip-sacks made of the skins of animals? Do they permit animals to be plucked of feathers for their comfort or ornament? Finally, do they encourage the enormous slaughter of animals by land and sea, for food and other purposes?

There is much important work done in the departments at Washington which will be affected by the bill that is soon likely to come before the Senate, and the educational institutions of the highest grade will be injured by it if it passes.

The bill it is said will be favorably reported to the Senate. It will, however, probably not come up for final action before the next session. Meanwhile biologists and humanitarians generally should urge on their Senators and Representatives the importance of defeating the bill in the interest of progress and humanity. Let them write to their Representatives for the Public Documents on Antivivisection of the District Committee of the Senate. The Medical men are active, but the biologists are not yet sufficiently awake to the importance of the situation. If members of the National legislature are fully informed, they will hardly pass the bill.

RECENT LITERATURE.

The Cambridge Natural History.[1]—Sometime ago we referred to the volume of this series containing the Molluscs and Brachiopods; the second volume in order of publication is now before us. As in the former volume there is a great lack of uniformity in the different parts

[1] The Cambridge Natural History, Vol. V. Peripatus by Adam Sedgwick; Myriapods by F. G. Sinclair; Insects, Part I by David Sharp. London, Macmillan and Co., 1895, pp. xi–584.

which compose it, a lack, in part attributable to the individuality of the authors, in part to an apparent failure on the part of the editors to lay down guiding rules for their authors.

Mr. Sedgwick devotes 26 pages to Peripatus, giving a good general account of the group, in its structure, development and habits, and following it with a list of the known species, essentially the same as that in his previous monograph. From his familiarity with the group no one was better able to treat of the group than he.

Mr. Sinclair should have been almost equally familiar with the Myriapods for he has published both on the structure and the embryology of the group, and yet his account is much less satisfactory. The general account of the habits is good and is based to a large extent upon the author's own observations, but we wish he had put into English some of the facts ascertained by vom Rath. The classification adopted, that of Koch, is rather antiquated (1847) while the investigations of Grassi, to say nothing of the later researches of Schmidt and Kenyon, show that the Scolopendrellidæ and Pauropidæ are not to be set aside as distinct from the Diplopoda, and the elevation of Cermatia to ordinal rank has very little in its support. One or two typographical errors are annoying. Scudder's figures of fossil Myriapods are attributed to "Meek and Worth," the author persisting in depriving the American paleontologist of the last syllable of his name. Here may be mentioned one of the inequalities of the work. While in treating of Peripatus a diagnosis is given of all (?) known species, in the Myriapods only the families are thus treated. Concluding the account is a discussion of the relationships of the group, and in this we find mixed up myths from Pliny and facts from other authors, including (p. 78) a quotation showing that the people of Rhytium were driven from their quarters by Myriapods, a statement which also occurs (p. 30) in another place. But in this whole part we see nothing but a feeble groping, not the firmness of the master hand. The chapter as a whole shows the lack of editorial supervision; its prolixity on minor points should have been suppressed.

The best of the book is that by Mr. Sharp—accounts of the Aptera, Orthoptera, Neuroptera and the lower Hymnoptera, the author using these names in the widest sense. In the introductory sections, dealing with the anatomy and embryology of the Hexapods, the author is evidently less at his ease that in the more systematic portion. Here he has given us one of the best of all books upon insects. The strictly systematic portion is well done, while the account of habits and transformation is excellent, and the perspective good. Thus the Mallophaga

are accorded 6 pages, the White Ants, 44. On the whole we like the retention of the almost Linnean system of classification, especially since the systems which are proposed in its place are open to almost as many objections as the older scheme; the remarks made upon this point seem to us especially appropriate.

The illustrations, of which there are some 370, are all fresh and are very well engraved. Some of them would, we think, look better in "half-tone," especially those dealing with anatomical and developmental points, but against this is the apparent inability of English printers to get good results from such plates, (witness several translations from the German where these half-tone illustrations, beautifully printed in the original, are extremely muddy). One more fault and we are done. The price charged for the work seems to us much too high.

W. Fraser Rae's biography of Richard Brinsley Sheridan, that remarkable man "who could rival Congreve in comedy and Pitt and Fox in eloquence" is announced by Messrs. Henry Holt & Co. It is to be in two volumes, and to include portraits and facsimile autographs of Sheridan and his famous contemporaries. Interesting documents written by the Prince of Wales, Sheridan, the Duke of Wellington, and the Marquis of Wellesley will be made public for the first time. The Introduction is by the Marquis of Dufferin and Ava, who is a great grandson of Sheridan.

Geological Biology.[1]—This treatise, in octavo form of 395 pages, is a study of organisms and their time-relations. The general laws of evolution are stated, and their formulation explained by detailed descriptions of characteristic examples. The examples are, for the most part, taken from the invertebrate forms. Mutability of species is illustrated by *Spirifer strictus* Martin, var. *S. loganii* Hall, the progressive evolution of class, ordinal, subordinal, etc., characters, by *Magellania flavescens*; the modification of generic characters is shown by the life-histories of Brachiopod families. The history of the Spirifers, a study of Cephalopods, and the evolution of the suture lines of Ammonoids, are each in turn used to demonstrate the fundamental laws of evolution. Throughout the book the author emphasizes the idea that these laws are best understood by a study of fossil forms.

The closing chapter sets forth the philosophy of evolution from the author's point of view. Beginning with the statement that "Evolu-

[1] Geological Biology. An Introduction to the Geological History of Organisms. By Henry Shaler Williams. New York, 1895. Henry Holt & Co.

tion is concerned with two distinct fields of human inquiry," he distinguishes them as follows:

"On the one hand, *evolution* is the name for *the natural order of unfolding of the characters of organic beings that have lived* on the earth; on the other hand, evolution is the name for our conception of *the mode of operation* of the *fundamental energy of the universe.* Thus it will be seen that the notion of God is as intimately involved in a discussion of evolution as is the notion of an organism." He sees in evolution the mode of creation of organic beings, a process that has been more or less continuous throughout geologic ages. "It is this continuation of the process of phenomenalizing that distinguishes the mode of creation in the organic realm from that in the lower realm of inorganic matter. Whatever is characteristic of organisms was not created at once, but has been unfolded by degrees, and there is no reason for supposing that the process is not still going on. Such expressions as 'effort,' 'growth force,' 'reactions,' etc., used in describing the phenomena of evolution, all express the notion of the preëxistence of some unphenomenal property, or power, or potency, which constitutes the cause of the particular characters which are acquired by organisms in the process of their evolution."

The tendency of organisms to vary is designated by the author as primarily a force acting from within, to which he gives the name "*intrinsic evolution.*" Differentiation of form and function are expressions of vitality, but these are modified by conditions of environment and natural selection.

A summary of the leading points in the work are thus given:

"The great facts attested by geology are that the grander and more radical divergences of structure were earliest attained; that, as time advanced, in each line intrinsic evolution has been confined to the acquirement of less and less important characters; such facts emphasize with overwhelming force the conclusion that the march of the evolution has been the expression of a general law of organic nature, in which events have occured in regular order, with a beginning, a normal order of succession, a limit to each stage, and in which the whole organic kingdom has been mutually correlated."

This book will prove instructive to the general reader, both on account of its facts and generalizations. The author, as a distinguished specialist in paleontology presents facts in an authoritative way, so that the reader may feel safe in his premises. The inferences made are obvious, so that while there is little exposition of efficient causes of evolution in the scientific sense, one can agree with the general conclusions.

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General Notes.

PETROGRAPHY.[1]

Malignite, a New Family of Rocks.—Lawson[2] uses the name malignite for a family of basic orthoclase rocks constituting an intrusive mass, possibly laccolitic, in the schists around Poohbah Lake, in the Rainy River district, Ontario. Three phases of the intrusive mass are recognized—a nepheline-pyroxene-malignite, a garnet-pyroxene-malignite and an amphibole-malignite. The constituents common to all phases are orthoclase, aegerine-augite and apatite. In the nepheline variety the nepheline occurs as patches in the orthoclase, or as micropegmatitic intergrowths with it. The orthoclase is in poikilitic relations with all the other minerals, surrounding them like the glass

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Bull. Dept. of Geol. Univ. of California, Vol. I, p, 337.

in a partially crystallized lava. It was evidently the last component to solidify. The composition of the rock is as follows:

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	Na_2O	K_2O	H_2O	P_2O_5	Total
47.85	13.24	2.74	2.65	14.36	5.68	3.72	5.25	2.74	2.42 =	100.65

This composition is so similar to that of the Vesuvian leucitophyres, that the rock is regarded as the plutonic equivalent of these lavas. The low percentage of silica and the high lime percentage separate the rock from the eleolite-syenites.

One form of the nepheline-malignite is panidiomorphic through the development of the orthoclase and all the other components in crystals. In the garnet-pyroxene phase of the rock the orthoclose is intergrown with albite in the form of phenocrsyts imbedded in a hypidiomorphic aggregate of aegerine-augite, melanite, biotite, titanite and apatite. The augite, melanite and biotite are allotriomorphic. They seem to have crystallized contemporaneously with each other, and with a part of the orthoclase. In the amphibole-malignite the distinguishing characteristic is the prevalence of a very strongly pleochroic amphibole, and the absence of any large quantity of aegerine. The augite that is present occurs intergrown with the amphibole. Melanite is wanting, otherwise this rock is very much like the mellanite-pyroxene malignite.

The author points out the fact that the great mineralogical differences observed in the three types of malignite, are accompanied by very slight differences in chemical composition. The three types are regarded as differentiation phases of the same rock mass.

Foliated Gabbros from the Alps.—Schäfer[3] gives an account of the olivine gabbro and its dynamically metamorphosed forms which constitute the rocks of the region in the vicinity of the Allalin glacier between the Zermatthal and the Saarthal in the Alps. The normal gabbro contains in its freshest forms much or little olivine. In its altered forms it consists of saussurite, amphibole, talc, actinolite and garnet. Ottrelite is often found enclosed in the talc and sometimes imbedded in the saussurite. In one of the granular varieties of the metamorphosed gabbro a blue amphibole is very abundant. It is intergrown in part with omphacite. The granular alteration forms of the gabbro pass gradually into foliated forms and through these into rocks called by the author "green schists." The schistose gabbros are mineralogically similar to the granular alteration phases of the rock, except that they contain in addition to the minerals named above a

[3] Neues Jahrb. f. Min., etc.. B.B., p. 91.

newly formed albite, zoisite and white mica. The final stage of the alteration is a zoisite-amphibole rock. The green schists are composed of ellipsoids of zoisite, feldspar and epidote imbedded in a schistose green amphibole clinochlor aggregate. Some of the schists are rich in garnets, and others are practically chlorite-schists. All are supposed to be derived from the gabbro.

In addition to the gabbros there are also in the region several exposures of serpentine whose contact with the green schists with which they are associated are always sharp. The original form of the rock is unknown, but it is supposed to have been a peridotite. Its most interesting feature is the possession of light yellow and brown crystals of some member of the humite family.

On the west side of the Matterhorn the author also found normal and olivine gabbros, both more or less altered. The former is cut by little veins of aplite. The peak of the Matterhorn is scarred by numerous fulgurites. Its rocks are fine grained green schists, some of which are like those described above, while others are dense and homogeneous in appearance. They consist of amphibole, clinochlor, zoisite, altered plagioclase, talc and alkali-mica. These rocks are defined as zoisite-amphibolites.

The Rocks of Glacier Bay, Alaska.—Cushing[4] gives a few additional notes on the petrography of the boulders and rocks of Glacier Bay, Alaska. The principal rocks of the region are diorites, altered argillites and limestones that are cut by dykes of igneous rocks. In addition to the diorites and quartz-diorites reported by Williams[5] from this vicinity, there are also in the region mica and actinolite-schists. The dyke rocks are mainly diabases. The author gives some additional information concerning the diorites and briefly describes the schists. The actinolite schists are aggregates of finely fibrous actinolite needles, in whose interpaces is a granular mixture of quartz and epidote and an occasional grain of plagioclase. The mica schists present no unusual features except that some of them are staurolitic.

Petrographical Notes.—As long ago as 1836 Thomson reported the occurrence of light yellowish-green rounded masses which he called huronite, imbedded porphyritically in a boulder of diabase from Drummond Island. Other occurrences of the same substance have been found by the Canadian geologist in diabase dykes cutting the rocks of the Lake Huron region. These have been investigated by Barlow[6]

[4] Trans. N. Y. Acad. Sci., Vol. XV, p. 24.

[5] Cf. AMERICAN NATURALIST, 1892, p. 698.

[6] Ottawa Naturalist, Vol. IX, p. 25.

and are pronounced by him to be aggregates of zoisite, epidote, sericite and chlorite in a mass of basic plagioclase. In other words, huronite is a saussuritized plagioclase. Descriptions of a number of dyke rocks containing 'huronite' are given by this author.

Bauer[7] describes a number of specimens of snow-white, lilac and emerald-green jadeite from Thibet and upper Burmah. One of the green varieties is cut by little veins of nepheline, containing plates of basic plagioclase and little bundles of a monoclinic augite (jadeite) with the same properities as that which constitutes the mass of the jadeite. The rock, according to the author, is made up of this augite and nepheline, the latter mineral acting as a groundmass. The veins are those portions of the rock in which the augite is in very small quantity. In other specimens nepheline occurs in small quantity, and plagioclase is abundant. His conclusion is that the rock is a jadeite-plagioclose-nepheline rock in which locally the one or the other component is most prominent. If the rock is, as the author supposes, a crystalline schist, the occurrence of nepheline in it is of extreme interest.

In a second article the same author[8] describes a serpentine from the jadeite mines at Tauman. It is composed of olivine, picrolite, chrysotite, webskyite and a few other accessories in an albite-hornblende matrix, consisting of an aggregate of single individuals of untwinned albite, in the midst of which lie brown and gray hornblendes surrounded by zones of a bright green variety of the same mineral. Between this zone and the albite there is a fringe of green augite needles. The rocks associated with the jadeite and the serpentine are also described. Among them is a glaucophane-hornblende-schist. All the rocks exhibit the effects of pressure.

In a very short note Beck[9] calls attention to the fact that the molecular volume of dynamically metamorphosed rocks, i. e., of the minerals composing these rocks—is less than that of the original rocks from which they are derived. For instance, a mixture of plagioclase, orthoclase and water in the proportion to form albite, zoisite, muscovite and quartz has a molecular volume of 547.1, while the corresponding mixture of albite, zoisite, etc., has a volume of 462.5,

[7] Neues Jahrb. f. Min., etc., 1896, I, p. 85.

[8] Record of Geol. Survey of India, XXVIII, 3, 1895, p. 91.

[9] Kais. Ak. Wiss. in Wien. Math. Naturw. Class, Jan., 1896.

GEOLOGY AND PALEONTOLOGY.

Phylogeny of the Dipnoi.—In a memoir recently published, M. Dollo adduces fresh evidence for the theory recently advanced by various English scientists that the diphycercy among the Dipnoi is in reality a secondary diphycercy or gephyrocercy. The author regards this as an important fact, and uses it as a basis in developing his theory of the origin and evolution of the order. The results of his researches are as follows:

I. *Dipterus valenciennesii* is the most primitive of the Dipnoi known.

II. In a general way, the evolution of Dipnoi, since the lower Devonian, is represented in the following series in the order of the enumeration of its terms.

Dipterus valenciennesii—Dipterus macropterus—Scaumenacia—Phaneropleuron — Uronemus — Ctenodus— Ceratodus—Protopterus—Lepidosiren.

III. The origin of the Dipnoi must be looked for among the Crossopterygia.

IV. The Batrachians are not in the line of the Dipnoi.

V. The specialization of the Dipnoi has been from a pisciform type toward an anguilliform type.

This order is a terminal group, derived from the stem that gave origin to the Batrachians.

In conclusion the author gives the phylogeny of the gnathostome vertebrates in a tabulated form. (Bull. Soc. Belge de Geol. T. ix, Fasc. 1, 1895, Bruxelles, 1896).

Fauna of the Knoxville Beds.[1]—The Knoxville beds are a Cretaceous series confined to the coast ranges of California, Oregon and Washington. They are characterized by the great abundance of Aucella, usually without associates, but, through the explorations of Mr. Diller and other geologists, a rich and varied invertebrate fauna has been discovered in the Aucella-bearing series of the Pacific States.

The description of this fauna was assigned to T. W. Stanton, and is now published as Bulletin No. 133 of the U. S. Geological Survey. The author recognizes 77 distinct species and varieties, of which 50 are new. All but 7 of the species are Mollusca, including 33 species of

[1] Bulletin United States Geological Survey, No. 113. Contributions to the Cretaceous Paleontology of the Pacific Coast; The Fauna of the Knoxville Beds. By T. W. Stanton. Washington, 1895, [issued Feb. 3, 1896].

Pelecypoda, 1 of Scaphopoda, 18 of Gastropoda, 18 of Cephalopoda (15 Ammonoids, 3 Belemnites). The other 7 species include 5 Brachiopoda and 2 Echinodermata.

The brief introduction comprises a geological description of the beds, with a discussion of their age, and of their relations to various other formations characterized by a similar fauna.

The new species are figured on twenty page plates.

Notes on the Fossil Mammalia of Europe, IV.—ON THE PSEUDOEQUINES OF THE UPPER EOCENE OF FRANCE.—Under the term Pseudoequines may be included the various species of the genus *Paloplotherium*, which occur in the Upper Eocene and Oligocene of Europe. This phylum parallels in a remarkable manner many of the characters which are typical of the true horses, but these characters, strange to say, are much earlier differentiated than in the real equine phylum.

Kowalevsky[2] in his great work on "*Anthracotherium*" clearly recognizes in his phylogenetic table of the Ungulates, that *Paloplotherium* is not in the direct line of the horses. Schlosser is also of the same opinion as Kowalevsky in regard to the relations of *Paloplotherium* to the horses. Professor Gaudry[3] as late as 1888 placed all the species of *Paloplotherium* in the direct line leading to *Equus*.

The earliest known species referred to *Paloplotherium* is the *P. codiciense* Gaudry; this form is from the Calcaire Grossier or Middle Eocene. In *P. codiciense* there are four upper and lower premolars, whereas in the more typical species of *Paloplotherium* from later deposits there are only three premolars. Moreover, in the *P. codiciense* all the upper premolars are simpler in structure than the true molars. The last upper premolar in this species is tritubucular in structure, and there are two well defined crests running outwards from the deuterocone, in other words this tooth is well adapted for further evolution into the molariform last premolar of the typical Paloplotheroids. In the true molars of *P. codiciense* the ectoloph has nearly the same form as that of the Palaeotheridæ in general, the metaloph or posterior crest, however, is less oblique in position than in the later species of *Paloplotherium* and *Palæotherium*. The type specimen of *Paloplotherium codiciense* consists of a facial portion of a skull with the teeth well preserved. This species is much larger than *P. minus* and corresponds more nearly in size with *P. annectens*.

[2] Monographie der Gattung Anthracotherium, p. 152.

[3] Les Ancêtres de nos Animaux, Paris, 1888.

In "La Petite Galerie" of Paleontology at the Jardin des Plantes, Paris, there is a well preserved skeleton of *Paloplotherium* which is labelled *Paloplotherium minus.* This specimen is of some importance, as we have here a case where the skeletal parts and teeth are associated and from the same individual. This skeleton was found in the Calcaire Grossier de Dampleix, Département of Aisne,[4] and it is important to add that Professor Gaudry's type of *P. codiciense* is from the same horizon of this Département, but from another locality.

Although this skeleton has been determined as *P. minus* it differs widely from this species, and also in fact from *P. codiciense.* In the upper and lower jaw there are four premolars as in *P. codiciense,* but these teeth are much more complicated than in that species and are exactly transitional in structure between *P. codiciense* and the typical species of *Palæotherium.* The premolars in this type are badly worn, but I can distinctly trace on one side that the internal cones are nearly double, but not distinctly separated as in *Palæotherium;* the structure the true molars is like that of *P. codiciense.* As will be seen the dentition of the skeleton above referred to differs from *Paloplotherium minus* in having four premolars, also the second upper tooth of this series is more complex than in the latter.

The parts of the skeleton associated with the teeth and skull consist of a scapula, radius and ulna, and also some metapodials. Among the latter there is a Mc. III and also another metacarpal, which I think may be Mc. V. If this determination be correct, we have here a Palæotheroid with four digits to the mamus. In the species of *Palæorium* from the Upper Eocene, Mc. V is represented only by a rudiment. I would like to add that the metapodial in this skeleton which I have determined as Mc. III is flatter and less triangular in section than in the typical *Paloplotherium minus;* this goes to show that the lateral toes were larger, and supports my view as to the presence of four anterior digits in this as yet undescribed species.

From the characters above adduced I conclude that this new species of Palæotheroid is more closely related to the true *Palæotherium* than to *Paloplotherium,* and moreover it is the most primitive form of *Palæotherium* yet discovered.

I am not able to learn that the beds in which this skeleton was discovered are any later than those in which the *P. codiciense* was found. However, from the structure of the premolars in the two species, I would conclude that *P. codiciense* came from an earlier subdivision of

[4] I am much indebted to my friend M. Marcellin Boule of the Jardin des Plantes for having given me information in relation to this specimen.

the Calcaire Grossier of Aisne than that in which this skeleton was found.

The most abundant species of *Paloplotherium* found in France is the *P. minus*. This species was described by Cuvier and referred to the genus *Palæotherium,* but it was later raised to a generic rank by Owen, and also by Pomel. In regard to *Paloplotherium minus* it is of importance to attempt to show that the teeth and feet of this species are properly associated. Osborn and Wortman[5] have lately questioned the correctness of this association, and furthermore these authors think it probable that the feet referred to *P. minus* by the French Paleontologists really belong to a small species of Lophiodont-like animal, closely related to the American genus *Colodon*. I cannot agree at all with these authors in this supposition, as I believe that the feet tending to monodactylism found in the Upper Eocene of France, which are referred by the French Paleontologist to *Paloplotherium,* are correctly identified.

Among the large collection of fossils in the Jardin des Plantes, many of which formed the types of Cuvier, and which were described by him in his "Ossemenes Fossiles," there is a nearly complete skeleton referred by Cuvier to *Paloplotherium minus;* this is figured by Cuvier[6] and also by Blainville.[7] In this specimen the feet are absent, but there are a few teeth embedded in the skeleton which have the same structure and size as those referred to *P. minus*. Again, Blainville figures an anterior extremity of a small Perissodactyles which he refers to *Paloplotherium minus*, and this specimen is of the same size as the fore limb of the nearly complete skeleton of *P. minus* described by Cuvier. Both these specimens are from the Gypse de Paris. However, since the time of Cuvier, *Paloplotherium minus* has been found in great abundance in the Upper Eocene of Débruge. The collection in the Jardin des Plantes from Débruge contains a large number of jaws and teeth, and portions of limbs containing numerous metapodials. These bones correspond exactly in size with those of the original skeleton described by Cuvier, and I am of the opinion that this is pretty conclusive evidence that the skeletal parts of *Paloplotherium minus* and the teeth are correctly associated. Moreover, I am not aware that any small Lophiodont Perissodactyle occurs in the Débruge Eocene. I use the term "Lophiodont" strictly in the sense as applied by Osborn and Wortman.

[5] Bulletin American Museum Natural History, 1895, p. 361.

[6] Ossemenes Fossiles, plate 115.

[7] Osteographie, Blainville, Palæotherium, plate VI.

Having now attempted to show that the monodactyle type of foot found in the Upper Eocene of France is in all probability correctly associated with the teeth of *Paloplotherium*, I shall review the characters of this phylum and indicate those points which parallel the true horses, and also point out those aberrant structures of the teeth which exclude the possibility of placing this series in the direct line leading to *Equus*.

Through the kindness of Professor Albert Gaudry, I have been enabled to study a beautifully preserved skull of *Paloplotherium javvalii* from the Phosphorites. This cranium is remarkably like that of the horse in many of its characters, and I think most Paleontologists would say at once that this type of horse-like skull should be associated with a foot tending to monodactylism. The position of the orbit is as in the primitive horses, its anterior termination being placed over the second true molar. The form of the facial region closely resembles that of the horse, being high and strongly compressed. The premaxillaries are elongated and slender, and slope gradually backwards as in the horse. Among the Palæotheroids, *P. crassum* has a skull resembling somewhat that of *Paloplotherium javalii*, but in the former the facial region is shorter and broader than in *P. javalii*. In the skull of *P. javalii*, there is a large flat area between the orbits, and the sagital crest is well marked. The post-orbital processes of the frontals are largely developed and extends well downwards towards the zygomatic arch. The post-tympanic and paroccipital processes are united as in *Palæotherium crassum*. The basal region of the skull in *P. javalii* is long and narrow, like that of the horse.

The structure of the skull in *Paloplotherium minus* is not known, only fragments of the occiput having been found. The teeth of *P. javalii* have been described by M. Filhol, and as is well known the crowns of the upper molars are much elongated and tending strongly to the hypsodont condition of *Equus*. Moreover, the valleys between the crests are filled with cements and the external and internal surfaces of the crown are coated with the same substance.

In *Paloplotherium* the last upper premolar is completely molariform and the posterior crest of this tooth, and that of the true molars is very oblique in position. The metaloph owing to its oblique position, only unites with the ectoloph after a long period of wear; this crest, in the true horses, moreover, is nearly at right angles to the ectoloph and unites early with the latter. In *Paloplotherium* also, the hypostyle—an element so essential in the evolution of the horse's molar, is absent. The lower true molars in the Paloplotheroids lack the reduplication of

the metaconid, which is present in the members of the true Equine phylum.

No metapodials of *P. javalii* have been found associated with the teeth; however there are in the collection of the Jardin des Plantes and also in the École des Mines, a number of enlarged third metacarpals and metatarsals from the same beds in which they find the teeth of *P. Javali*, and in all probability belong to this species. As already stated the horse-like skull and teeth of *Paloplotherium javalii* support the view that this type of cranium belong with these specialized metapodials. The third metacarpal in *P. javalii* is long and slender, and has a large facet for the unciform, the section of this bone is triangular with the lateral surfaces very oblique. This structure of the metapodial shows that the lateral digits were placed far to the inside and behind. The posterior cannon bone is more progressive in its horse-like character than the anterior, the proximal surface is much expanded transversely and the postero-lateral cavities for the metapodials are placed further behind than in the fore foot.

M. Filhol has described remains of *Paloplotherium minus* from the Oligocene of Ronzon, and in these beds they again find the enlarged third metapodials which are so abundant in the Débruge Eocene. This is another proof that the teeth and podial elements in *Paloplotherium* are properly referred.

A form closely related to the Palæotheridæ is the genus *Anchilophus*. This genus is more normal in its tooth structure in comparison with the early horses than *Paloplotherium*, and is considered by some authors[8] as in the direct line leading to *Equus*. Kowalevsky[9] however, calls *Anchilophus* a "Versuchgenus in der Pferderichtung, der Versuch war aber erfolglos, und der Anchilophus erlischt im Eocän, ohne directe Nachfolger zu hinterlassen." Kowalevsky reached this conclusion from studying the carpal bones of *Anchilophus*. I have had access only to the teeth of *Anchilophas desmarestii* and consequently must base my conclusions upon the characters of one species only of this genus. A comparison of the superior molars of *A. desmarestii* with those of *Mesohippus*, a genus which is considered by all competent authorities to be in the true Equine series, shows the following differences: The ectoloph in both genera has nearly the same form, but in *Anchilophus* the mesostyle is absent, this is well developed in *Mesohippus*. In *A. desmarestii* the hypostyle is wanting, which is so prominent in the molars of *Mesohippus*. The direction of the metaloph in *An-*

[8] Etudes sur l'Historie Palaeontologique des Ongules. Par Mme. Pavlow. Bull. Soc. Nat. de Moscou, 1888, p. 148.

chilophus is less oblique than in *Paloplotherium* and is more as in *Mesohippus*. In the lower molars of *Anchilophus* the metaconid is not reduplicated, and the crescents are in form more like *Palaeotherium*.

From the fact that Kowalevsky, in studying the podial elements of *Anchilophus*, concluded that this genus could not be in the true Equine series, adds mnch weight to the view of its non-persistence. Again, we have seen that the molars of *Anchilophus* are wanting in a number of important elements which are present in all later genera leading to *Equus*. The above evidence points to the fact that *Anchilophus* must be considered as another aberrant form not leading to permanent results.—CHARLES EARLE.

Reclamation of Deserts.—The shifting of the sand dunes in the Sahara desert frequently ends in destruction of fertile oases. To prevent the encroachment of the dunes upon the arable land has long been a problem with the French. Commandant Godron has inaugurated a system of tree planting in the neighborhood of Aïu-Sef-ra, Ouargla and El-Golea from which excellent results have been obtained. Following out the theory that tree plantations would prevent the dunes being at the mercy of the wind, and finally make them stationary, M. Godron planted a neighboring dune with seedlings of various species of trees and shrubs. To prevent the sand from shifting while the new plants were establishing themselves, a light covering of alfa straw was spread over the ground. This was found to effectually shield the sand from the action of the wind.

In making a plantation, Mr. Godron combines seeding, cuttings and plants already rooted. The species best adapted for growing on the dunes have proved to be the Barbary fig, peach, aspen, Italian poplar, weeping willow, *driun*, grape-vine, Spanish broom, acacia and roses.

To supply the demand for cuttings and rooted plants for this new desert industry, M. Godron has established local nurseries at Aïu-Sefra and at El-Golea. The water supply for maintaining the growth of vegetation is from artesian wells. The reclaimation of vast extents of desert land is hoped for in the future, through the adoption of the plantation methods of Commandant Godron. (Revue Scientif., Fev., 1896).

[9] Anthracotherium, p. 157.

BOTANY.[1]

Botany in the National Education Association.—An effort is now under way to bring about greater interest in the *teaching* of Botany than has hitherto been shown by American botanists. The new department of Natural Science Instruction is intended to bring together the teachers of science (Botany, Zoology, Chemistry, Physics, etc.) who are interested in science *as a means of culture*, and to stimulate thought and discussion as to how this end may best be obtained. What rôle should Botany play in the mental development of a man? In what way may the study of plants be made an efficient factor in a man's mental training? When and how should plant study be made a part of a man's training? These are some of the questions which will be discussed by the botanists in the Buffalo meeting of the National Educational Association on July 9th and 10th next. It is to be hoped that many who are interested in this department of Botany will be present.

—CHARLES E. BESSEY.

Coulter's Revision of N. A. Cactaceæ.—Nearly two years ago Dr. Coulter brought out the first part of his revision of the N. A. Cactaceæ (Contrib. U. S. Nat. Hist., Vol. III, No. 2), and now in No. 7 of the same volume we have the concluding part. The family as revised now includes North American genera and species as follows: *Cactus* Linn., Sp. Pl., 466 (= *Mamillaria* Haw. Synop., 177), with 64 species and varieties; *Anhalonium* Lem., Cact. Gen. Nov., with 5 species; *Lophophora* Coulter, a new genus, with 2 species; *Echinocactus* Link & Otto, Verh. Preuss. Gartenb. Ver., 3,420, with 52 species and varieties; *Cereus* Mill. Gard. Dict. Ed. 8, with 82 species and varieties; *Opuntia* Mill. Gard. Dict. Ed. 7, with 101 species and varieties. We have thus a total of 306 species and varieties of North American cactuses. The work is styled a *preliminary* revision, and the author says, in his prefatory note, that on account of the peculiar difficulties attending the revision "the undertaking would have been abandoned only that it seemed but proper to contribute to the knowledge of the group such facts as had come to light in the course of several years' study," a most commendable conclusion, indeed.

—CHARLES E. BESSEY.

Botanical News.—Dr. Charles A. White has recently prepared a Memoir of George Engelmann for the National Academy of Scien-

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

ces. It is a sympathetic sketch of the life of a strong and industrious man.

A recent bulletin (No. 10) from the Division of Forestry of the U. S. Department of Agriculture, with the title "Timber," contains much of general botanical interest. Such topics as "wood of coniferous trees," "wood of broad-leaved trees," "weight of wood," "shrinkage of wood," "mechanical properties of wood," etc., illustrate the scope of the work. It is deserving of a place in any botanical library

Professor R. A. Harper's confirmation of the act of fertilization in *Sphaerotheca costagnei*, in the *Berichte der Deutschen Botanischen Gesellschaft* (Bd. XIII, heft. 10) brings grateful relief from the monotonous repetition of doubts as to the accuracy of DeBary's work. The applicability of modern imbedding processes to the study of the life-history of the smaller fungi has rarely been better demonstrated than in this satisfactory paper.

The Eli Lilly Co., of Indianapolis, have recently issued an "Exchange List" of their herbarium. It includes 976 names, all in the modern nomenclature.

The Report of the Botanical Department of the New Jersey Experimental Station, by Dr. Halsted, possesses more than the usual interest of similar publications. With much of practical value, the author has mingled a great deal which possesses high scientific interest.

In Professor Scribner's New North American Grasses (*Bot. Gaz.*, March, 1896), four new species and one new genus are described, viz.: *Avena mortoniana* from Silver Plume, Colo.; *Danthonia parryi* from Georgetown, Colo.; *Zeugites smilacifolia*, a curious broad-leaved species from Cuernavaca, Mexico, and *Pringleochloa stolonifera*, from the vicinity of Mt. Orizaba, Mexico. The new genus is apparently very close to *Bulbilis* (*Buchloë*).

The Field Columbian Museum recently issued No. 2 of the Botanical Series of its publications. It contains the "Flora of West Virginia," by Dr. Millspaugh, and is a considerable enlargement of an experiment station report made by the same author a couple of years ago. It includes 2584 names, of which 980 are Fungi, 115 Lichens, 123 Bryophytes, 57 Pteridophytes, and 1309 Authophytes. The nomenclature is modern and the work is well done, but one is sorely puzzled with the peculiar sequence of families in the Fungi, in which one finds in strange juxtaposition *Saccharomycetaceæ*, *Diatomaceæ* and *Myxomyceteæ* (pp. 84–85).

Professor Greene's "New Western Plants," in the Proceedings of the Academy of Natural Sciences of Philadelphia (Feb. 7, 1896), con-

tains descriptions of some interesting plants, e.g., *Trifolium truncatum*, *T. lilacinum*, *T. rostratum*, *Boisduvalia diffusa*, *Valerianella magna*, *V. ciliosa*, *Lessingia pectinata*, *Pyrrocoma eriopoda*, *P. solidaginea*, *P. subviscosa*, *Aster militaris*, *A. frondeus* and *Vagnera pallescens*. To the same paper is appended a revision of the genus *Tropidocarpum*, including four species.

An important paper comes to us from the College of Agriculture of the Imperial University of Japan (Bull. 5, Vol. II, Dec., 1895). It includes a descriptive list of the winter state of the trees of Japan, by H. Shirasawa, illustrated by twelve crowded plates of twigs and buds.

Dr. J. C. Arthur has found out that the common notion of farmers that one of the seeds in the bur of the Cocklebur (*Xanthium canadense*) germinates one year and the other does not grow until the following or some subsequent year is true. He details his observations and experiments in a paper in the Proceedings of the 16th Annual Meeting of the Society for the Promotion of Agricultural Science. "The purpose of this seemingly unique character is to distribute the two seeds of the bur in time, the customary distribution in space being impossible owing to the indehiscent structure."

The announcement that the *Botanical Gazette* is hereafter to be published by the University of Chicago will please every friend of science, since it insures its permanence and provides for that growth which the development of American botany demands.

Suggestions About Antidromy and Didromy.—The interesting notes from Prof. Todd in the NATURALIST for March seem to me to bear upon a phenomenon which is different from what I have called antidromy: upon the *secondary changes* in the ordinary growth which seem to be intimately related to the direction of light and the necessities of exposure to air. They are commonly shown by the foliage of such plants as the elm, morningglory, peach, and Forsythia, and very often by flowers, in which they may subserve cross-fertilization.

Antidromy, in its strict sense, is a diversity of a primitive character, arising phylogenetically away down in the cryptogams, and maintained with singular constancy through all the Phaenogams; and ontogenetically it starts in the ovule, depending on the circumstance that every plant bears two castes of seeds, one set on each border of the carpellary phyllome. I have not yet determined whether it is a dextrose seed that grows on the right margin of the carpel: but all the carpels on the same plant seem to retain the twist of the plant which bears them (well

shown by the twisted pods of species of *Prosopis*), whilst the seeds which arise from the two sides of a carpel are diverse. The order of development in the carpel may depend on the direction of the nutriment, and on the lines of least resistance in the crowded condition of young organs resulting in right-handed and left-handed ovules. The outcome is that in the adult plant the whole phyllotaxy, including the floral structure and ramification of the entire organism and its inflorescence, are of one and the same order in any one individual plant, and of a different order in other plants of the same species. Whether this is of any special advantage to the grown plants I cannot say; but possibly by imparting different habits of growth to the various members of crowded vegetation, it may cause them to separate from each other, and so may diminish the intricate interlacing which is so injurious to gregarious plants.

Now that the season of vegetation is returning it is to be hoped that some of our young botanists will make and record their observations on this subject. We want especially to find out exceptions, apparent or real. My first paper was incorrect as to the supposed antidromy of rows of grains in the case of maize; dissection seemed to teach this; but I might have foreseen that the ear is just like the male panicle, having a disorderly crowd of grains rearranged by a secondary process into orderly rows, each row, however, including both dextral and sinistral grains. The case of the Bilsted (*Liquidambar*) is a puzzle, some of the branches of the same tree having dextral phyllotaxy, and others having sinistral phyllotaxy; this is the only case of the true internal antidromy known to me, though I shall not be surprised by the discovery of other similar cases. A somewhat similar condition is reported to me by Prof. Francis E. Lloyd, of Forrest Grove, Oregon, in ten cases of *Acer circinatum*. Perhaps these cases are allied to that of plants arising from rootstalks, as *Iris* and *Calla*, *Helonias*, *Nuphar*, etc., in which different plants arising from the same rootstalk are antidromic. It will be worthy of examination whether sarmentose plants, as strawberry, and the *Saxifraga* described by Prof. Todd are antidromic as between those grown from the same original stock.

The phenomenon which I have termed *didromy*, where the same member is twisted in opposite directions at its two extremities, seems to me to be always related to the immediate life of the plant, and to have no genetic significancy. The didromic twist of the awn of *Danthonia* and other grasses, results in the upper part penetrating an object so soon as the lower part untwists by the application of water: that of the long peduncle of *Vallisneria* approximates the extremities, thus

pulling the fertilized flower down through the water without turning it around. If I am correct in the observation that some plants of Vallisneria have the dextral twist at the lower part, and others have the sinistral twist below, this would be a complex of the primitive antidromy having superposed upon it a recently acquired didromy.

A different line of investigation will search out the relation of dextrorse or sinistrorse phyllotaxy to the leaf-traces in the stem. I am convinced that inattention to this point has marred some of the work on the histology and the plan of the fibrovascular bundles, and that even with opposite-leaved plants, many species will be found to exhibit a duplicate pattern as between the arrangements in different individuals of a species.—GEORGE MACLOSKIE.

Princeton College, March 16, 1896.

VEGETABLE PHYSIOLOGY.[1]

A New Classification of Bacteria.—In a recent number of *Die Natürlichen Pflanzenfamilien* (Lieferung 129, Leipsic, 1896) Prof. W. Migula, of Karlsruhe, gives a classification of the bacteria which is much more practical and satisfactory than that of Dr. Alfred Fischer, noticed in the September (1895) number of this journal. Migula's arrangement seems, on the whole, to be the best yet devised, and will probably come into general use, at least among botanists. The characters of several genera are amended, properly it seems to the writer, e. g., *Bacterium, Bacillus, Streptothrix,* and other genera are discarded as being founded on purely biological grounds, e. g., *Photobacterium, Nitromonas, Clostridium.* Of course, biological peculiarities are recognized as indispensable in the differentiation of species. In reading this paper one is occasionally surprised at the omissions, but taken in its entirety the work of consulting literature seems to have been very carefully done, and what is more important the classification appears to have grown out of a long and wide experience in the laboratory, and seems to be eminently usable. This paper treats briefly of most important literature, morphology, vegetative condition, resting state, cultures on artificial media, biological peculiarities, geographical distribution, rela-

[1] This department is edited by Erwin F. Smith, Department of Agriculture, Washington, D. C.

tionships, earlier classifications, etc., and then proceeds to the description of the families and genera. The group Schizomycetes forms the first section of the Schizophyta, and is divided into five families; Coccaceæ, Bacteriaceæ, Spirillaceæ, Chlamydobacteriaceæ and Beggiatoaceæ, as follows:

I. Cells globose in a free state, not elongating in any direction before division into 1, 2 or 3 planes	1. Coccaceæ.
II. Cells cylindrical, longer or shorter, only dividing in one plane, and elongating to twice the normal length before the division.	
(1) Cells straight, rod-shaped, without a sheath, non-motile or motile by means of flagella	2. Bacteriaceæ.
(2) Cells crooked, without a sheath	3. Spirillaceæ.
(3) Cells enclosed in a sheath	4. Chlamydobacteriaceæ.
(4) Cells destitute of a sheath, united into threads, motile by means of an undulating membrane	5. Beggiatoaceæ.

The genera recognized by Prof. Migula are as follows:

(1) Coccaceæ	
A. Cells without organs of motion	
a. Division in one plane	1. Streptococcus.
b. Division in two planes	2. Micrococcus.
c. Division in three planes	3. Sarcina.
B. Cells with organs of motion	
a. Division in two planes	4. Planococcus.
b. Division in three planes	5. Planosarcina.
(2) Bacteriaceæ	
A. Cells without organs of motion	1. Bacterium.
B. Cells with organs of motion (flagella)	
a. Flagella distributed over the whole body	2. Bacillus.
b. Flagella polar	3. Pseudomonas.
(3) Spirillaceæ	
A. Cells rigid, not snake-like flexuous	
a. Cells without organs of motion	1. Spirosoma.
b. Cells with organs of motion (flagella)	
1. Cells with 1, very rarely 2–3 polar flagella	2. Microspira.

2. Cells with polar flagella-tufts	3. Spirillum.
B. Cells flexuous	4. Spirochæta.
(4) Chlamydobacteriaceæ	
A. Cell contents without granules of sulfur	
a. Cell threads unbranched	
I. Cell division always only in one plane	1. Streptothrix.
II. Cell division in three planes previous to the formation of conidia	
1. Cells surrounded by a very delicate, scarcely visible sheath (marine)	2. Phragmidiothrix.
2. Sheath clearly visible (in fresh water)	3. Crenothrix.
b. Cell threads branched	4. Cladothrix.
B. Cell contents containing sulfur granules	5. Thiothrix.

(5) Beggiatoaceæ

Only one genus known (Beggiatoa Trev.) which is scarcely separable from *Oscillaria.* Character as given under the family.

This scheme is simple in comparison with that of Fischer, but to fully appreciate it one should compare it with that of de Toni and Trevisan in Saccardo's *Sylloge Fungorum,* where cumbrousness and triviality reach a climax, these authors breaking up the group into no less than 50 genera.

From among the various general statements we cull the following: For the most part the wall of the cell is formed not out of cellulose or any similar carbohydrate, but out of albuminoids. The wall may, however, contain embedded in its substance variable quantities of a carbohydrate coloring blue with iodine. There is no "centralkörper" in the true bacteria, but vacuoles have frequently been mistaken for such bodies. Most bacterial pigments are non-nitrogenous bodies related to the analin colors; others are nitrogeneous substances related to the albuminoids, such apparently are the fluorescent pigments. The manner of cell division is a fundamental distinction between Bacteriaceæ and Coccaceæ. Cell division always takes place at right angles to the longitudinal axis when any such is clearly visible. With few exceptions motility is accomplished by means of flagella. These are

extremely delicate protoplasmic structures originating directly from the membrane. This mode of origin makes it still more probable that the wall of the cell is only an external denser layer of plasm. Bacteria in which the contents has been drawn away from the point of insertion of the flagella by plasmolysis are apparently still capable of motion. The flagella are either fastened to one or both poles of the cell or else are scattered irregularly over the whole body. This position of the flagella on the bacterial body is constant and may be used to distinguish genera. In some species all of the plasma of the mother cell is not used up in spore formation, but sometimes a considerable part is left in the rod. In germination the spores swell up, lose their refractive power, and usually open either at the pole or equatorially allowing the young germ to protrude, but sometimes the wall of the spore entirely deliquesces before the germ has protruded, the latter simply elongating as a vegetative cell. It is not always easy to determine the character of the refractive contents of bacterial cells, and in such cases absolute demonstration of their sporiferous nature can only be had by seeing them germinate. All bacterial cells may pass into a resting state when for any reason growth ceases, but such cells do not possess any spore character and are only ordinary vegetative cells under conditions unfavorable to growth. Arthrospores do not exist, and this term should be discarded. Gonidia occur in the Chlamydobacteriaceæ. In *Cladothrix* these bodies escape from the sheath as swarm cells. In *Crenothrix* and *Phragmidiothrix* the contents of the vegetative cells becomes septated into Sarcina-like cubical packets, the individual cells of which finally round off and escape on the opening of the sheath as non-motile bodies which soon grow out into new threads. In *Streptothrix* the contents of the cell breaks up into a series of ovoid or round non-motile cells which escape from the sheath and grow wherever they happen to lodge.

This paper can be had from Wilhelm Engelmann, Leipsic, for 3 marks, and ought to be in the hands of every working bacteriologist.

ERWIN F. SMITH.

Ambrosia Once More.—There are two species of *Xyleborus* which bore in orange wood, one is *X. fuscatus* and the other is really undescribed, but goes well enough for the present under the name of *X. pubescens*. Both of these are associated frequently with *Monarthrum fasciatum* and *Monarthrum mali*, not only in the orange but in other hard wood trees of any kind, and even in the wood of wine and ale casks, in which they are able to propagate their "ambrosia" as well as in living or rather in dying trees. The ambrosia is very probably a dif-

ferent fungus for the different species of these beetles, even where several species occur in the same tree trunk. The ambrosia of *X. pubescens* and *X. fuscatus* is deep black in its stain, or in its later stages, and the same fungus may serve both these species; but *X. xylographus*, a cosmopolitan species frequently found in hickory, oak, beech and the like, has a very different ambrosia, which is olive-green when dry, or leaves a stain of that color in the chambers. The ambrosia of *X. celsus*, a large species found in hickory, is dark brown in stain and in the form of its conidia is entirely different from that grown by any other of the species I have mentioned.

In the orange trees injured or killed by frost the most numerous borer is *Platypus compositus.* Its ambrosia is entirely different from that of the Xylebori in the orange, and stains the chambers dark brown. Several species of Platypus are found in the Southern States in all sorts of timber, conifers and deciduous trees alike. *P. compositus* attacks all sorts of trees, including our pines.

The scolytid boring into the whortleberry, which I brought to the Department of Agriculture last fall is *Corthylus punctatissimus.* It lives in the roots of several shrubs, as hazel, witch hazel, etc. Its ambrosia leaves a deep black stain. *Corthylus columbianus*, discovered by Hopkins, makes notable black stains in Liriodendron wood.

I have had the opportunity of examining three or four kinds of ambrosia and have found them very distinct in the form and arrangement of the conidia as well as in the habit of growth of the mycelium. There is, I think, very little doubt that the different species of ambrosia are connected with certain scolytid beetles irrespective of the wood in which they make their galleries. The different genera in which I have found the food to consist of ambrosia are *Platypus*, *Xyleborus*, *Monarthrum* and *Corthylus.* It is useless to give lists of plants attacked by these ambrosia-raising beetles because most of them make their galleries and brood chambers in a great variety of trees and shrubs Some species live preferably in the roots of plants and others in the trunks or larger branches, but very few species are restricted to one or even a few kinds of timber.

I expected to learn much about the forms of ambrosia found in galleries of the different borers in orange trees, but I find on my return here this winter that a ferment has taken complete possession of all the ambrosia which I have examined, and the operations of the beetles are for the time being at a standstill. A slide of the material now lining the galleries [Feb. 20, 1896] shows only fragments of the mycelium of the fungus, and the entire field swarms with bodies like yeast (or bac-

teria?) in active eruption. These spores form dense masses and entirely fill up many of the burrows, often smothering the insects. I am curious to know whether this is really the end of a prodigious attack that has been made by the beetles during the past summer upon the dying orange trees, oaks and other timber injured by the great freezes of last winter [Dec. 27–29, 1894 and Feb. 7–9, 1895], or whether it is only a temporary condition due to the inactivity of the beetles during the winter I should not be surprised to find that it has put an end to further increase of the colonies of ambrosia-eating insects by making it impossible for them to propagate their food fungus.—HENRY G. HUBBARD, Crescent City, Florida.

NOTE.—Mr. Hubbard reports (May 18) that the beetles were finally overwhelmed by this "intruding ferment," and now believes that their depredations are usually brought to an end in this manner.—E. F. S.

ZOOLOGY.

The Feeding Phenomena of Sea Anemones.—Nagel has claimed that only the tentacles of the sea anemones were stimulated by food, while Loeb has shown that other parts of the oral disc were equally sensitive. To settle which was correct Dr. Parker made his experiments on our common *Metridium marginatum*.[1] When the animal is expanded, carmine dropped on the tentacles is gradually carried outwards by the ciliary action until it is dropped outside the disc. In this there is at first only a slight muscular action, and then the tentacles are quiet as before. If, however, a bit of crab-flesh be dropped on the tentacles, these are stimulated much more. They now gradually bend inward, and the flesh, carried toward the tips of the tentacles like the carmine, is dropped inside the lips of the mouth. Experiments showed that this stimulation was produced by the juices of the meat, sugar, quinine, meat and picric acid—all substances with taste—as well as filter paper, rubber, etc., produced no stimulation. Each tentacle was stimulated alone. Between the tentacles and the lips is an intermediate zone in which no stimulation occurs. Bits of crab meat dropped upon it remain quiet for a time, and then gradually move outwards. Dr. Parker was not able to explain with certainty how. When, however,

[1] Bull. Mus. Comp. Zool. XXIX, No. 2, 1896.

the meat reached the tentacles, these were stimulated, and acted just as before.

The region of the lips was different in its action. At either end (sometimes at only one) is the ciliated siphonoghlphe, and here the ciliary action is constantly directed inward, and all matters placed upon them were invariably carried inward—paper, meat, quinine, sand, all acting alike. At both siphonoghyphes the current was the same—directed inward. The rest of the oral area between the siphonoghyphes is also ciliated, but here the current is normally outward. Sand or carmine dropped here was carried outward, across the intermediate zone and over the tentacles as before. If, however, a bit of meat be placed here, it at first starts outward, then stops and moves in toward the mouth, thus indicating, as did other experiments, that there was a reversal of the direction of ciliary action. Stimulation of one side in this way did not cause reversal in the other lip. Again, indifferent substances and crab-meat placed near each other moved in different directions, thus indicating that the reversal affected only a small area and not the whole of the lip. Filter paper alone was passed outward; filter paper soaked in crab juice was swallowed. No such ciliated areas occurred on the column, and this region did not react in any way to food stimuli.

The Relation of Myrmecophile Lepismids to the Ants.— The relation of the numerous forms of animal life found in ant hills (and therefore myrmecophilous) to the owners of the hills is varied. It has long been well-known that the plant-lice found in the hills have a relation to the ants nearly analoguous to that of the cow to man. They are retained and cared for by their owners for the liquid that they exude from their bodies when tickled by the "milker's" antennæ.

Certain staphylinids also exude a substance of which the ants seem to be fond, and in return are fed by the ants. As a consequence of this symbiotic relation, Wasmann has pointed out that the palpi of the staphylinids have become more or less noticeably reduced in size, thus indicating some degree of dependence upon the ants. In the case of *Claviger testaceus* found in the ant hills in the neighborhood of Paris, this dependence is so complete, according to Janet,[2] that the beetles perish upon being separated from the ants. To this sort of symbiotic relation the name-coiners have applied the term myrmecoxeny.

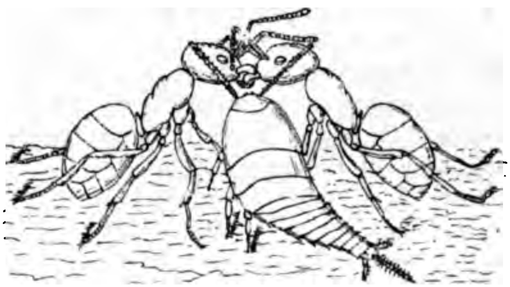
In addition to these myrmecoxenous forms there are those that like *Myrmedonia funesta* capture and devour either the ants themselves or

[2] Comptes Rendus, CXXII, 799–802.

their young, and must in consequence bear the term myrmecophagous. Then there are nematode internal and acarid external parasites. Then there are forms, that like the isopod *Platyarthrus hoffmanseggi* of Europe, flourish in and upon the detritus of the hills without molesting or being molested by the ants, a mode of life denominated synæketic.

Lastly, come the lepismids, living what Janet calls a myrmecocteptic life. Notwithstanding the name, the relation of the lepismids as told and illustrated by the above named writer is peculiar, and withal rather interesting. His experiments were performed with *Lepismina polypoda* Grassi, captured along with a colony of *Lasius umbratus* Nyl.

Some twenty-one of the lepismids were separated from the ants, and fed upon a mixture of honey, sugar, flour, and yolk of egg. At the end of two years and six months only nine remained in good condition. These willingly eat the drops of food presented to them upon the



points of fine pincers. Those left and reared along with the ants were much more active than the separated lot, running incessantly among the ants, always seeming to avoid remaining quiet in their presence. Sometimes they were pursued, but owing to their superior agility were able to escape. In the conditions of the artificial nest, however, where places of safety were doubtless fewer than under natural conditions, they were often captured. Two days after the beginning of the experiment five Lepismid cadavers were noted. In order to save the rest the colony was given a new nest, where certain places would be less frequented by the ants. These the Lepismids found, and in them remained quiet; but as soon as a single ant made its appearance, they scampered precipitously away.

When the ants were fed with their customary supply of small drops of honey, the Lepismids, by their agitation, manifested that they had become aware of the proximity of very desirable food. Meanwhile, the ants that had discovered it, gorged themselves to fulness. Then, returning to the neighborhood of their companions, who had not found

the supply, seemed to be requested by the latter to "give me some;" a request that did not seem to be refused. Soon pairs of ants became locked together mandible to mandible, the one giving, the other receiving, a drop of honey. As quickly as a lepismid perceived this condition of affairs, he rushed in between the pair and intercepted the drop or a portion of it in its passage, and then retreated precipitately, but only to treat another pair in a similar manner, and so on until his hunger was appeased. Lepisma then is not in the ant hill for an exchange of services, like some of the staphylinds; nor to be "milked," like the aphid, nor to be a common parasite, nor a common thief; but is there as more or less of a wary freebooter.—F. C. K.

Lipophrys a Substitute for Pholis.—In my article on the application of the name *Pholis* to the gunnels, I find a note was omitted, replacing the homonymous name of the blennioid genus. As the latter will be left without a proper name on account of the preoccupation of the one it has so long borne by the gunnel, a new one will be requisite for the blennioid genus, then, I propose the designation Lipophrys (λιπ, indicating want or absence; οφρύς, eyebrow)[3] in illusion to the absence of the superciliary cirri, and its type is the common *Blennius pholis* (Linn) of Europe.

I have given the family name *Pholididæ*, because there are some who will not retain *Xiphidion* on account of the existence of a prior *Xiphidium*, and therefore would not adopt the family name derived from that genus. If, however, the latter is retained, it would be better modified as *Xiphidiidæ*.—THEODORE GILL.

Blind Batrachia and Crustacea from the Subterranean Waters of Texas.—From an artesian well, 188 feet deep, recently bored at San Marcos, Texas, there were expelled more than a dozen specimens of a remarkable batrachian, together with numerous crustaceans. The latter are described by Mr. Benedict, and the batrachian by Dr. Stejneger.

The crustaceans comprise numerous shrimps (one new species, *Palæmonetes antrorum*, a lesser number of Isopods of a new genus (Cirolanides), and a very few Amphipods.

All the species are white, blind and have unusually long, slender feet and antennæ.

The Batrachian, for which Stejneger creates a new genus, is described under the name *Typhlomolge rathbuni*. It belongs to the family Proteidæ, and is more nearly allied to Necturus than to Proteus.

[3] In analogy with λίπο-βλέφαρος, without eyelids, and λίπογληνος, without eyeballs, or sightless.

Like the crustaceans, it is blind. The most remarkable external feature is the length and slenderness of the legs. In commenting on this peculiarity, Dr. Stejneger says: "Viewed in connection with the well-developed finned swimming tail, it can be safely assumed that these extraordinarily slender and elongated legs are not used for locomotion, and the conviction is irresistible that in the inky darkness of the subterranean waters they serve as feelers, their development being thus parallel to the excessive elongation of the antennæ of the crustaceans."

The gills are external, its color nearly white, having the upper surfaces densely sprinkled with minute pale gray dots, and its total length measures 102 mm. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

Lungless Salamanders.—Following up the observations of Dr. H. Wilder, certain tailed Batrachia examined by Dr. Einar Lönnberg with reference to their possessing functional lungs have brought to light the following facts: *Desmognathus auriculatus Holbr.* and *Plethoden glutinosus* Green exhibit no trace of either lungs or larynx. A median longitudinal groove is the only remaining rudiment of the aditus ad laryngem. The transverse laryngeal muscles are well developed in *Plethodon glutinosus*, as is also the median narrow strip of connective tissue at which the muscles insert themselves.

Manculus quadridigitatus has no trace of lungs, larynx or aditus ad laryngem, and, although the laryngeal muscles are well developed, the connective tissue between the muscles is very feebly developed. The median strip of connective tissue forming a point of insertion for the laryngeal muscles can be seen. This species exhibits the most reduced rudiments of the laryngeal apparatus of the specimens under observation.

Amblystoma opacum possesses rudimentary lungs and a small aditus ad laryngem. The author regards the lungs as rudimentary, because they are so very small and narrow, measuring about 9 mm. in length and 1½ mm. in width at the broadest place. It is probable that the function as respiratory organs in conjunction with some other organ, either the skin or "la cavité bucco-pharyngienne," as Camerano has found to be the case with *Spelerpes fusca.*

The theory suggested by the author to explain the reduction and oss of lungs in these animals is stated as follows: When these salamanders lost the gills and increased in bulk, the small and not very composite lungs were insufficient for respiration, so that the bucco-pharyngeal cavity (? together with the exterior integument) even from the beginning had to play a certain part. In some of the forms the

respiratory capacity of that cavity increased more rapidly than that of the lungs, which is the easier to understand, as the air breathed must first pass through that cavity, and because the cavity is rather large. When this capacity had developed to a certain extent, the lungs were no longer needed, and gradually atrophied from disuse.

All the salamanders examined lead a more or less terrestrial life; but the peculiar characteristic of reduction of lungs is not confined to terrestrial forms. (Zool. Anz. XIV, Bd., No. 494, 1896.)

Batrachia Found at Raleigh, N. C.—*Necturus maculatus.* Water Dog. This species is caught by anglers in the spring, and seems scarce, as I have only seen eight specimens so far, none of which measured over 7½ inches in total length. Some of them were evidently breeding females.

Amblystoma opacum. Marbled Salamander. Common. They lay their eggs in dry season under logs on the edges of dried-up pools, and the eggs hatch out quickly when the pools fill up again from rain; whether they do this in wet seasons I do not know. Sometimes the larvæ are very abundant, sometimes very scarce. This winter, after a dry autumn, they are abundant. Last winter, after a wet autumn, I found difficulty in securing any. The eggs are laid in October and November.

Amblystoma punctatum. Quite rare here.

Plethodon glutinosus. Viscid Salamander. Very common under rotten logs in woods.

Manculus quadridigitatus. Tolerably common. This species enters the water in December to breed, and retires to dry land again about February. It seems entirely terrestrial, except when breeding. I took nearly full grown larvæ in May, 1895.

Spelerpes bilineatus. Striped Salamander. Common. This salamander is found in the water, breeding from December to March; the larvæ first appear in May, and do not attain their full growth till a year or more afterwards. Except in the breeding season I believe it to be entirely terrestrial.

Spelerpes guttolineatus. Tolerably common. Found mostly in or around rocky springs or on the edges of rocky brooks, or of the larger streams. They can be taken containing eggs in November; but I have never seen any larvæ that had any sign of belonging to this species.

Spelerpes ruber. Red Triton. Aquatic, though like the next species sometimes found under logs not far from the water. Judging from the varying size of larvæ taken at the same time of year, I think it proba-

ble that this species spends at least one whole year, and possibly two, in the larvæ state.

Desmognathus fusca. Brown Triton. Found in all brooks, and is very common. The larvæ attain the adult condition in a shorter time than those of *Spelerpes bilineatus*, as though they are both hatched about the same time; the larvæ of this species complete their metamorphosis in the autumn or winter following their birth, being then only about one-half the size of larvæ *Spelerpes bilineatus* of the same age.

We get specimens of very varying coloration; some being nearly black, some very light.

Diemyctylus viridescens. Newt. Common in weedy pools.

Amphiuma means. Rare. I know of eight adults and twenty-two larvæ having been taken here, all being two-toed specimens.

Bufo americanus. Common Toad. Very abundant. Breeds in spring and summer.

Scaphiopus holbrookii. Last May I collected fifty breeding in a pool only a few yards from my house; in every case the grasp of the male was inguinal. The cry was not much louder than that of the common toad. I have occasionally dug them out of the ground.

Hyla versicolor. Common.

Hyla pickeringii. Abundant. Breeds in March and April.

Chorophilus feriarum. Abundant. Breeds in February and March. I have never seen this species except at the breeding season.

Acris gryllus. Cricket Frog. Abundant. Active all the year round except in the severest weather. This species breeds from April through most of the summer.

Engystoma carolinense. This species is very abundant in the breeding season, which is in July and August, and possibly the two preceding months. Have never seen any except when breeding; I think they are nocturnal.

Rana pipiens. Leopard Frog. Abundant. Breeds in March.

Rana clamata. Spring Frog. Common.

Rana catesbiana. Bull Frog. Not as common as the preceding two breeds in February and March.

Rana palustris. Pickerael Frog. Rare. Only four specimens so far. —C. S. Brimley.

The Frilled Lizard.—The report that the Frilled Lizard (*Chlamydosaurus kingii*) inhabiting the tropical parts of the Australian continent, is in the habit of running erect on its hind legs, receives confirmation from W. Saville Kent. Specimens in captivity were

seen by him to run thirty or forty feet at a stretch, in an erect position on their hind legs, and when after resting momentarily on their haunches, to resume a running course. The conformation of the hind foot is such that when running only the three central digits rest upon the ground. Consequently the track made by this lizard in passing erect over wet sand would correspond with such as are left in mesozoic strata by various Dinosauria (Nature, Feb., 1896). Mr. Kent suggests affinities with the latter order; but these do not exist, as Chlamydosaurus is a typical Lacertilian. It is not the only lizard that progresses on its hind legs, as Mr. Francis Sumichrast pointed out several years ago that a species of the Iguanid genus Corythophanes found in Mexico has the same habit.—(ED.)

The Palatine Process of the Mammalian Premaxillary.—While engaged in the study of the comparative anatomy of Jacobson's Organ, Mr. R. Broom came across some interesting facts in connection with the palatine process of the mammalian premaxillary, which he puts on record in the Proceeds. of the Linnean Soc., N. S. W., Vol. X, 1895. From his observations he concludes that the os paradoxum in Ornithorhyncus, the anterior vomer (Wilson) in Ornithorhynchus, the anterior paired vomer in fœtal Insectivora, etc. (Parker), the prepalatine lobe of vomer in Caiman (Howes), and the vomer in Lacertilia and Ophidia (Owen, Parker, etc.), are homologues or synonyms of the process under discussion. He therefore suggests the name prevomer, to cover all the designations which the different forms of this ossification has received. (Proceedings of the Linnæan Soc. of N. S. Wales).

New formation of nervous cells in the Brain of the Monkey, after the complete cutting away of the occipital lobes.—It is known that the noviformation in the nervous cells in the nervous centres and above all in the brain has not yet received a definite solution. There has been made, however, a number of researches on this important question, but the contradictory results arrived at, have not as yet advanced our knowledge on this subject. On the contrary, the conclusions arrived at by M. G. Marinesen, presented to the Society of Biology in 1894, are that the cells and nervous fibres of the nervous centres do not grow again after their destruction.

In pursuing his studies on the physiology of the occipital lobes, M. Alex. N. Vitzou has discovered the presence of cells and of nervous fibres in the substance of noviformation, in the Monkey, two years and two months after the complete cutting away of the occipital lobes. The entire extirpation of these lobes results, as is known, in a total loss of

sight in both monkeys and dogs. The experience of the author, concerning this point agrees with that of M. H. Munck and confirms his conclusions. The later researches of different scientists have confirmed the facts which he demonstrated.

Repeating the experiment of total extirpation of the two occipital lobes of monkey, February 19, 1893, M. Vitzou noticed that during the fourth month the animal commenced to perceive persons and objects, but with great difficulty. At the end of fourteen months, the ability to perceive was greatly increased. The monkey could avoid obstacles, which he could not do during the first months following the operation.

On the 24th, of April, 1895, Mr. Vitzou repeated the operation upon the same animal. After denuding the skull he found the orifices of trepanation closed by a mass of rather firm connective tissue. On lifting this mass with care, to his astonishment and that of the assistants standing about him, he found the entire space which had formerly been occupied by the occipital lobes completely filled with a mass of new formed substance. This he proceeded at once to examine.

A portion was taken from the centre of the mass closing the orifice of trepanation, and another from the posterior part of the new formed substance found in the skull. Employing both the rapid method of Golgi and Ramon y Cajal, and the method of double coloration with hematoxyline of Erlich and eosine in aquous solution, M. Vitzou demonstrated the presence of pyramidal nervous cells and of nerve fibres. The nerve tissue was present in large quantities and the nerve cells less numerous than in the occipital lobes of the adult animal, but their presence in the new formed mass was constant.

In brief the conclusion from the preceding experiment is that the new substance occupying the place of the occipital lobes, was of nerve nature, and that it was due to a new formation of cells and of nerve fibres in the brain of the monkey. Here is a fact, says the author, which demonstrates the possibility of regeneration of nerve tissues in the brain, as well as, what was previously known, that active nutrition is maintained in the rest of the organ.

Moreover, we find in the presence of cells and nerve fibres in the new formed mass an explanation of the fact concerning the betterment, although slight, of the sense of sight. This explains also contradictory facts presented by different scientists, in the case of partial extirpation of the brain followed by an amelioration of the functions lost during the first operation.

M. Vitzou adds that the monkey having been subjected to a second operation lost the sight from both eyes for three months and a half, at

the end of which time he gave signs, although somewhat uncertain, of recovering his vision. The animal is well cared for in order that the author may continue his observations for some time to come; then later, he will be sacrificed in order that a complete study may be made of the new formation. (Revue Scientif. 1895, p. 406.)

ENTOMOLOGY.[1]

Domestic Economy of Wasps.—Much attention has recently been given to the biology of wasps. One of the most interesting accounts is that of M. Paul Marchal[2] summarized in the Annals of *Magazine of Natural History.* The investigator studied the earth-burrowing wasps (*Vespa germanica, V. vulgaris*). The fully-formed nests contain small and large cells, the latter constituting two or more of the lowest combs, while the others make up the six to ten upper combs. The large cells, built only by the workers in August, may, at an early period, receive indifferently either females or males, the former being either queens or very large workers, the latter always in small proportion; after the first of September these cells are entirely set apart for the queens, so that in October no males are to be found in them.

The small cells, from the time that the laying of eggs for males has begun, contain indifferently up to the end of the season either workers or males. The proportion of males in the combs of small cells decreases from below upwards, with this remarkable exception—that if there be a mixed comb containing both large and small cells, the small cells are influenced by the proximity of the large cells, and contain very few males.

The beginning of the period for laying males coincides very nearly with the time of appearance of large cells, early in August. The curve which represents their production rises suddenly in an almost vertical manner to reach its maximum; it then descends gradually with or without oscillations to the end of the reproduction. The queen takes a prominent part in this great production of males, because the laying workers have already long since disappeared, whilst the young male larvæ are still to be found in great numbers in the nest.

The queen has then (at least after the early days of September) the power to determine with certainty the female sex of the eggs which

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] *Comptes Rendus*, t. cxxi, pp. 731–734.

she lays in the large cells; on the other hand, she lays indifferently either female or male eggs in the small cells. One can only admit in order to explain this remarkable fact, the principle of the theory of Dzierzon, based upon the fecundation, because if the production of males were due, for example, to the influence of season, it is evident that the eggs laid at the same epoch in the large cells would become male just as much as the others. In order to interpret all the facts, M. Marchal thinks this theory should be modified, by allowing the intervention of another factor than the will of the queen, and continues: We will admit, then, that after her first deposit of eggs, exclusively those of workers, which lasts until the first of August, the reflex which brings about the contraction of the seminal receptacle at the moment of the laying of each egg is no longer produced with the same energy, and that therefore the eggs can be laid without being fecundated; thence the almost sudden appearance of males corresponding to the relative state of inertia of the receptacle. Then it is that the workers building the large cells give the queen a choice between two distinct classes of the alveoli, and she, stimulated by the presence of the large alveoli, which seem to possess the power of rendering her reflexes more energetic, will concentrate from that time all her energies upon them and will lay only fecundated eggs and females. The modification thus introduced into the theory is important because it replaces the voluntary act of the queen by a passive one. The queen does not deposit males and females at will; but there comes a time when she cannot do otherwise than deposit males, because of the relative inertia of her receptacle.

M. Marchal finds that the laying of eggs by workers is normal in August to a small extent, and that it is greatly increased in case the queen is removed or stops laying.

Circulars on Injurious Insects.—A valuable series of circulars on injurious insects is being issued by the United States Division of Entomology. In each, one of the more important pests is discussed, its method of work, distribution, life-history, natural enemies and remedies being clearly described. Recent issues include circulars 9 to 15, with the following titles: Canker-worms, by D. W. Coquillet; The Harlequin Cabbage Bug, by L. O. Howard; The Rose Chafer, by F. H. Chittenden; The Hessian Fly, by C. L. Marlatt; Mosquitoes and Fleas, by L. O. Howard; The Mexican Cotton-Boll Weevil, by L. O. Howard, and Shade Tree Insects, also by Mr. Howard.

Gypsy Moth Extermination.—The last Report of the Massachusetts Gypsy Moth Commission shows that decided progress has been made in checking the pests. About $130,000 was spent during 1895. In commenting on the policy of State control of the pest, Prof. C. H. Fernald writes:

The value of the taxable property in this State is $2,429,832,966, and an appropriation of $200,000 is a tax of less than one-twelfth of a mill on a dollar. A man having taxable property to the amount of $5,000 would have to pay a tax of only 41 cents and 6 mills. This beggarly sum of money would make but a small show in the work of clearing gypsy moth caterpillars from an infested $5,000 farm, while in the uninfested parts of the State the land owners would be paying an exceedingly small premium to the State to insure them against the ravages of the gypsy moth. This premium on a $1,000 farm would be $8\frac{1}{3}$ cents, and for fifty years it would amount to only 4.16\frac{2}{3}$ cents. This protection would extend not only to farmers and owners of forest lands, but also to residents in villages and cities who own lots with trees and shrubs on them, and to vegetation wherever grown within the limits of our Commonwealth.

Entomological Notes.—Messrs Howard & Marlatt publish, as Bulletin No. 3, of the United States Division of Entomology, an elaborate discussion (80 pages) of The San José Scale: Its Occurrences in the United States, with a Full Account of its Life-history and the Remedies to be used against it.

In reporting[3] on the 1895 experiments with the Chinch-Bug diseases, Prof. F. H. Snow says that the year's experience corroborates the conclusion of former years that *Sporotrichum* is ineffective unless the weather conditions favor its development.

In Bulletin 36, of the Hatch Experiment Station of Massachusetts, Messrs Fernald and Cooley discuss the imported Elm Leaf Beetle, the Maple Pseudococcus, the Abbot Sphinx and the San José Scale.

Some potato insects are discussed by Prof. H. Garman in Bulletin 61 of the Kentucky Experiment Station.

Mr. M. V. Slingerland continues the excellent entomological bulletins from the Cornell University Experiment Station. Recent issues deal with Climbing Cutworms (Bulletin 104), Wireworms and the Bud Moth (107) and the Pear Psylla and Plum Scale (108).

In Bulletin No. 43, of the Minnesota Experiment Station, Prof. Otto Lugger discusses Insects Injurious in 1895. The Bulletin covers about

[3] Fifth Report of Experiment Station of the University of Kansas. Lawrence, 1896.

150 pages with sixteen plates, and shows that a large amount of work has been done. The entomological department has a special annual appropriation of $5,000 which enables it to carry on extensive field experiments.

In the December, 1895, Bulletin of the Tennessee Station, Chas. E. Chambers discusses the Chinch Bug.

Mr. Frank Benton's admirable Manual of Instruction in Apiculture, issued as Bulletin No. 1, New Series of the United States Division of Entomology, is being most cordially welcomed by the bee keeping fraternity.

EMBRYOLOGY.[1]

Morphology of the Tardigrades.[2]—R. v. Erlanger has published the results of his observations on the early development of *Macrobiotus macronyx* Dujardin. The division of the egg is total and equal, segmentation resulting in the formation of a long oval blastula with the segmentation cavity located nearer the posterior, more pointed pole. Regular gastrulation takes place, with the cells of both ectoderm and entoderm at the anterior more flattened, pole considerably larger than those posterior to the blastopore, this difference being noticable throughout the entire development. The embryo bends ventrally and the entoderm becomes constricted into two sections, the anterior, the germ of the œsophagus together with the sucking stomach and the posterior, the germ of the true stomach. The ectodermal cells of the anterior and ventral walls increase in number and size, representing respectively the starting points of the eyes and ventral nerve chain. The hind gut, extending dorso-ventrally, represents the third division and is in open communication with the blastopore. In the ensuing stage the blastopore becomes closed and later the true anus breaks through in the same place.

Up to this stage the embryo has consisted of but the two primary germ layers. The mesoderm develops as paired cœlomic pouches from the Archenteron, the first pair appearing at the posterior end of the embryo forming the fourth segment, the second pair in the anterior end giving rise to the first segment, the third pair in the second segment

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Morph. Jahrbuch., Bd., XXII, 1895.

and the fourth pair in the third segment. In addition to these the first pair of mesodermal pouches, right and left of the first pair of appendages divide and give rise to a pair of head pouches. The gonad develops as a dorsal evagination of the archenteron between the second and third segments and later pushes itself forward into the region of the second segment.

There develops further, in the region between the stomach and midgut an unpaired accessory sexual gland and at the same stage there is developed a larger pair of evaginations of the midgut which the author designates as the midgut glands. The salivary glands develop as ectodermal invaginations of the head segment. The author does not consider the musculature, the nerves, or the transformation of the cœlomic pouches, reserving those points for another paper.

After a careful consideration of historical and comparative points, in which he discusses the results and views of the various authors who have published papers on the tardigrades and presents his own ideas on the questions concerned, the author, in closing, hopes that what he has contributed to a knowledge of the morphology of the tardigrades will be sufficient to give them a place at the bottom of the arthropod stem. He does not maintain that the tardigrades represent the stem form of the arthropods but that they have branched off early and at the very bottom of the arthropod phylum and in many respects developed partially, but a considerable number of primitive characters remain which seem to show that they are transitional forms to other phyla.

In a second paper by the same author[3] the earlier embryonic stages of *Macrobiotus macronyx* Duj. are described as follows.

Contrary to condition found in the terrestrial tardigrades, in the species studied, the males are equal to the females in number. The males are smaller by half than the females, the latter appearing brownish-yellow in color owing to the eggs in the ovary which in a ripe condition attains a considerable size. The author was unable to distinguish a copulatory apparatus as described by Graff and the manner of fertilization precludes the existence of such an organ. The female withdraws her body into the chitinous envelope so that the hinder part is clear as far as the second pair of appendages and the eggs are extruded into this cavity through the anus. The hinder end of the chitinous shell of the female is turned in for a short distance, forming a short tube.

During copulation the female moves about dragging the male clinging to her back. The male deposits the spermatazoa near the posterior

[3] Biologisches Centralblatt., 15.

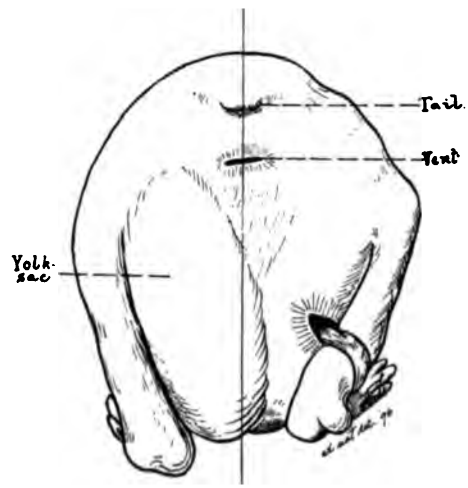
end of the female and they are sucked in through the tube at the posterior end of the female by a sort of pumping motion maintained by some peculiar muscular action on the part of the female.

The maturation stages viewed externally and in section present the usual phenomena with one peculiarity, the formation of *four* polar globules instead of three, the second globule extruded dividing in the same manner as the first, thus giving rise to the additional body.

The first division of the egg presents a two cell stage, the second division, a three cell stage, the third division a four cell stage followed by the eight, sixteen and thirty-two cell stages. In the four cell stage only two of the four cells are in contact, the former being somewhat oblique to the long axis.

The egg membrane is a product of the egg itself, and probably derived from the alveolar layer. In the young the appendicular glands, opening through tubes between the claws are much larger than in the adult and consist in great part of the cœlomic pouches. In contrast to the terrestrial tardigrades *Macrobiotus macronyx* does not revive after desiccation.—F. D. LAMBERT.

An abnormal chick.—In a brood of chicks there occurred one case in which there was an opening in the abdominal wall on the right side through which extended what seemed to be a loop of the intestine. The loop ligatured the tibio-tarsus just above the ankle joint.



Upon dissection, however, it appeared the loop was nothing more nor less than a region of the yolk sac constricted by twisting. The

(smaller) portion of the yolk—sac thus constricted off was darker and had begun to undergo decay. The opening in the abdominal wall is nearly oval, 9 mm. long by 7 mm. broad. The edges are smooth as if about a natural opening.

A glance at the accompanying figure will reveal a certain amount of asymmetry, due mostly to the position of the yolk sac.

The youngster was hatched alive. How long he might have lived, or whether the opening would have healed cannot be said, as the owner did not give it a chance.

In all probability the condition just described was caused by some inadvertent movement of the embryo, thus displacing the yolk sac or a part of it.—Francis E. Lloyd.

PSYCHOLOGY.

A Study in Morbid Psychology, with some Reflections. —When Descartes uttered his famous aphorism, "*Cogito, ergo sum,*" he reasonably flattered himself on having made an irrefutable proposition. But the "abysmal depths of personality" are not as easily sounded as the great French philosopher imagined. Certainly *something* thinks; but is it one consciousness or more that is represented in one human brain? The celebrated experiments of Professor Janet, of Havre, led to the discovery of no less than three distinct personalities in his patient, Madam B., and the no less noted cases of Félida X. and of Louis V.[1] show one or more personalities controlling the same brain. And there are epileptiform and hypnotic states where all the functions of civilized society are discharged without the consciousness of the ordinary primary self.

I will now proceed to describe the case, which forms the main subject of this article—that of Ansel Bourne, a carpenter and itinerant preacher of Rhode Island. The experiences of Ansel Bourne are amongst the most curious to be met with in the annals of morbid psychology. Whether the symptoms of this case are due to epilepsy —"masked epilepsy"—or post-epileptic mental disturbance, they are equally worthy a study.

Ansel Bourne, who is of New England parentage, was born in New York City, July 8, 1826, and worked steadily at his trade as a carpen-

[1] Note.—See article "Double Consciousness," in the Dictionary of Psychological Medicine, edited by D. Hack Duke, M. D., where cases are given, including those of Louis V. and Félida X. (cases 4 and 7).

ter till his thirty-first year. At that time, as the result of some strange experiences ending with the direct promptings of a hallucinatory (?) voice, he gave up his trade, and spent some thirty years as an itinerant preacher or "evangelist." At the time of his extraordinary seizure he was about 61 years old, and, except for some mental disturbance caused by the objection of his second wife to his work as an itinerant preacher, he seems to have been in excellent health.

On January 17, 1887, he went from his home in Coventry, Rhode Island, to Providence, in order to draw money from the bank to pay for a farm he had arranged to buy. He left his horse at Greene Station in a stable, expecting to return that afternoon from the city. He drew out of the bank $551, and paid several bills, after which he went to his nephew's store, 121 Broad Street, and then started to go to his sister's house. This was the last that was known of his doings at that time. He did not appear at his sister's house, and did not return to Greene, where his horse remained for about three weeks, till it was taken away by Mrs. Bourne.

On Thursday, Jan. 20, the following paragraph appeared in the *Bulletin*, of Providence, R. I., the information having been given by police:

A MISSING PREACHER.

"This morning, Mrs. Bourne, the wife of Ansel Bourne, of Greene Station, called at police headquarters and reported that her husband had been missing since Monday last. Rev. Mr. Bourne is quite widely known as an evangelist, and during the past twenty-five years he has carried on his religious work in various parts of the United States. For some years, it is said, he has been subject to attacks of a peculiar kind, which rendered him temporarily insensible, and on some occasions he has remained in an unconscious state for many hours. . . . . . Mr. Bourne was in Providence on Monday, but he did not return to his home, and has not been heard of since."

Notwithstanding the publicity given to the fact of his disappearance, no tidings whatever were received of him till about March 14, eight weeks later. The account of the morning of March 14 was furnished to Dr. Weir Mitchell (one of the many medical men interested in this extraordinary case) by Surgeon-General L. H. Read, who was summoned to examine Ansel Bourne on the morning of March 14, soon after he regained his ordinary waking consciousness.

It appears that Ansel Bourne arrived at Norristown, Pa., about Feb. 1, 1887, *i. e., two weeks after* his disappearance from Providence, R. I. Under the name of A. J. Brown he rented a store room at 252

Main Street, from Mr. Pinkston Earle, and divided the room into two by means of curtains. The rear portion of the room he filled with furniture and used as a "general living" apartment, sleeping and preparing his meals there. The front portion of the room he stocked with miscellaneous goods, toys, confectionery, etc. These he purchased and paid for in Philadelphia, which he visited each week with the purpose of replenishing his stock. He fastened a sign to his window reading A. J. Brown. Had he known Latin enough to say so he might truly have exclaimed, "Cogito ergo sum!" only the particular ego which was thinking would unhesitatingly have called itself A. J. Brown!

The room which he rented was part of a house in which the Earle family were dwelling, but although they came into daily contact with "Mr. Brown," there was nothing in his manner or proceedings which suggested anything peculiar. He was quiet in his behavior, precise and regular in his habits, and paid his bill promptly. He was especially punctual in the closing of his store at 9 P.M. on ordinary week days and at 10 P.M. on Saturdays. He attended the Methodist church on Sunday, and on one occasion, at a religious meeting, he related an incident he said he had witnessed on a steamer years previously, on the passage from Albany to New York, and his remarks were thought particularly relevant to the point under consideration. In short, none of the persons who had any dealings with him conceived any suspicion that he was in any unusual condition.

On the morning of Monday, March 14, about five o'clock, he heard, he says, an explosion like the report of a gun or a pistol, and waking, he noticed there was a ridge in his bed not like the bed he had been accustomed to sleep in. He noticed the electric light opposite his windows. He rose and pulled away the curtains and looked out on the street. He felt very weak, and thought he had been drugged. His next sensation was that of fear, knowing that he was in a place where he had no business to be. He feared arrest as a burglar. He says this is the only time in his life he ever feared a policeman.

The last thing he could remember before waking was seeing the Adams Express wagons at the corner of Dorrance and Broad Streets in Providence, on his way from the store of his nephew in Broad Street to his sister's residence in Westminster Street, on January 17.

He waited to hear some one move, and for two hours he suffered great mental distress. Finally he tried the door, and finding it fastened on the inside, opened it. Hearing some one moving in the next room, he rapped at the door. Mr. Earle opened it and said, "Good morning, Mr. Brown." B.: "Where am I?" E.: "You're all

right." B.: "I'm all wrong. My name is'nt Brown. Where am I?" E.: "Norristown." B.: "Where's that?" E.: "In Pennsylvania." B.: "What part of the country?" E.: "About 17 miles west of Philadelphia." B.: "What time in the month is it?" E.: "The 14th." B.: "Does time run backwards here? When I left home it was the 17th." E.: "17th of what?" B.: "17th of January." E.: "It is the 14th of March."

Mr. Earle thought Mr. "Brown" was out of his mind, and said that he would send for a doctor. He summoned Dr. Louis H. Read, to whom Mr. Bourne told the story of his doings at Rhode Island, and how he remembered nothing between the time of seeing the express wagons on Dorrance Street and waking up that morning March 14th. "These persons," he said, "tell me I am in Norristown, Pennsylvania, and that I have been here six weeks, and that I have lived with them all the time. I have no recollection of ever having seen one of them before this morning." He requested Dr. Read to wire to his nephew, Andrew Harris, at 121 Broad Street, Providence, R. I. Dr. Read telegraphed:" Do you know Ansel Bourne? Please answer." The reply came: "He is my uncle. Wire me where he is, and if well."

Later this nephew came up to Norristown, sold the goods in the store by auction, and settled up the business affairs of "Mr. Brown," who, as Ansel Bourne, travelled back with him to Rhode Island. Dr. Read adds, in the account of the case, which he furnished to Dr. Weir Mitchell; "He said he was a preacher and farmer, and could not conceive why he should have engaged in a business he knew nothing about and never had any desire to engage in. When asked about his purchasing and paying for goods, and paying freight bills, he said he had no recollection of any such transactions."

The family with whom he lived say that after the occurrence of that morning he was greatly changed. He was annoyed at any reference to his store, and never entered it afterwards. He became despondent, took no food, was unable to sleep, and became greatly prostrated, both physically and mentally.

Whether or no this was a case of "masked epilepsy," no one familiar with the peculiarities of the hypnotic state can fail to see the likeness between the experience of Ansel Bourne and that of patients who have purposely been kept for considerable periods under the influence of hypnotic suggestion. Such patients, when aroused from the hypnotic trance have never any recollection of the time which has elapsed since they were "put to sleep," although in the interim they have been carrying on the ordinary business of life as if the whole "ego" were acting.

Early in 1890, Professor James, of Harvard, hearing of the case, conceived the idea that if Mr. Bourne could be hypnotized, a complete history of the whole incident might be obtained from him whilst in the hypnotic trance. The circumstances had naturally left a painful and perplexed impression on Mr. Bourne; he was anxious to have any light possible thrown on his strange experience, and he readily acquiesced in the proposals made for hypnotism.

Here, it must be noted, that *no amount of suggestion, however strongly urged or frequently repeated, ever succeeded in merging the consciousness of "Albert Brown" in that of "Ansel Bourne;"* the one personality was absolutely separated from the other.

Ansel Bourne came to Boston on five consecutive days, May 27–31, and during that time Professor James and Mr. Hodgson obtained from him, in the "deep" hypnotic state, the following detailed account of his doings during the eight weeks from January 17 to March 13, 1887.[2]

He said that his name was Albert John Brown, that on January 17, 1887, he went from Providence to Pawtucket in a horse-car, thence by train to Boston, and thence to New York, where he arrived at 9 P. M. and went to the Grand Union Hotel, registering as A. J. Brown. He left New York on the following morning and went to Newark, N. J., thence to Philadelphia, where he arrived in the evening, and stayed for three or four days in a hotel near the depot. He then spent a week or so in a boarding house in Filbert Street, about No. 1115, near the depot. It was kept by two ladies, but he could not remember their names. He thought of taking a store in a small town, and after looking round at several places, among them Germantown, chose Norristown, about twenty miles from Philadelphia, where he started a little business in five cent goods, confectionery, stationery, etc.

He stated that he was born in Newton, New Hampshire, July 8, 1826 [he was born in New York City, July 8, 1826], had passed through a great deal of trouble, losses of friends and property; loss of his wife was one trouble, she died in 1881; three children living, but everything was confused prior to his finding himself in the horse-car on his way to Pawtucket; he wanted to get away somewhere, he did'nt know where, and have rest. He had six or seven hundred dollars with him when he went into the store. He lived very closely, boarded by himself, and did his own cooking. He went to church and also to one prayer-meeting. At one of these meetings he spoke about a boy who

[2] Professor Janet, of Havre, discovered accidentally that by inducing a deeper condition of hypnotic trance, a personality can be "tapped" which would otherwise be unknown.

had kneeled down and prayed for the passengers on a steamboat from Albany to New York.[3]

He had heard of the singular experience of Ansel Bourne, but did not know whether he had ever met Ansel Bourne or not. He had been a professor of religion himself for many years, belonged to the "Christian" denomination, but back there everything was mixed up. He used to keep a store at Newton in New Hampshire, and was engaged in lumber and trading business; had never been previously taken up with the business which took him to Norristown. He kept the Norristown store for six or eight weeks. How he got away from there was all confused; since then it has been a blank. The last thing he remembered about the store was going to bed on Sunday night March 13, 1887. He went to the Methodist church in the morning, walked out in the afternoon, stayed in his room in the evening and read a book.[4]

During the enquiry, one of the most remarkable phenomena is the utter failure of suggestion to combine the Bourne with the Brown state, thereby demonstrating that suggestion is *not* the principal factor in hypnotism. I will give two instances:

At 11.45 A. M., May 31, Mr. Hodgson hypnotizes Bourne, and, after a couple of minutes, says, "What's your name? It's Bourne, is'nt it?" "No, it's Brown." Mr. Hodgson wakes him up and tries the same experiment again, with the same result: at the first touch of trance he is Brown. Other experiments were made on succeeding days to connect the two personalities, but vainly. On July 7, at 10 P. M., Ansel Bourne was entranced by Mr. Hodgson who *tells him he will remain Ansel Bourne after being hypnotized.* In vain; he passes at once into the Brown state. Mr. Hodgson then enumerates the chief events of Bourne's life, telling "Brown" that he is "Bourne," and that he remembers these events. This is repeated several times, and Mrs. Bourne and Professor James reiterate the same circumstances. "Brown," however, reaches nothing more than a faint remembrance of the year of his birth, of his first marriage and of the death of his first wife. It seems doubtful, though, if these remembrances were not connected with the "Brown" state, because "Brown" always gave the date of his birth (though not the place) correctly, and remembered he had had a wife who was dead.

[3] An experience of his real life.

[4] A detailed account of the questions and answers in this enquiry is given, but would take up too much space here.

It would take too long to recapitulate all the evidence collected by Mr. Wm. Romaine Newbold, Lecturer on Psychology in the University of Pennsylvania, in verification of Ansel Bourne's statements whilst in trance. At the Kellogg House, where "Brown" stayed for about two weeks before going to Norristown, he was well remembered by the colored waiter Jackson, and by Mrs. Kellogg. They described him as a very quiet man, who said he was a carpenter and came from "down east, somewhere." Every day he used to go out and look out for a suitable place to begin business in. After a while he came one day and said he had found just the place for him, and that was Norristown. Then he bought goods for the store he intended to open there, all of which goods he left in Jackson's care. "Seemed perfectly himself," and never gave any reasons for wishing to commence business here. Afterwards, Jackson and Mrs. Kellogg had seen accounts in the papers and recognized the man there referred to as the man who had stayed with them. They thought, however, he had become crazy after leaving them, but it never occurred to them that there had been anything wrong with him whilst with them.

I will now give an account, as briefly as I can, of the curious experiences which befel Ansel Bourne when he was about thirty years of age; experiences which were accounted for by the medical man who attended him (Dr. Thurston, of Westerly, R. I.) as the results of sunstroke, and by the people in the village where he lived as "Wonderful Works of God."

Ansel Bourne, as already stated, was of New England parentage, and, up to the age of thirty-one, was a hard working carpenter who, from being a member of the Baptist church, became a "convinced atheist," not of the aggressive sort, but "silent and stubborn." It must be noted that this "atheism" in a man of scanty education must have been of the shallowest sort, and that beneath the surface lay depths of Calvinistic ancestry and training. He had conceived a rooted aversion for a sect calling itself the "Christian" church, and for one of its ministers who was his near neighbor.

In August, 1857, he had several attacks of sickness, brought on possibly by working in extremely hot weather, and these attacks culminated in a fit of unconsciousness which lasted from Sunday, the 16th of August, till the following Tuesday, when he became conscious of his condition, but remained in a critical state for some days. The next two months were passed in renewed attempts to work, and fresh attacks of illness, though of a less serious character than those of August. On Sunday, the 25th of October, he spent the day and evening at his

own house playing cards—a horrible crime for the Calvinist conscience which was lying ready to revenge itself!

[5]On the 28th of October, Ansel Bourne started for the village of Westerly, and was noticed by some neighbors to be walking fast, as though feeling quite well. He was conscious of no unusual feelings till the thought came vividly into his mind that he ought to go to meeting (*i. e.*, to church). Mentally he enquired, "Where?" The inner voice replied, "To the 'Christian' chapel." To this idea his spirit rose in bitter opposition, and he said within himself, "I would rather be struck deaf and dumb forever than to go there." A few minutes after he felt giddy, and sat down on a stone by the wayside to rest. He saw an old man in the distance approaching him with a wagon, and immediately after felt as though some powerful hand drew down something over his head and face and finally over his whole body, depriving him of his sight, his hearing and his speech, and leaving him perfectly helpless. Yet, he declares, he had as perfect a power of thought as at any time in his life, and the awful choice he had made (that he would rather be deaf and dumb forever than go to the Christian chapel) came with awful significance before him. His whole mind was full of agonizing horror and dread of the God he thought he had so irretrievably offended. He was conscious of being taken up in the wagon; of being carried into a house and placed in a chair, and then of being put in bed.

Dr. Thurston, who was summoned immediately, says that on reaching his patient's bedside he "found him perfectly insensible . . . . the pupils of his eyes quite insensible of light, widely dilated and not contracting on the application of sudden and vivid light." The patient himself, however, constantly maintained that he was entirely conscious. "About him," he says, "all was as silent as though there were neither a God, nor life, nor motion in the whole, wide universe. The silence was as though the soul had been cast into a deep, bottomless and shoreless sepulchre, where dismal silence was to reign eternally." He fully acknowledged the justice of God and spurned from his soul the thought of insulting God by asking mercy for such a sinner.

Powerful counter-irritants were applied, and by Friday consciousness was partially restored for external things. He felt the posts of his bedstead and the window near, and was satisfied he was in his own house; he felt movements on the bed and recognized the caresses of his little children; then, about 26 hours after the attack, his power of sight suddenly returned. He saw his wife and a neighbor, and made signs that

[5] From an account written under the direction of Ansel Bourne.

he wanted pen and paper. An internal voice asked "if he were willing to forgive those he had injured?" and he immediately answered in the affirmative. He expressed in writing a wish to see the minister of the "Christian" church and another neighbor with whom he had been on bad terms. Both came and treated the sufferer with kindness and sympathy; and then when he was reconciled with his brother men, he felt emboldened to approach God and offered up "unutterable prayer."

A prayer-meeting was held in Ansel Bourne's house, and he wrote saying he was determined thenceforth to be on the Lord's side. On November 11th, just two weeks from the time of his seizure, he was carried to the Christian chapel, and though unable to speak or hear, he endeavored to signify his altered feelings to the congregation by standing and holding up his hands. He also wrote a very touching message to be delivered for him by the minister. He was requested by the minister, after his second visit to the chapel, to stand up in the pulpit, and here suddenly his hearing returned—in his own words, "Every manner of sound that comes from the living things of nature broke upon his ears" . . . . his tongue was unloosed instantly, and he exclaimed, in the hearing of the whole congregation, "Glory to God and the Lamb forever!" It is needless to say that this scene, and the moving exhortation from the convert which followed, caused the deepest emotion in the congregation.

From that day onward, until the 17th of January, 1887, Ansel Bourne's faculties were unimpaired. But two weeks after the restoration of his speech and hearing in chapel, he had a "vision" which commanded him to "Settle your worldly business and go to work for me." This vision came back several times in the same night, and the result of all these experiences was that Ansel Bourne became an "evangelist," and for more than thirty years went about preaching, attending at revivals and performing strenuously all the offices of an unattached minister. At the wish of his second wife, whom he married in 1882, he gave up his itinerant preaching; and he thinks the distress of mind, caused by leaving what he considered the path of duty, may have led to the strange mental experiences which I have already described.—Alice Bodington.

(*To be Continued.*)

A Match-Striking Bluejay.—The note in the November, 1895, Naturalist, concerning the striking of matches by one of the monkeys (*Cebus*) has just fallen under my notice.

It may interest the readers of the Naturalist to know that a neighbor of mine once had a little bluejay (*Cyanocitta cristata* (Linn.)) which

had acquired the same habit, but confined exclusively to the so-called "parlor" or "popping" matches. I never knew how he acquired the habit—perhaps accidentally, by striking them with the beak or beating them against some hard substance as he did much of his food.

When given a match he always hopped to a chair-round and struck it almost directly downward, fulminate "end on," and if it did not explode at once his blows were repeated rapidly until it ignited. He would then drop it, spring away and watch it wonderingly while it burned. All matches about the house had to be kept from him. He knew them by their odor, and would tear open packages to get them out. On one occasion his mistress came in and found him with a box from which he had ignited nearly three dozen.

—James Newton Baskett.

Mexico, Mo.

ANTHROPOLOGY.[1]

Professor Holmes Studies of Aboriginal Archictecture in Yucatan.—Professor W. H. Holmes in his recent visit to the Islands on the east coast of Yucatan, the sites of Chichen Itza, Izamal and Uxmal and certain shell heaps, near Progreso (See Archeological Studies among the ancient cities of Mexico, by W. H. Holmes. Field Columbian Museum Publication 8. Chicago 1895) has presented us with a valuable and characteristically clear summary of the important architectural features of the Peninsular ruins.

Eschewing archaeological investigation in such directions as those of implements, pottery, metals, art, food, burial, etc., he fixes our attention upon the stones used in building, the manner of dressing and laying them and the purpose of completed structures. The details of this subject casually referred to by Charnay and Waldeck and in the unindexed pages of Stephens, are summed up to together with certain origiual observations and arranged in order, until we see the relationship, in purpose that characterizes the ruined structures in the region. No demonstration has yet been made as to the kind of tools used in carving the limestone of the facades and Professor Holmes like all previous travellers, leaves the question unanswered. Neither does he refer to Mr. McGuires' theory that the work was done with round hammerstones. But a block fortunately found at Chichen Itza, pecked on

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

the surface with a pointed instrument and lined off for edge dressing with a flat edged tool, is shown as an interesting illustration (Fig. 1) of

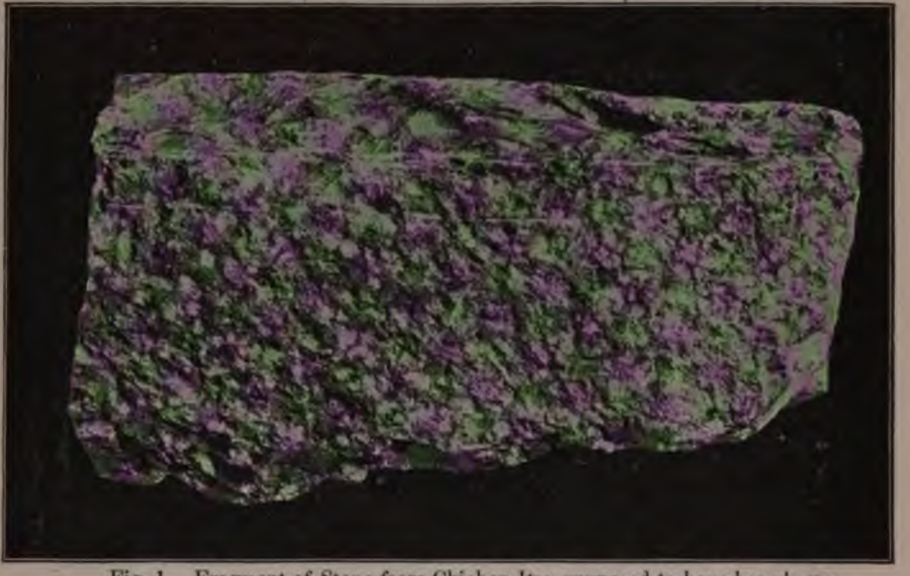


Fig. 1. Fragment of Stone from Chichen Itza, supposed to have been hewn by the ancient masons of Yucatan, the tools used are unknown, but we see the peckings of *a pointed implement* on the dressed side, and the long cuts of *an edged tool* along the upper margin.

the effect on stone of the kind of tool we are hunting for. Until we find the implement, however, we may believe on early Spanish authority, that hard copper was used, or imagine adzes and chisels of stone as we please, while we recognize with Professor Holmes the importance of ransacking the sites of quarries, where the innumerable blocks (20,000 carved on the facade of the "Governors House", at Uxmal alone) were procured.[2] Happily chosen general observations give a clearness to the whole presentation, and the delightful yet confused and complex impression of the ruins left upon the mind by the accounts of travelers becomes simple in the colder light of Professor Holmes systematic observations. The reader continually thanks him as he would thank the compiler of an index to a work of many volumes. Such characteristic general features as the ignorance of a master principle of mason craft like joint binding, the feeble grasp of the

[2] Captain Theobert Maler informed me in Ticul in 1895, that he had seen several such quarries.

PLATE VIII.

Fig. 4. Miniature portal of a small temple on the Island of Cozumel, a little entrance only 4 feet 6 inches high, to a diminutive building not over 20 feet square by 10 to 12 high resting on a terrace about 5 feet high, of the two round columns supporting the stone lintels, one is carved to represent a kneeling human figure. From a photograph by Mr. E. H. Thompson.

PLATE IX.

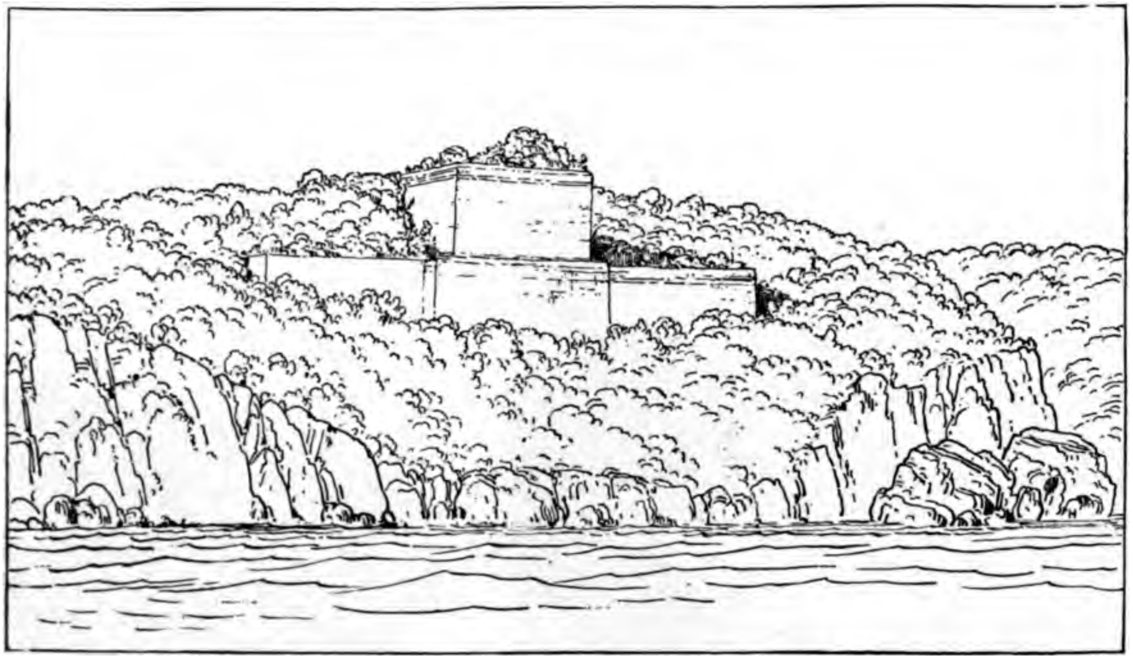


Fig. 5 Ruins of Tuloom as seen from the Sea.
The building fronts inland and its rear walls facing the water are without doors and windows.

facade upon the structure where no long stones project from the pudding like hearting within into the face, to clinch the crust to the mass, the V shape and consequent lack of catch of many of the facing

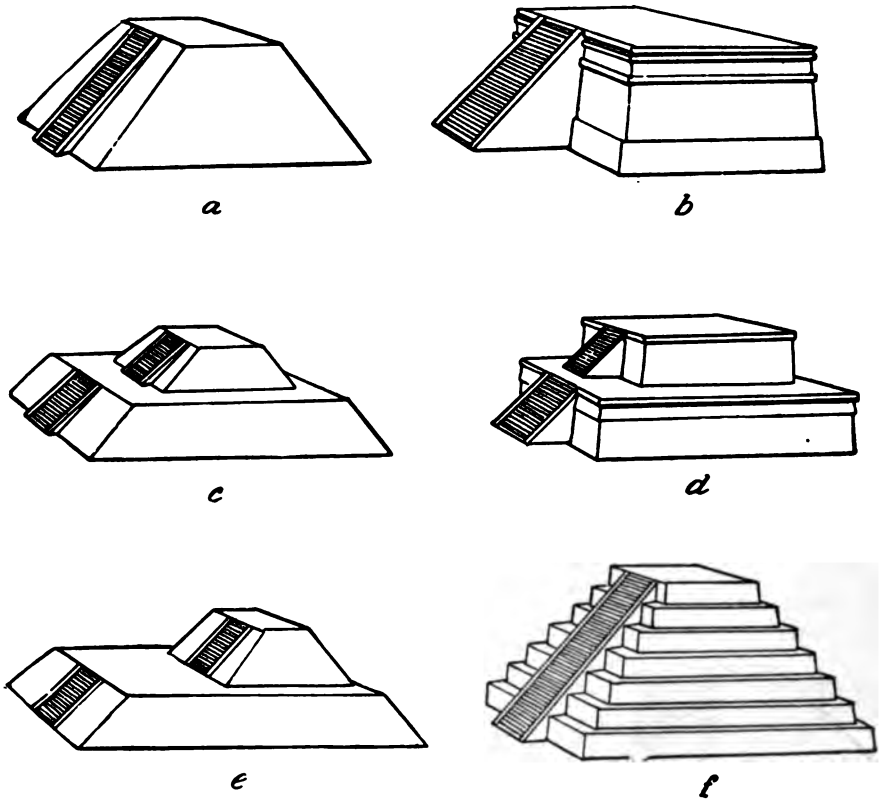


Fig. 2. Examples of Terraces and Pyramids, superstructures omitted.

stones, are dwelt upon in order, and a series of sketches disposed to catch the attention and impress the memory, show the varying forms of tumuli, (Fig. 2) the generally rectangular ground plan of buildings, (Fig. 3) and the construction of the arch by the edging in of opposing walls.

A question of much interest is touched upon when Professor Holmes in the introduction, refers to the geological age of the rock floor of the region in question, since the chance for establishing conclusions in Yucatan as to man's existence in geologically ancient times diminishes according as we learn that the Peninsula was too long under water

to count as an early human foothold. My statement (See Hill Caves of Yucatan. Lippincott, Phila., 1895, p. 21) referring to the rocks of Yucatan as of Mesozoic Age, is at variance with the recent observations of geologists, while Professor Holmes says on the other hand, (p. 18): "The massive beds of limestone of which the Peninsula is formed contain and are largely made up of the remains of the marine forms of life now flourishing, along the shores. Fossil shells obtained from the rocks in various parts of the country are all of living species and represent late Pliocene or early Plistocene times, thus possibly bringing the date of the elevation of Yucatan down somewhat near that of the reputed sinking of Atlantis, some eleven or twelve thousand years ago, or not far from the period that witnessed the oscillations attending the glacial period." Though true that the peninsular limestone is largely composed of existing marine forms we learn on

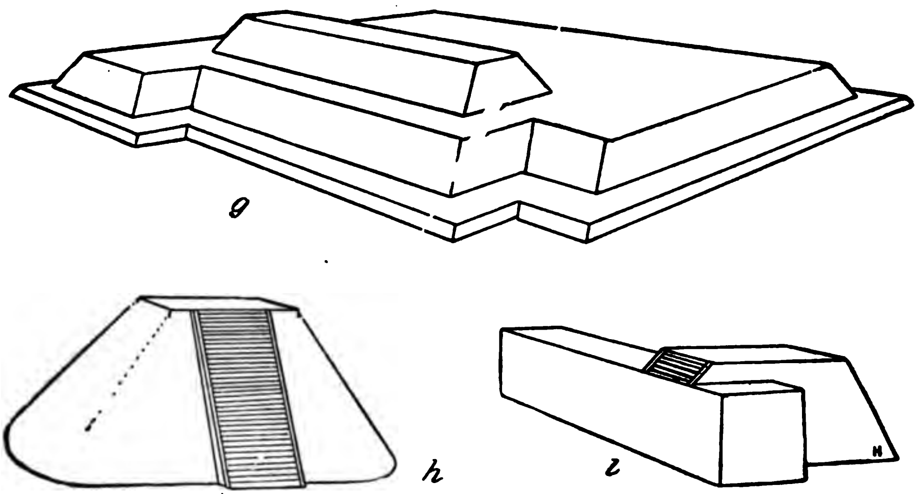
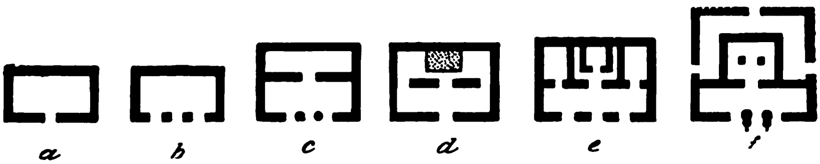


Fig. 2. Examples of Terraces and Pyramids, superstructures omitted.

closer examination that it is not entirely so, and that the shells are not *all* modern. We find that the full list of age denoting fossil mollusca collected from the rocks of Yucatan by the expedition in 1891 of the Academy of Natural Sciences of Philadelphia (See Geol. Researches in Yucatan, by Prof. Angelo Heilprin, Proc. Acad. Nat. Sci. 1891. p. 136) does not characterize the Yucatan rock as of Plistocene Age while the recent researches of geologists (Prof. J. W. Spencer makes the Niagara Gorge 32,000 years old) now tend to add to the antiquity of the Glacial Epoch. Professor Heilprin who conducted the Yucatan

expedition informs me that " the fossil shells are not *all* recent species since even the level plains about twenty to twenty five miles from the coast contain fossil mollusca (Amusium mortoni, from Cenotes near Merida, Turritella perattenuata and Turritella apicalis from R. R. cut one-half mile east of Tekanto. Ostrea meridionalis and Arca species undetermined, from a digging near Merida and Lucina disciformis) not now known to be living, and which make part of the Floridian formation (the typical Pliocene of the United States). Furthermore in the Sierra which contains the caves, a number of fossil forms have been found the determination of which is rather doubtful, but which



a b c d e f

Fig. 3. The various kinds of ground plans used in Maya (ancient Yucatecan) temples.

(*a*) Single chamber building with plain door.

(*b*) Single chamber temple with wide doorway and two square columns.

(*c*) Two chamber temple with wide doorway and round columns and the Sanctuary with single plain doorway.

(*d*) Two chamber temple, the vestibule with simple doorway and the Sanctuary with three doorways and a low altar.

(*e*) Four chamber temple Palenque type, the vestibule with three entrances and two squarish piers, the Sanctuary with tablet chamber, and two small lateral chambers.

(*f*) Three chamber temple, Chichen Itza type, the vestibule entered by wide portel with two serpent columns, this Sanctuary enlarged by introducing two square columns to support the triple vault, and a long gallery with three doorways extending behind.

may be of early Pliocene or even of Miocene Age." Professor Pilsbry of the Academy of Natural Sciences and Mr. C. W. Johnson of the Wagner Institute say further after examination of the shell bearing rock specimens brought home by the Expedition above mentioned and now in the Academy of Natural Sciences, that " the shells indicate late Pliocene but by no means Plistocene Age, the presence of several characteristic Pliocene species Turritella (2 species) Fulgur rapum. Pecten eboreus Amusium mortonii, and Ostrea meridionalis preventing the possibility of the rocks being assigned to a later Epoch than the Pliocene while the fossils extinct and still existing considered together, indicate that the formation was contemporaneous with the Floridian formation of Prof. Heilprin."

In the second part of the volume a talent for lucid simplification impresses us in novel panoramic views of Uxmal and Chichen Itza, when stationed upon an imaginary height, we view the arrangement of walls and mounds clear of obscuring masses of leafage and rubbish, add to this something of the ever delightful charm of the landscape painter in sketches illustrating the course of expedition along the east coast, as we follow it from the Isle of women (Mujeres) to Tuloom, and from Cozumel to Cancun and El Meco. Looking from water to land we seem to see the tropical distance taking on its mirage like garb of coolness, and by grotesque pinnacles of rock, hear the rush of green waves upon the sands, where mysterious walls set softly in the deceitful blue allure us from the shore.—HENRY C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Nova Scotian Institute of Science.—May 11, the following papers was read: Notes on the Geology of Newfoundland, by T. C. Weston, Esq., F. G. S. A., Ottawa; Phenological Observations for 1895, by A. H. McKay, Esq., LL. D., F. R. S. C., Superintendent of Education; Glacial Succession in Central Lunenburg, by W. H. Prest, Esq., Chester Basin, N. S.; On the Flora of Newfoundland, No. 3, by Rev. Arthur C. Waghorne, New Harbour, Newfoundland; Notes on Nova Scotian Zoology, No. 4, by Harry Piers, Esq.; Water Supply of the Towns of Nova Scotia—Financial, Sanitary and other Considerations, by W. R. Butler, Esq., M. E., Professor of Mathematics, Natural Philosophy and Engineering, King's College, Windsor; On the Broad Cove Coal Field, by W. H. Ross, Esq., C. E.—HARRY PIERS, *Secretary.*

Boston Society of Natural History.—The Annual Meeting was held Wednesday evening, May 6th. The following business was transacted: Reports of the Curator, Secretary, Librarian, Treasurer and Trustees; Announcement of the award of the Walker Prize for 1896; Election of Officers for 1896-97. The following paper was read: Prof. Charles S. Minot: On the Principles of the Construction of the Microtomes. There was shown a collection of the microtomes illustrating the evolution of the instrument, and also a microtome of a new model.

May 20.—The following papers were read: Prof. E. S. Morse: Man as a Tertiary Mammal; Dr. G. A. Dorsey: On the Photograph and Skeleton of Neddy Larkin, a native of Australia.—SAMUEL HENSHAW, *Secretary.*

American Philosophical Society.—April 17.—Dr. D. G. Brinton read an obituary notice of Henry Hazelhurst.

May 1st.—A Symposium on the Factors of Organic Evolution was held. Three stated papers were followed by open discussion. The papers were read by Prof. E. D. Cope, who approached the subject from the standpoint of paleontology; Prof. E. G. Conklin, who discussed it from the embryologic point of view; and Prof. L. H. Bailey, who adduced the facts of botany in support of his conclusions. Dr. D. G. Brinton discussed the papers previously read.

May 15th.—Prof. A. H. Smyth read an obituary notice of Henry Phillips, Jr. Prof. E. D. Cope read two papers, entitled Sixth Contribution to the History of the Miocene Vertebrata of N. A.; and Second contribution to the history of the Cotylosauria.

Philadelphia Academy of Natural Sciences.—May 8th.—Anthropological Section.—Papers were read by Dr. M. V. Ball on "Tattooing among Convicts." and by Dr. H. Allen on "Ethnic Bearing of the Classification of the Hand."—CHAS. P. MORRIS, *Recorder*.

The Academy of Science of St. Louis.—At the meeting of May 18, 1896, Professor C. M. Woodward presented a critical examination of some of the mathematical formulæ employed by Herbart to represent mental phenomena, in which these formulæ were criticised as inadequate. Though not considering any formulæ likely to be adequate, from the nature of the case, the speaker offered a substitute for the Herbart formula pertaining to the bringing into consciousness of a sub-latent concept through the suggestion afforded by another concept similar in some respects while differing in others.

Dr. A. N. Ravold made a report on the use in St. Louis of diphtheria antitoxine, prepared by the Health Department of the city. During the past winter, 342 cases of diphtheria had been treated with this serum, by 93 physicians. Doses of from 2.5 to 106 cc. had been administered. As a rule, the recovery was far slower when the quantity used was small than when a larger quantity was employed. Usually the serum was administered only once. In about half the cases a decided change for the better was noticeable within 24 hours, and these cases were practically cured within 48 hours, although attention was called to the fact that for some weeks the throat of a convalescent is a breeding-place for the diphtheritic bacilli, the virulence of which did not seem to be diminished by the serum treatment. Of the cases reported on, 9.06 per cent. only, died, and as a considerable number of cases were hopeless when treatment was administered, the patients dying

within 24 hours thereafter, it was considered fair to deduct these deaths from the total, which reduced the mortality to 4.6 per cent. when the serum was administered in the earlier stages of the disease. The injurious consequences of administering the serum were fully considered, but held to be practically insignificant. It was also stated that when used on persons who had been exposed to but had not manifested the disease, the serum proved an unfailing means of conferring immunity for a certain period of time. Among the advantages in the use of this serum was mentioned that of lessening the chances of secondary infection, so frequent after an attack of diphtheria.

A committee presented resolutions on the death of Dr. Charles O. Curtman, for many years a member of the Academy.—WM. TRELEASE, *Recording Secretary.*

SCIENTIFIC NEWS.

The Biological Laboratory of the Brooklyn Institute of Arts and Sciences will open at Cold Spring Harbor, Long Island, July 3d, 1896 for its seventh session. As in the previous years Prof. Herbert W. Conn, of Wesleyan University is the Director. This year he is assisted by Prof. H. T. Fernald who gives instruction in Embryology, Prof. H. S. Pratt who takes charge of general zoology, Dr. D. S. Johnson, instructor in Botany, Dr. Edward L. Rice Assistant in Biology and W. H. C. Pyncheon instructor in photography. The session lasts six weeks but students, upon special arrangement, can remain longer. The Institute now possesses five buildings for the use of the laboratory, a good equipment of the apparatus necessary for collecting and for investigation and can accommodate about sixty students. The laboratory fees are as follows: The laboratory fee, including any one course of instruction, the general lectures and the use of the laboratory privileges is $20.00. For each additional course of instruction an additional fee of $5.00 is charged. The fee for the course in elementary zoology is $15.00. Board is furnished for $4.50 a week; rooms from $1.50 to $3.00 a week. The total expense for the session is thus from $55.00 to $75.00. For circulars and other information address, Prof. H. W. Conn, Middleton, Ct.

The second Annual Meeting of the Botanical Society of America will be held in Buffalo, N. Y., on Friday and Saturday August 21 and 22, 1896. The Council will meet at 1.30 p. m. on Friday, and the Society will be called to order at 3 p. m. by the retiring President, Dr.

William Trelease, Director of the Missouri Botanical Garden. The President-elect, Dr. Charles E. Bessey, Professor of Botany in the University of Nebraska will then take the chair. The afternoon session will be devoted to business. At the evening session the retiring President will deliver a public address on "Botanical Opportunity." The sessions for the reading of papers will be held on Saturday at 10 a. m. and 2 p. m. The Botanical Society of America is affiliated with the American Association for the Advancement of Science whose sessions this year begin on Monday August 24th, in Buffalo.

At the Springfield Meeting of the American Association for the Advancement of Science, it was voted that the Sectional Committee of Section E be directed to prepare a program for the meeting of the Section, and to transmit the same to the Permanent Secretary for printing and distribution not less than one month before the meeting.

It is therefore requested that members intending to present papers at the Buffalo Meeting of the Association send title and abstract of the same to the Secretary of the Section on or before July 15, 1896.

A. C. GILL, *Secretary Section E.*

Prof. S. P. Langley states that the Smithsonian Institution has decided to rent a table at the Naples Zoological Station for another period of three years, for the benefit of American students. The interest manifested by American educators and biologists in this matter has convinced the Institution that in so doing it best cooperates with the educational institutions of the United States.

About July 1st next a party under the direction of Mr. T. H. Mobley will start from Lacomb, Alberta, for a two year exploring trip through northern Canada, taking in all points of interest between Edmonton and the Arctic Sea. There are four in party, all of which are thoroughly experienced men, of whom two are naturalists.

Dr. H. Baumhauer, formerly of Lüdingshausen, goes to the University of Freibourg, Switzerland, as ordinary professor of Mineralogy, while Dr. O. Mügge, of Münster, goes to a similar position in the University of Königsberg.

Two of the honors received by Prof. Leuckart in connection with his fifty year doctor-jubilee were elections to honorary membership in the Zoological Society of France and the Russian Academy of Sciences.

Dr. G. B. Grassi, of Catania, well-known for his researches on the structure of the lower Arthropods has been called to the University of Rome as professor of comparative anatomy.

Mr. F. E. Willey, formerly of the Kew Botanical Gardens, has gone to Sierra Leone, West Africa, as director of the botanical gardens there.

Prof. O. Bütschli, of Heidelberg, has been elected president of the German Zoological Society for the years 1896 and 1897.

Dr. M. von Lenhossék has been advanced to the position of professor extraordinarius in the University of Tubingen.

Dr. Dannenberg is now privat docent for Mineralogy and Geology in the technical school at Aix-la-Chapelle.

A. Quadri, Professor of Zoology in the University of Siena, Italy, died December 25th, 1895.

Dr. G. Karsten, of Leipzig, goes to the University of Kiel as Private docent in Botany.

Mr. H. T. Wharton, a well known English ornithologist, died recently at the age of 50.

Prof. F. Chatin, has been elected Vice-President of the French, Academy of Sciences.

Dr. O. L. zur Strassen is now private docent for Zoology in the University of Leipzig.

Dr. A. Weiss has been appointed assistant in Mineralogy in the University of Greifswald.

Dr. Jean Müller, Diréctor of the Botanical gardens of Geneva, is dead at the age of 68.

Dr. F. von Wagner, of Graz, goes to Giessen as assistant in the zoological institute.

Dr. A. N. Beketow, of St. Petersburg, has resigned from the chair of botany there.

Dr. P. Vuillemin has been appointed to the chair of botany at Nancy, France.

Dr. P. Voglino has been appointed Docent in Botany in the University of Turin.

Dr. F. Dahl, of Kiel, has gone to New Guinea, to study the flora and fauna.

Dr. P. Knuth, of Kiel, has received the title of Professor of Botany.

J. B. Wilson, botanist, of Gerlong, Victoria, died October 22, 1895.

Dr. L. Jacoby, ichthyologist, of Zürich, Switzerland, is dead.

The botanist Prof. K. Rattlef, died recently.

THE

AMERICAN NATURALIST

VOL. XXX. July, 1896. 355

THE CLASSIFICATION OF DIATOMS (BACILLARIACEÆ).[1]

BY CLARENCE J. ELMORE.

There have been many systems of classification employed for the *Bacillariaceæ*, but very few of these have any valid claim, to be regarded as natural systems. They may be divided into three classes; (1) those based on the structure of the valves, of which Kuetzing's, Prof. H. L. Smith's, and that employed by Kirchner are examples; (2) those based on the form of the frond, the connecting membrane, and the gelatinous envelope, represented by Rev. Wm. Smith's; and (3) those based on the structure of the endochrome and the manner of forming auxospores, represented by that of Paul Petit. The following is a brief outline of the systems mentioned.

Kirchner divided the *Bacillariaceæ* into two groups[2] those whose markings are bilateral, that is, arranged on two sides of a longitudinal line or raphe, and[3] those with radial markings.

[1] Read before the Botanical Seminar of the University of Nebraska, March 21. 1896.

[2] Kryptogamen-Flora von Schlesien; Algen, 171. Breslau, 1878.

[3] Species Algarum, 1. Leipsic, 1849.

Those with bilateral markings he divided into two subdivisions the first comprising those with a central nodule, and the second those with none.

Kuetzing divided the *Bacillariaceæ* into three tribes; I, *Striatæ*, that is those with transverse striations; II, *Vittatæ*, that is those with longitudinal stripes; and III, *Areolatæ*, that is those whose surfaces are divided into angular areolæ. The first-two tribes, *Striatæ* and *Vittatæ* he divided into two orders each, viz.; I, *Astomaticæ* and II, *Stomaticæ*. The *Astomaticæ* included those with no central nodule, or as he understood it, with no central opening, while the *Stomaticæ* included those with a central nodule. If the central nodule were really a stoma or aperture as Kuetzing considered it, this grouping might have been a natural one; for this difference in structure might have connoted important physiological differences, but it is generally conceded that the nodules are merely markings on the valves, and it is likely that they indicate nothing as to the physiology of the plant. So no higher groups than genera, or possibly species, can be based on this character. His third tribe, *Areolatæ*, he also divided into two orders; I, *Disciformæ* that is, those of a circular or angular form, and, II, *Appendiculatæ*, or forms with appendages, as *Biddulphia*.

The classification of Prof. H. L. Smith[4] is one that has had considerable following. Bessey's Botany[5] was the first American textbook to adopt and give an outline of the system. It was adopted by Van Heurck[6], Wolle[7] and De Toni[8]. To say the least, it is a good practical system of classification, and probably this is the most that can be said for it, though in some points it seems to approach a natural system. Smith divides the Diatoms into three tribes, the *Raphideæ*, *Pseudoraphideæ*, and *Cryptoraphideæ*. The *Raphideæ* are all supposed to possess a raphe. The *Pseudoraphideæ* are usually elongated, have no raphe, but in its place there is a blank space resembling a

[4] Conspectus of the Families and Genera of the Diatomaceæ in *The Lens*, I: 1 1872 and II: 65, 1873.

[5] Botany for High Schools and Colleges, Henry Holt and Co, New York, 1880,

[6] Synopsis des Diatomées de Belgique, 1885.

[7] Diatomaceæ of North America, 1890.

[8] Sylloge Algarum, 1891.

raphe. The *Cryptoraphideæ* are usually circular or angular and have nothing resembling a raphe. Upon the supposition that the raphe is an essential organ, and that it is present in one tribe, replaced by another structure in the second, and "hidden" in the third, this might be a natural classification. But if the raphe is known to exist only in the first tribe and its existence in the others is wholly theoretical, it will hardly serve as a character on which to base a classification. It is true that the genera brought together by this system appear to bear more or less relation to each other, but if we knew as little about Phanerogams as we do about Diatoms, we should think that a division of them into *Arboræ*, *Frutices*, and *Herbæ* placed related genera together, for it would be easy to see that *Salix* and *Populus* are related, and also that *Solanum* and *Physalis* are more or less closely allied. I venture to regard the *Raphideæ*, *Pseudoraphideæ*, and *Cryptoraphideæ* as having no greater naturalness than the divisions *Arboræ*, *Frutices*, and *Herbæ;* and it is to be hoped that they will soon be consigned to the same botanical limbo in which the latter have long since found obscurity.

It is true, however, that in the *Raphideæ*, there seems to be a trace of naturalness in the system. The author begins with the bilaterally symmetrical forms, that is those in which the raphe is a median line, as for example, *Navicula*. Those with the raphe at one side of the center, as in *Cymbella*, he considers a modification of the first type by a curving of the frustule and thus bringing the raphe nearer the concave side. And in the third division the raphe has approached so near to the concave margin that it fuses with it, as in *Amphora*. If this is to be considered simply as a modification of a typical form, it means little. But if this modification shows the course of development from the *Navicula* form to the *Amphora* form, it means a great deal. In *Navicula* and *Cymbella* two auxospores are formed from two mother cells without conjugation, and in *Amphora* two auxospores are formed from two mother cells by conjugation. It is probable that the method of reproduction found in the derived form is a development from that found in the primitive form. If then the *Amphora* form has developed from the *Navicula* form, there is reason to believe that the for-

mation of auxospores without conjugation is the primitive method, although Murray[9] holds that the formation of auxospores by conjugation is probably the original method, and that their formation without conjugation is the derived method.

Wm. Smith[10] divided the Diatoms into two tribes in the first of which the frustules are free, and in the second imbedded in a gelatinous envelope. Under the first tribe he makes five subtribes, depending upon the form of the connecting membrane and the relation of the frustules to each other. The second tribe he divided into four subtribes based on the form of the fronds. This arrangement seems not only extremely artificial but also very impractical. Nothing about Diatoms is more variable than the form of the fronds; and where it is at all constant, such a system places closely related genera far apart; for example, *Cymbella* and *Encyonema*, *Nitzschia* and *Homœocladia* are placed in separate tribes, while in structure they are very similar, the main difference being that in *Encyonema* and *Homœocladia* the frustules are arranged in rows, while in *Cymbella* they are free or stipitate and in *Nitzschia* they are free. This method of classifying Diatoms may be likened to a separation of Grasses into those forming a dense sod and those not forming a sod; or of Dicotyledons into those exuding a resinous fluid and those that do not. Wm. Smith places *Gomphonema* in his first tribe, that is, the one having no gelatinous envelope; but some species of *Gomphonema* are stipitate while others are enclosed in an amorphous mass of jelly. The latter species would have to be placed in his second tribe, thus dividing the genus. It would lead to even greater difficulty than this, for the same species is sometimes stipitate and sometimes imbedded in a gelatinous envelope.

Of all existing systems that of Paul Petit[11] seems to approach

[9] An Introduction to the Study of Seaweeds, p. 195, 1895.

[10] For a synopsis of Smith's classification see Pritchard's History of the Infusoria, 101, fourth edition, 1861.

[11] Liste des Diatomées et des Desmidées observées dans les Environs de Paris precedée d'un essai de classification des Diatomées. Bull. Soc. Bot. France, tom. XXIII–XXIV, Paris, 1877.

An Essay on the Classification of the Diatomaceæ translated by F. Kitton, Monthly Microscopical Journal and Transactions of the Royal Microscopical Society, XVIII, 1877, pp. 10, 65.

Pfitzer, Die Bacillariaceen, in Schenk's Handbuch der Botanik, Breslau, 1882.

most nearly to a natural one because it is based on characters having physiological significance. It is based primarily on the structure of the endochrome, and secondarily on the method of forming auxospores and the general shape of the frustules. Van Heurck does not employ this system in his *Synopsis* because of the large number of fossil specimens and those from deep-sea soundings to which it could not be applied. But this is not a valid objection, for all the genera are represented by modern species, and these are sufficient for a basis of classifications, and since the specific characters are based mainly on the structure of the valves, there will be no trouble with the fossil forms. The following synopsis of Petit's system includes the higher divisions only.

I. *Bacillariaceæ coccochromaticæ.*

With numerous endochrome granules.

- A. Frustules concentrically constructed. One mother cell forming asexually a single auxospore. *Melosireæ*, etc.
- B. Frustules bilateral, one or two mother cells forming two auxospores, as far as known asexually. *Fragilarieæ*, etc.

II. *Bacillariaceæ placochromaticæ.*

With one or two large endochrome plates.

- A. One endochrome plate lying against the convex valve; one mother cell forming one auxospore asexually. *Cocconeideæ.*
- B. A single endochrome plate extending diagonally across the cell cavity, or lying next the girdle. Two auxospores formed from two mother cells, with or without conjugation. *Nitzschieæ. Amphoreæ, Cymbelleæ*, etc.
- C. Two endochrome plates lying next the two valves. Two mother cells forming two auxospores by conjugation. *Eunotieæ, Synedrieæ, Surirayeæ.*
- D. Two endochrome plates lying next the two girdle bands; two mother cells forming two auxospores without conjugation. *Amphipleureæ, Naviculeæ*, etc.

Although Petit's system is by no means perfect, it is at least a step in the right direction. He bases it upon characters that have some physiological significance, while the other systems are wholly or in greater part based on merely accidental characters. A clue to the genetic relationships of Diatoms, as of other plants, will be most certainly found in their method of reproduction. The shape of the frustules, or their markings, will serve for specific, or in some cases for generic characters, but they have no significance that will warrant their use in the erection of higher groups. Absolute shape and size will not serve as definite characters, for a single species between one auxospore stage and the next varies greatly in both these respects. Owing to the peculiar mode of cell division in which each new valve is formed inside the old one, each new frustule is smaller than the parent, hence the size gradually decreases until an auxospore is formed. Schumann[12], out of 470 species found ten in which the length of the largest was five times that of the smallest; twenty-nine in which the largest were from three to four times as long as the smallest, and the rest showing less variation. The variation in form is even as great as the variation in size. This is probably due to the difference in the thickness of the girdle, *i. e.* the part of the valves that overlaps, in different parts of the frustule. *Navicula iridis* Ehr. is a good example of a variable species. Its different forms have been described as species by most writers. In the typical form the valves are elliptical with gracefully curved margins. The first variation from this type has apices cuneate, and a still further deviation shows them acuminate-cuneate; and from this it varies to rostrate or capitate; and a diminution in size goes step by step with this change in form. These forms are represented by *Navicula iridis* Ehr., *N. amphigomphus* Ehr., *N. affinis* Ehr., *N. amphirhynchus* Ehr., and *N. producta* W. Sm. If the overlapping portions of the valves are slightly thicker near the ends than elsewhere, this variation would be the necessary result, for each new valve formed inside an old one would be slightly constricted opposite this thickened place, at first changing the rounded ends to cuneate, and as the narrowing pro-

[12] Pfitzer, l. c., p. 441.

ceeded still further, the cuneate form would become rostrate and a still further narrowing would give a capitate form. So form and size, although they have a certain significance, are not to be considered infallible characters.

The geological records throw no light upon the relationship of the *Bacillariaceæ*, for when this family first appeared, we find the same genera, and largely the same species as in our modern ones. This is probably due to the fact that their ancestors lacked the siliceous covering, and hence were not preserved. Diatoms evolved the same as all other plants until they developed their shells, but these put a stop to their further evolution, at least they show no trace of evolution since their first appearance. So the question arises whether the Diatoms represent the ends of several closely related genetic lines the further development of which was stopped by their siliceous shells, or whether we may trace the development of one form from another. The former supposition is the more probable, for the form of the earliest fossil specimens is identical with that of modern specimens of the same species; and the same genera are found among fossil as among modern Diatoms. If one genus of Diatoms developed from another, we ought to find the more primitive forms in the earlier strata, for there is little chance that their remains would not be preserved had they existed. But instead of this, Diatoms of all forms appear almost simultaneously. We may conclude then that the *Bacillariaceæ* represent the silicified ends of several closely allied genetic lines and that they have not changed in form since they acquired their siliceous covering. The structure of the valves it follows will tell us practically nothing of their relationship.

There are five methods by which auxospores are formed[13]. In the first the protoplasm of one frustule simply escapes from the valves, grows to a certain size, and then invests itself with new valves. In the second, two auxospores, instead of one, are formed in the same way by the dividing of the protoplasm of a single plant. In the third, the protoplasm of two Diatoms unites to form an auxospore. In the fourth, the protoplasm of

[13] Murray, l. c.

two Diatoms emerges from the valves, and placed by side, but without conjugation, forms each an auxospore. In the fifth, two Diatoms divide transversely and the two halves of each conjugate, each half with the corresponding half of the other and thus form two auxospores. Before any truly natural classification can be made the significance of these various modes of producing auxospores must be understood. Whether the sexual or the asexual method is the primitive one must be known, or whether the different methods are so many expedients to overcome the difficulties imposed upon these plants by their siliceous shells. At present our knowledge of the structure and physiology of Diatoms is not sufficient to enable us to construct a perfectly natural system of classification, and until something better is proposed, Petit's may well be adopted, for although it is not wholly natural, it is more so than any which has preceded it.

A NEW FACTOR IN EVOLUTION.

By J. Mark Baldwin.

(*Continued from page 451*).

III.

Social Heredity.—There follows also another resource in the matter of development. In all the higher reaches of development we find certain co-operative or "social" processes which directly supplement or add to the individual's private adaptations. In the lower forms it is called gregariousnes, in man sociality, and in the lowest creatures (except plants) there are suggestions of a sort of imitative and responsive action between creatures of the same species and in the same habitat. In all these cases it is evident that other living creatures constitute part of the environment of each, and many neuro-genetic and psycho-genetic accommodations have reference to or involve these other creatures. It is here that the principle of imitation gets tremendous significance; intelligence and vol-

ition, also, later on; and in human affairs it becomes social co-operation. Now it is evident that when young creatures have these imitative, intelligent, or quasi-social tendencies to any extent, they are able to pick up *for themselves*, by imitation, instruction, experience generally, the functions which their parents and other creatures perform in their presence. This then is a form of ontogenetic adaptation; it keeps these creatures alive, and so produces determinate variations in the way explained above. It is, therefore, a special, and from its wide range, an extremely important instance of the general principle of Organic Selection.

But it has a farther value. *It keeps alive a series of functions which either are not yet, or never do become, congenital at all.* It is a means of extra-organic transmission from generation to generation. It is really a form of heredity because (1) *it is a handing down of physical functions*; while it is not physical heredity. It is entitled to be called heredity for the further reason (2) that *it directly influences physical heredity in the way mentioned*, i. e., it keeps alive variations, thus sets the direction of ontogenetic adaptation, thereby influences the direction of the available congenital variations of the next generation, and so determines phylogenetic development. I have accordingly called it "Social Heredity" (ref. 2, chap. xii; ref. 3).

In "Social Heredity," therefore, we have a more or less conservative, progressive, ontogenic atmosphere of which we may make certain remarks as follows:—

(1) *It secures adaptations of individuals all through the animal world.* "Instead of limiting this influence to human life, we have to extend it to all the gregarious animals, to all the creatures that have any ability to imitate, and finally to all animals who have consciousness sufficient to enable them to make adaptations of their own; for such creatures will have children that can do the same, and it is unnecessary to say that the children must inherit what their fathers did by intelligence, when they can do the same things by intelligence" (ref. 6).

(2) *It tends to set the direction of phylogenetic progress* by Organic Selection, Sexual Selection, etc., *i. e.*, it tends not only

to give the young the adaptations which the adults already have, but also *to produce adaptations which depend upon social coöperation ; thus variations in the direction of sociality are selected and made determinate.* "When we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits by others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Wherever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected" (ref. 3). Why is it, for example, that a race of Mulattoes does not arise faster, and possess our Southern States? Is it not just the social repugnance to black-white marriages? Remove or reverse *this influence of education, imitation, etc.*, and the result *on phylogeny* would show in our faces, and even appear in our fossils when they are dug up long hence by the paleontologist of the succeeding aeons!

(3) *In man it becomes the law of social evolution.* "Weismann and others have shown that the influence of animal intercourse, seen in maternal instruction, imitation, gregarious coöperation, etc., is very important. Wallace dwells upon the actual facts which illustrate the 'imitative factor,' as we may call it, in the personal development of young animals. I have recently argued that Spencer and others are in error in holding that social progress demands use-inheritance; since the socially-acquired actions of a species, notably man, are socially handed down, giving a sort of 'social heredity' which supplements natural heredity" (ref. 4). The social "sport," the genius, is very often the controlling factor in social evolution. He not only sets the direction of future progress, but he may actually lift society at a bound up to a new standard of attainment (ref. 6). "So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of social

life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood, the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit. When social life begins, we find the beginning of the artificial selection of the unfit; and this negative principle begins to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress in two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and second, by putting a direct premium on variations which are socially available" (ref. 3).

4. The two ways of securing development in determinate directions—the purely extra-organic way of Social Heredity, and the way by which Organic Selection in general (both by social and by other ontogenetic adaptations) secures the fixing of phylogenetic variations, as described above—seem to run parallel. Their conjoint influence is seen most interestingly ingly in the complex instincts (ref. 4, 5). We find in some instincts completely reflex or congenital functions which are accounted for by Organic Selection. In other instincts we find only partial coördinations ready given by heredity, and the creature actually depending upon some conscious resource (imitation, instruction, etc.) to bring the instinct into actual operation. But as we come up in the line of phylogenetic development, both processes may be present *for the same function;* the intelligence of the creature may lead him to do consciously what he also does instinctively. In these cases the additional utility gained by the double performance accounts for the duplication. It has arisen either (1) by the accumulation of congenital variations in creatures which already performed the action (by ontogenetic adaptation and handed it down socially), or (2) the reverse. In the animals, the social

transmission seems to be mainly useful as enabling a species to get instincts slowly in determinate directions, by keeping off the operation of natural selection. Social Heredity is then the lesser factor; it serves Biological Heredity. But in man, the reverse. Social transmission is the important factor, and the congenital equipment of instincts is actually broken up in order to allow the plasticity which the human being's social learning requires him to have. So in all cases both factors are present, but in a sort of inverse ratio to each other. In the words of Preyer, "the more kinds of co-ordinated movement an animal brings into the world, the fewer is he able to learn afterwards." The child is the animal which inherits the smallest number of congenital co-ordinations, but he is the one that learns the greatest number (ref. 2, p. 297).

"It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the same point of view has been reached by Weismann and others on the purely biological side. The instinctive equipment of the lower animals is replaced by the plasticity for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of inheritance of acquired characters as factor in development" (ref. 7).

"Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of dependence, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view" (Ref. 3).

But these two influences furnish a double resort against Neo-Lamarkism. And I do not see anything in the way of considering the fact of Organic Selection, from which both these resources spring, as being a sufficient supplement to the principle of natural selection. The relation which it bears to natural selection, however, is a matter of further remark below (V).

"We may say, therefore, that there are two great kinds of influence, each in a sense hereditary; there is *natural heredity* by which variations are congenitally transmitted with original endowment, and there is '*social heredity*' by which functions socially acquired (*i. e.*, imitatively, covering all the conscious acquisitions made through intercourse with other animals) are also socially transmitted. The one is phylogenetic; the other ontogenetic. But these two lines of hereditary influence are not separate nor uninfluential on each other. Congenital variations, on the one hand, are kept alive and made effective by their conscious use for intelligent and imitative adaptations in the life of the individual; and, on the other hand, intelligent and imitative adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarkian factor" (ref. 4).

"The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Lamarkism argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn anything new, just because their nervous systems are not in the mobile condition represented by high consciousness. They have instinct and little else" (ref. 3).

IV.

The Process of Organic Selection.—So far we have been dealing exclusively with facts. By recognizing certain facts we have

reached a view which considers ontogenetic selection an important factor in development. Without prejudicing the statement of fact at all we may enquire into the actual working of the organism is making its organic selections or adaptations. The question is simply this: how does the organism secure, from the multitude of possible ontogenetic changes which it might and does undergo, those which are adaptive? As a matter of fact, all personal growth, all motor acquisitions made by the individual, show that it succeeds in doing this; the further question is, how? Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in development, is not involved in the solution of the further question as to how the adaptations are secured. But from the answer to this latter question we may get further light of the interpretation of the facts themselves. So we come to ask how Organic Selection actually operates in the case of a particular adaptation of a particular creature (ref. 1; ref. 2, chap. vii, xiii; ref. 6, and 7).

I hold that the organism has a way of doing this which is peculiarly its own. The point is elaborated at such great length in the book referred to (ref. 2) that I need not repeat details here. The summary in this journal (ref. 6) may have been seen by its readers. There is a fact of physiology which, taken together with the facts of psychology, serves to indicate the method of the adaptations or accommodations of the individual organism. The general fact is that the organism concentrates it energies upon the locality stimulated, for the continuation of the conditions, movements, stimulations which are vitally beneficial, and for the cessation of the conditions, movements, stimulations, which are vitally depressing and harmful. In the case of beneficial conditions we find a general *increase of movement, an excess discharge of the energies of movement in the channels already open and habitual; and with this, on the psychological side, pleasurable consciousness and attention.* Attention to a member is accompanied by increased vasomotor activity, with higher muscular power, and a *general dynamogenic heightening in that member.* "The thought of a

movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement" (ref. 3). By this organic concentration and excess of movement many combinations and variations are rendered possible, from which the advantageous and adaptive movements may be selected for their utility. These then give renewed pleasure, excite pleasurable associations, and again stimulate the attention, and *by these influences the adaptive movements thus struck are selected and held as permanent acquisitions.* This form of concentration of energy upon stimulated localities, with the resulting renewal by movements of conditions that are pleasure-giving and beneficial, and the subsequent repetitions of the movements, is called the "circular reaction."[4] (ref. 1, 2). It is the selective property which Romanes pointed out as characterizing and differentiating life. It characterizes the responses of the organism, however low in the scale, to all stimulations—even those of a mechanical and chemical (physico-genic) nature. Pfeffer has shown such a determination of energy toward the parts stimulated even in plants. And in the higher animals it finds itself exactly reproduced in the nervous reaction seen in imitation and—through processes of association, substitution, etc.—in all the higher mental acts of intelligence and volition. These are developed phylogenetically as variations whose direction is constantly determined, by this form of adaptation in ontogenesis. If this be true—and the biological facts seem fully to confirm it—this is the adaptive process in all life, and this process is that with which the development of mental life has been associated.

It follows, accordingly, that the three forms of ontogenetic adaptation distinguished above—physico-genetic, neuro-genetic, psycho-genetic—all involve the sort of response on the part of the organism seen in this circular reaction with excess discharge; and we reach one general law of ontogenetic adaptation and of Organic Selection. "The accommodation of an organism to a new stimulation is secured—not by the selection of this stimulation beforehand (nor of the necessary move-

[4] With the opposite (withdrawing, depressive affects) in injurious and painful conditions.

ments)—but by the reinstatement of it by a discharge of the energies of the organism, concentrated as far as may be for the excessive stimulation of the organs (muscles, etc.) most nearly fitted by former habit to get this stimulation again (in which the " stimulation " stands for the condition favorable to adaptation). After several trials the child (for example) gets the adaptation aimed at more and more perfectly, and the accompanying excessive and useless movements fall away. This is the kind of selection that intelligence does in its acquisition of new movements" (ref. 2, p. 179; ref. 6).

Accordingly, *all ontogenetic adaptations are neurogenetic.*[5] The general law of " motor excess " is one of *overproduction;* from movements thus overproduced, adaptations survive; these adaptations set the determinate direction of ontogenesis; and by their survival the same determination of direction is set in phylogenesis also.

The following quotation from an earlier paper (ref. 7) will show some of the bearings of this position:

" That there is some general principle running through all the adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions;* that all new functions must be reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—i. e., by the selection of fit movements from excessively produced movements, that is, from *movement variations.* So granting this, we now have the further question :

[5] Barring, of course, those violent compelling physical influences under the action of which the organism is quite helpless.

How do these movement variations come to be produced *when and where they are needed?*[6] And with it, the question: How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected?

"Now these two questions are the ones which the biologists fail to answer. But the force of the facts leads to the hypotheses of "conscious force," "self-development" of Henslow and "directive tendency" of the American school—all aspects of the new Vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the *second* of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others.

"But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the *first* question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for—the *movement variations when and where they are required.* The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose; this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital move-

[6] This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of 'Germinal Selection' (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as it is to variations in separate organisms? As against "germinal selection," however, I may say, that in the cases in which ontogenetic adaptation sets the direction of survival of phylogenetic variations (as held in this paper) the hypothesis of germinal selection is in so far unnecessary. This view finds the operation of selection *on functions in ontogeny* the means of securing "variations when and where they are wanted;" while Weismann supposes competing germinal units.

ments wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis in vital movements always. Healthy, overflowing, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which the theory of selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give the excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should do the reverse, *i. e.*, draw off energy and suppress movement.[7]

"If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the "Circular Reaction," in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since

[7] It is probable that the origin of this antithesis is to be found in the waxing and waning of the nutritive processes. "We find that if by an organism we mean a thing merely of contractility or irritability, whose round of movements is kept up by some kind of nutritive process supplied by the environment—absorption, chemical action of atmospheric oxygen, etc.—and whose existence is threatened by dangers of contact and what not, the first thing to do is to secure a regular supply to the nutritive processes, and to avoid these contacts. But the organism can do nothing but move, as a whole or in some of its parts. So then if one of such creatures is to be fitter than another to survive, it must be the creature which by its movements secures more nutritive processes and avoids more dangerous contacts. But movements toward the source of stimulation keep hold on the stimulation, and movements away from contacts break the contacts, that is all. Nature selects these organisms; how could she do otherwise? . . . . We only have to suppose, then, that the nutritive growth processes are by natural selection drained off in organic expansions, to get the division in movements which represents this earliest bifurcate adaptation." (Ref. 2, p. 201).

it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.'"

The advantages of this view seem to be somewhat as follows:

1. It gives a method of the individual's adaptations of function which is *one in principle with the law of overproduction and survival now so well established in the case of competing organisms.*

2. It reduces nervous and mental evolution to strictly parallel terms. The intelligent use of phylogenetic variations for functional purposes in the way indicated, puts a premium on variations which can be so used, and thus sets phylogenetic progress *in directions of constantly improved mental endowment.* The circular reaction which is the method of intelligent adaptations is liable to variation in a series of complex ways which represent phylogenetically the development of the mental functions known as memory, imagination, conception, thought, etc. We thus reach a phylogeny of mind which proceeds in the direction set by the ontogeny of mind,[8] just as on the organic side the phylogeny of the organism gets its determinate direction from the organism's ontogenetic adaptations. And since it is the one principle of Organic Selection working by *the same functions* to set the direction of both phylogenies, the physical and the mental, the two developments are not two, but one. Evolution is, therefore, not more biological than psychological (ref. 2, chap. x, xi, and especially pp. 383–388).

3. It secures the relation of structure to function required by the principle of "use and disuse" in ontogeny.

4. The only alternative theory of the adaptations of the individual are those of "pure chance," on the one hand, and a "creative act" of consciousness, or the other hand. Pure chance is refuted by all the facts which show that the organism does not wait for chance, but goes right out and effects new adaptations to its environment. Furthermore, ontogenetic

[8] Prof. C. S. Minot suggests to me that the terms "ontopsychic" and "phylopsychic" might be convenient to mark this distinction.

adaptations are determinate; they proceed in definite progressive lines. A short study of the child will disabuse any man, I think, of the "pure chance" theory. But the other theory which holds that consciousness makes adaptations and changes structures directly by its *fiat*, is contradicted by the psychology of voluntary movement (ref. 4, 6, 7). Consciousness can bring about no movement without having first an adequate experience of that movement to serve on occasion as a stimulus to the innervation of the appropriate motor centers. "This point is no longer subject to dispute; for pathological cases show that unless some adequate idea of a former movement made by the same muscles, or by association some other idea which stands for it, can be brought up in mind the intelligence is helpless. Not only can it not make new movements; it can not even repeat old habitual movements. So we may say that intelligent adaptation does not create coördinations; it only makes functional use of coördinations which were alternatively present already in the creature's equipment. Interpreting this in terms of congenital variations, we may say that the variations which the intelligence uses are alternative possibilities of muscular movement" (ref. 4). So the only possible way that a really new movement can be made is *by making the movements already possible so excessively and with so many varieties of combination, etc., that new adaptations may occur.*

5. The problem seems to me to duplicate the conditions which led Darwin to the principle of natural selection. The alternatives before Darwin were "pure chance" or "special creation." The law of "overproduction with survival of the fittest" came as the solution. So in this case. Let us take an example. Every child has to learn how to write. If he depended upon chance movements of his hands he would never learn how to write. But on the other hand, he can not write simply by willing to do so; he might will forever without effecting a "special creation" of muscular movement. What he actually does is to *use his hand in a great many possible ways as near as he can to the way required*; and from these excessively produced movements, and after excessively varied and numerous trials, he gradually selects and fixes the slight successes made

in the direction of correct writing. It is a long and most laborious accumulation of slight Organic Selections from over-produced movements (ref. for handwriting in detail, 2, chap. v; also 2, pp. 373, ff.).

6. The only resort left to the theory that consciousness is some sort of an *actus purus* is to hold that it *directs* brain energies or selects between possible alternatives of movement; but besides the objection that it is as hard to direct movement as it is to make it (for nothing short of a force could release or direct brain energies), we find nothing of the kind necessary. The attention is what determines the particular movement in developed organisms, and the attention is no longer considered an *actus purus* with no brain process accompanying it. The attention is a function of memories, movements, organic experiences. We do not attend to a thing because we have already selected it, or because the attention selects it; but *we select it because we—consciousness and organism—are attending to it.* "It is clear that this doctrine of selection as applied to muscular movement does away with all necessity for holding that consciousness even directs brain energy. The need of such direction seems to me to be as artificial as Darwin showed the need of special creation to be for the teleological adaptations of the different species. This need done away, in this case of supposed directive agency as in that, the question of the relation of consciousness to the brain becomes a metaphysical one, just as that of teleology in nature became a metaphysical one; and it is not to much profit that science meddles with it. And biological as well as psychological science should be glad that it is so, should it not?" (ref. 6; and on the metaphysical question, ref. 7).

V.

A word on the relation of this principle of Organic Selection to Natural Selection. Natural Selection is too often treated as a positive agency. It is not a positive agency; it is entirely negative. It is simply a statement of what occurs when an organism does not have the qualifications necessary to enable it to survive in given conditions of life; it does not in any way

define positively the qualifications which do enable other organisms to survive. Assuming the principle of Natural Selection in any case, and saying that, according to it, if an organism do not have the necessary qualifications it will be killed off, it still remains in that instance to find what the qualifications are which this organism is to have if it is to be kept alive. So we may say that *the means of survival is always an additional question* to the negative statement of the operation of natural selection.

This latter question, of course, the theory of variations aims to answer. The positive qualifications which the organism has arise as congenital variations of a kind which enable the organism to cope with the conditions of life. This is the positive side of Darwinism, as the principle of Natural Selection is the negative side.

Now it is in relation to the theory of variations, and not in relation to that of natural selection, that Organic Selection has its main force. Organic Selection presents *a new qualification of a positive kind* which enables the organism to meet its environment and cope with it, while natural selection remains exactly what it was, the negative law that if the organism does not succeed in living, then it dies, and as such a qualification on the part of the organism, Organic Selection presents several interesting features.

1. If we hold, as has been argued above, that the method of Organic Selection is always the same (that is, that it has a natural method), being always accomplished by a certain typical sort of nervous process (*i. e.*, being always neuro-genetic), then we may ask whether that form of nervous process—and the consciousness which goes with it—may not be a variation appearing early in the phylogenetic series. I have argued elsewhere (réf. 2, pp. 200 ff. and 208 ff.) that this is the most probable view. Organisms that did not have some form of selective response to what was beneficial, as opposed to what was damaging in the environment, could not have developed very far; and as soon as such a variation did appear it would have immediate preëminence. So we have to say either that selective nervous property, with consciousness, is a variation,

or that it is a fundamental endowment of life and part of its final mystery. "The intelligence holds a remarkable place. It is itself, as we have seen, a congenital variation; but it is also the great agent of the individual's personal adaptation both to the physical and to the social environment" (ref. 4).

"The former (instinct) represents a tendency to brain variation in the direction of fixed connections between certain sense-centers and certain groups of coördinated muscles. This tendency is embodied in the white matter and the lower brain centers. The other (intelligence) represents a tendency to variation in the direction of alternative possibilities of connection of the brain centers with the same or similar coördinated muscular groups. This tendency is embodied in the cortex of the hemispheres" (ref. 4).

2. But however that may be, whether ontogenetic adaptation by selective reaction and consciousness be considered a variation or a final aspect of life, it is a *life-qualification of a very extraordinary kind.* It opens a new sphere for the application of the negative principle of natural selection upon organisms, *i. e.*, with reference to *what they can do*, rather than to what they are; to the new use they make of their congenital functions, rather than to the mere possession of the functions (ref. 2, pp. 202 f.). A premium is set on congenital plasticity and adaptability of function rather than on congenital fixity of function; and this adaptability reaches its highest in the intelligence.

3. It opens another field also for the operation of natural selection—still viewed as a negative principle—through the survival of particular overproduced and modified reactions of the organism, by which the determination of the organism's own growth and life-history is secured. If the young chick imitated the old duck instead of the old hen, it would perish; it can only learn those new things which its present equipment will permit—not swimming. So the chick's own possible actions and adaptations in ontogeny have to be selected. We have seen how it may be done by a certain competition of functions with survival of the fit. But this is an application of natural selection. I do not see how Henslow, for example, can get the so-called "self-adaptations"—apart from "special

creation"—which justify an attack on natural selection. Even plants must grow in determinate or "select" directions in order to live.

4. So we may say, finally, that Organic Selection, while itself probably a congenital variation (or original endowment) works to secure new qualifications for the creature's survival; and its very working proceeds by securing a new application of the principle of natural selection to the possible modifications which the organism is capable of undergoing. Romanes says: "it is impossible that heredity can have provided in advance for innovations upon or alterations in its own machinery during the lifetime of a particular individual." To this we are obliged to reply in summing up—as I have done before (ref. 2, p. 220)—we reach "just the state of things which Romanes declares impossible—heredity providing for the modification of its own machinery. Heredity not only leaves the future free for modifications, it also provides a method of life in the operation of which modifications are bound to come."

VI.

The Matter of Terminology.—I anticipate criticism from the fact that several new terms have been used in this paper. Indeed one or two of these terms have already been criticised. I think, however, that novelty in terms is better than ambiguity in meanings. And in each case the new term is intended to mark off a real meaning which no current term seems to express. Taking these terms in turn and attempting to define them, as I have used them, it will be seen whether in each case the special term is justified; if not, I shall be only two glad to abandon it.

Organic Selection.—The process of ontogenetic adaptation considered as keeping single organisms alive and so securing determinate lines of variation in subsequent generations. Organic Selection is, therefore, a general principle of development which is a direct substitute for the Lamarkian factor in most, if not in all instances. If it is really a new factor, then it deserves a new name, however contracted its sphere of application may finally turn out to be. The use of the word

"Organic" in the phrase was suggested from the fact that the organism itself coöperates in the formation of the adaptations which are effected, and also from the fact that, in the results, the organism is itself selected; since those organisms which do not secure the adaptations fall by the principle of natural selection. And the word "Selection" used in the phrase is appropriate for just the same two reasons.

Social Heredity.—The acquisition of functions from the social environment, also considered as a method of determining phylogenetic variations. It is a form of Organic Selection but it deserves a special name because of its special way of operation. It is really heredity, since it influences the direction of phylogenetic variation by keeping socially adaptive creatures alive while others which do not adapt themselves in this way are cut off. It is also heredity since it is a continuous influence from generation to generation. Animals may be kept alive let us say in a given environment by social cooperation only; these transmit this social type of variation to posterity; *thus social adaptation sets the direction of physical phylogeny and physical heredity is determined in part by this factor.* Furthermore the process is all the while, from generation to generation, aided by the continuous chain of extra-organic or purely social transmissions. Here are adequate reasons for marking off this influence with a name.

The other terms I do not care so much about. "Physicogenetic," "neuro-genetic," "psycho-genetic," and their correlatives in "genic," seem to me to be convenient terms to mark distinctions which would involve long sentences without them, besides being self-explanatory. The phrase "circular reaction" has now been welcomed as appropriate by psychologists. "Accommodation" is also current among psychologists as meaning single functional adaptations, especially on the part of consciousness; the biological word "adaptation" refers more, perhaps, to racial or general functions. As between them, however, it does not much matter.[9]

[9] I have already noted in print (ref. 4 and 6) that Prof. Lloyd Morgan and Prof. H. F. Osborn have reached conclusions similar to my main one on Organic Selection. I do not know whether they approve of this name for the "factor;" but as I suggested it in the first edition of my book (April, 1895) and used it earlier, I venture to hope that it may be approved by the biologisst.

THE PATH OF THE WATER CURRENT IN CUCUMBER PLANTS.

By Erwin F. Smith.

(*Continued from page 457*).

3. Downward Movement of One Per Cent Eosine Water in Cut Stems Not Severed From Their Roots.

(No. 17). This was a young vine, 120 centimeters long, full of blossoms and young fruits and very thrifty; it bore about 24 leaves, the largest five averaging 20 cm. in breadth. March 23, 3:20 P. M. The terminal 12 cm. of the stem was cut away under water and the stump bent over and plunged into 1 per cent eosine water. The sun shone hot and the air of the house was rather dry. 4:20 p. m. No trace of stain in the veins of any of the leaves. March 25, noon. It is now over 44 hours since the cut stem was plunged into the eosine water and judging from the quantity remaining in the bottle no measurable volume has gone down the stem. The external appearance, proceeding from above downwards, is as follows: The first internode (the one in the eosine and just above it) is badly shriveled and diffusely stained. The first leaf (9.5 cm. from the cut end) is not quite as turgid as the rest, and its veins show a faint stain. The second internode (10 cm.) is pinkish green and in the grooves of the stem pink, especially toward the upper end, seeming to indicate that most of the stain has passed through the inner ring of bundles. The veins of the second leaf are also distinctly but faintly pink. The petiole of this leaf is 9 cm. long and its blade 12 cm. broad, and the same pale stain is to be seen in all of the veins. Further down there is no external evidence of stain. The downward movement of the stain has, therefore, been very slight. 1:30 p. m. A long tendril from the second node shows a faint internal stain outward for a distance of 10 cm. On cutting, this is seen to be due to stain lodged in the bundles, while at its base there is also a little diffuse stain. The stain now shows

through the interior of the third node which is 9 cm. long. 1:35 p. m. The stem was now cut for examination. The surface of the eosine water in the bottle has not lowered perceptibly. The diffuse stain in the first internode includes everything; the tissues are shriveled and seem to be dead. In the petiole of the first leaf there is a faint stain of the xylem part of each bundle; no diffuse stain into the phloem or any of the tissues outside of the bundle. At the base of the second internode (9 cm farther from the cut stem) the entire xylem of each bundle shows a pale red stain and this has diffused out from three bundles into the surrounding tissues. The second petiole, cut in the middle, shows a faint pink stain, best seen under the lens. It is sharply restricted to the bundles, but occurs in each one and includes the whole of the xylem. At the base of the third internode (9 cm. farther away from the fluid) the stain is fainter and is restricted to the xylem. It is in all of the bundles and is sharper (?) in the spirals of some. Apex of third petiole (down) shows faintest trace of color in 3 bundles, only to be seen under the lens. Color more distinct in the middle part but very faint. Base of fourth internode (9 cm. further from the eosine) there is a very faint stain sharply restricted to the xylem of 6 bundles, all of which is stained. Middle of next lower petiole shows barest trace of stain in two bundles, not visible without a lens. Stain visible in ten bundles of a small fruit from the same node. The base of the next internede (10 cm. further down) shows not a trace of stain. Five cm. farther up, no stain. Additional 3 cm. up, i. e., close under the node, there is a faint stain in the xylem of three bundles and this is not restricted to the spirals. One-half centimeter closer to the node the color is faint and is still restricted to the three bundles.

The stain seems to have travelled in all of the lignified walls, and it appears clear that the spirals did not carry it more than the other woody parts of the bundle. The movement of the eosine water down these stems, contrary to the water current, was scarcely more abundant than the upward movement past the gelatine plugs. Judging from this, the very slow downward movement of the stain apparently follows another

law than that governing the rapid upward movement of the transpiration water, i. e., that of surface tension or capillarity.

(No. 19). This was a large old vine, nearly destitute of leaves, the only large one being 8 centimeters below the cut stem. March 23, 4:06 p. m. The tip of this stem was cut under water and immediately transferred to 1 per cent eosine water. 4:15 p. m. No stain in the veins of the first leaf, 8 cm. from the cut. March 25, 12:45 p. m. The leaf, 8 cm. from the cut end, is flabby and its veins show a very decided stain. Farther down there is no stain visible externally. The stem was now removed from the fluid and cut open for examination. At 5 cm. down there was a diffuse stain involving the whole stem, but it was not dense and the bundles were not deeper stained than farther down the stem. At 10 cm. the sieve tube tissue was stained as well as the xylem and there was also a slight diffuse stain into the parenchyma, but the general tone of the stem remained green. At 20 cm. from the cut tip one of the 9 bundles (outer ring) showed no stain. No stain outside of the bundles. At 40 cm. from the cut all of the bundles showed the stain but in one (outer ring) it was much fainter than in the rest. The color was a decided pale red, including the whole of the xylem but not extending to any other part of the stem. At 80 cm. down, the stain was restricted to 4 bundles (the whole of the xylem part) and was barely discernable. At 85 cm. there was still a trace in these bundles—stain in the whole of the xylem and not brighter in the spirals. At 90 centimeters, and farther down, the stain was wholly absent.

This also proved a very instructive stem. The fact that at remote distances the stain was not restricted to the spiral vessels of the stem but tinged the whole xylem equally (the lignified walls) is very striking and decidedly different from the results obtained by passing the stain up the stem, in which case the spirals are stained ahead of the pitted vessels and are clearly seen to be the carriers of the eosine. In this case that portion of the stem in the fluid was not shriveled, probably because it was old and woody.

4. Movement of Water Through Boiled Stems Not Severed From the Plant.

(No. 11). A fine thrifty vine, 180 centimeters long, bearing 18 large leaves and half as many more small ones. The largest leaves have a spread of 17 to 19 centimeters. March 21, 4:00 p. m. About 35 cm. from the earth, the bright green stem was bent over and immersed for a distance of 20 cm. in hot water. An attempt was made to boil this water but the heat under the basin was not sufficient, although ample to kill the stem. 4:30 p. m. The temperature of the water during the last half hour has risen from 71° C. to 75° C. There is no change in the color of the immersed part of the stem, nor any change in the foliage above, but the effect of the hot water is already noticeable in the very decided shrinkage of the immersed stem. It has shrunk in diameter nearly one-half. 4:50 p. m. During the last 20 minutes the temperature of the water has risen only one degree. This was now poured out and water at 89° C. substituted. In pouring, the temperature fell to 85° C. In this hotter water the stem quickly became paler green. 4:58 p. m. Temp. of water 80° C. The immersed part of the stem has now shrunk to one-third of its normal diameter, and this shrinkage has extended both up and down, for a short distance out of the water (a few centimeters). 5:15 p. m. Temp. now down to 76° C. Stem taken out. Except the apex of one leaf, 15 cm. up, the foliage did not become flabby. Below the boiled part is a small branch with half a dozen leaves, sufficient to carry the roots. March 22, 11 a. m. The boiled part of the stem, which is now dry and greenish-brown, was wrapped in many folds of rubber cloth. The foliage of this vine shows no wilt, except parts of 5 small leaves, which were near the boiled part and may have been injured by the heat of the lamp. It is windy and sunny and the air of the house is rather dry so that transpiration is active. Temperature in shade, 1 foot above the bench, 26° C. Noon. A check vine (cut off at base, yesterday p. m.) has wilted and shriveled. Temperature three feet above the bench, among the leaves, 30° C. 1:20 p. m. No change. What is

especially surprising is that the tender terminal leaves show no signs of wilt. 4:15 p. m. This vine has stood up remarkably to-day. The transpiration demands have been large and there has been no wilt—not a trace—that mentioned as occurring on a few of the small basal leaves being evidently due to imperfect protection from the heat of the lamp when the stem was boiled. March 23, 11 a. m. Sunny and hot; some wind; air of the house rather dry, and transpiration large. No wilt of the foliage except the margins and tips of the blades of three big leaves midway up the stem. These are slowly drying out. 12:30 p. m. The greater part of the foliage on this vine is still turgid and normal in appearance. The tips and margins of the three leaves above mentioned are crisp, but this injury involves only a small part of each leaf. Transpiration active. Temp. in sun 30° C. Dry bulb 26.5° C.; wet bulb 22° C. 3:00 p. m. Slight, if any, change. Nearly all of the leaves are turgid and entirely normal in appearance, including all at the top of the vine. 4:20 p. m. No change since the last record. The vine stands up well. Temp. now 24° C. Active transpiration all day. March 25, 1:15 p. m. The vine stands up well. Nearly all of it is perfectly healthy, including the tender upper part, but portions of the lower leaves already mentioned are slowly drying out and in a very interesting manner, i. e., after the fashion of the California vine disease, the larger veins and their branches and a little of the adjacent parenchyma remaining green, even dark green, while the parenchymatic areas between the veins, especially at the apex of the blades and on the margins, are becoming first yellow and then a dead brown. 5:30 p. m. Vine stands up beautifully. It is four days since the stem was killed by the hot water. March 26, 2:45 p. m. A great change for the worse since yesterday. All of the foliage has now wilted (as yet only the blades) and the large leaves midway down as well as the smaller lower ones are rapidly drying out. March 27, 1:20 p. m. All of the leaves are now crisp, except a few very small flabby ones which are in the vicinity of a half grown fruit from which they are drawing water. The stem is still turgid but some of the petioles begin to droop.

The leaves below the boiled part are still healthy. March 28, 1:30 p. m. The stem and the petioles are still green but the latter are becoming more and more flabby, most of them at the top of the vine having lost all of their turgor.

This vine was able to draw all the transpiration water necessary to supply a large leaf surface (more than 3.000 sq. cm.) through about 25 centimeters of dead stem for a period of four days, during a part of which time the transpiration was very active. All of this water must have passed up through the bundles, since all the outer parts were dead and dry and shriveled down onto the bundles, the vessels of which preserved their shape unaltered as shown by subsequent examination.

(No. 13). This vine was 130 centimeters long. It bore six small leaves and 12 large ones, the best averaging 17 cm. in breadth. March 22, 1:02 p. m. The stem was bent over near the earth and inserted for a distance of 18 centimeters into water at 90° C. In two minutes the temperature rose to 95° C. 1:07 p. m. Water simmering; temp. 97° C. Boiled part not yet noticeably smaller. 1:10 p. m. Stem shows shrinkage and change of color. 1:15 p. m. Slight loss of turgidity in most of the leaves. 1:20 p. m. A marked shrinkage of the diameter of the stem is now first visible. The flabbiness of the foliage is increasing rapidly, every leaf is affected. 1:27 p. m. Water has remained at 97° since last record. Stem taken out because of the marked wilt of the foliage. This wilt appears to be due to the transpiration of hot water. The wilt is too sudden and decided to be due to anything else. The stem has not only shriveled in the water but also for a distance of 10 cm. up and 5 cm. below, making a total of 33 cm. of dead stem. Sun hot; earth and air of house rather dry; transpiration active. Such an experiment were better tried when the air is nearly saturated and transpiration slight. 1:45 p. m. Stem wrapped in many folds of rubber cloth. Roughly estimated it has shrunk to about one-third its normal diameter. The leaves seem to be recovering their turgor.

2:00 p. m. The lowest leaves are still flaccid but the upper ones have fully regained their turgor. 3:45 p. m. The lower leaves have now also regained their turgor. Its loss was

clearly due to the transpiration of hot water. (Subsequent experiments showed that it is very easy to push this wilting beyond the power of the plant to recover). 4:15 p. m. The plant stands up well. There is no trace of wilt. March 23, 11 a. m. No sign of wilt. Noon. The lowest five leaves show distinct signs of wilt at the tip of the blade. None of the upper leaves show any trace of it. 12:25 p. m. The wilting is worse but is still confined to the lower leaves. It is very decided on the lowest one which is exposed to the bright sun. The tender apical leaves are turgid, as well as those in the mid part of the stem. 1:20 p. m. The leaf next to the lowest one begins to crisp. 3:00 p. m. Blade of lowest leaf but one is now crisp, and the blades of the other four are drying out at the apex and on the margins and between the larger veins. 4:30 p. m. No change. The bulk of the foliage stands up well, including all of the upper leaves March 25, 1:20 p. m. The lower leaves of this plant are dried out to a greater extent than are those of No. 11, but the major part of the foliage is normal and the tips of both vines are noticeably turgid. The drying out of the parenchyma between the veins is also to be seen in the affected leaves of this vine, the larger veins and a narrow border of the leaf parenchyma remaining a bright green. 5:40 p. m. The vine stands up well. It is three days and four hours since the stem was boiled. March 26, 3:00 p. m. The vine begins to show symptoms of collapsing. All of the petioles are turgid, but the blade of the lowest leaf is nearly dry, that of the next up is wholly dry; those of the next three above are crisp at the apex and on the margins (one-fifth to one-third the surface); the three next up show a trace of drying on their margins, and in all the rest there is a faint suggestion of loss of turgor. March 27, 2:00 p. m. All of the leaves on this vine are now crisp-dry except three at the top which are flabby. The stem and the petioles are still turgid. March 28, 1:30 p. m. The upper three leaves are still flabby, and all of the petioles are still rigid except the tips of some of the lower ones which begin to droop.

This vine gives results confirmatory of the preceding. For more than three days the plant was able to draw all of the

water necessary for its use through 33 cm. of dead stem. Probably if air could be prevented from gradually passing through the shriveled stem into these water carrying vessels and interfering with the normal condition of things the plant might continue to draw its water through a dead stem almost indefinitely.[1]

5. THE RESULT OF PARASITIC PLUGGING OF THE VESSELS.

From these experiments and those upon the cucumber wilt, which I have published elsewhere, it follows that the downward path of *Bacillus tracheiphilus* from the inoculated leaf blade into the stem of the cucumber (for an account of this disease see *Centr. f. Bakt. u. Par. Allg.* I, No. 9–10, 1895) is exactly that made use of by the ascending water current, just as I stated it to be at the Brooklyn meeting of the A. A. A. S., and the general wilt of the foliage may be explained, first, by a functional disturbance, due to the more or less complete clogging of the lumina of the spiral vessels with countless millions of these bacteria which thrive in the alkaline fluid of the vessels, and, second, by a structural disturbance, due to the breaking down (dissolving) of the walls of these spirals and the flooding out and subsequent growth of the bacteria in the surrounding parenchyma and in the pitted vessels, accompanied, of course, by the more or less free entrance of air into the spirals. It is probable, although not enough examinations have yet been made to render this certain, that no leaf wilts from secondary infection until the water carrying spirals in its petiole have become clogged by the bacillus, i. e., that the wilt of the leaf is not induced by the partial clogging of the vessels farther down in the stem. This is the more likely, first, from the fact that there is always a progressive wilt, leaf after leaf, beginning with the ones nearest the point of infection and moving both ways therefrom, and, second, from the fact that very rarely are all of the pitted vessels filled, so that water lifted up from the roots has always the opportunity to

[1] Those who wish to follow these subjects may consult the above mentioned work by Strasburger, pp. 510–936, where many interesting experiments are detailed.

pass around the clog in the spirals by way of the unfilled pitted vessels and to enter the spirals once more farther up. Were this not so, i. e., were pitted vessels filled as readily, as quickly, and as fully as the spirals, we should have not the gradual wilt of leaf after leaf up and down the stem, but the sudden collapse of all the leaves beyond the original point of attack. This is exactly what does happen in watermelon vines attacked by *Fusarium niveum*, (for a brief account of this parasite see *Proc. Am. Asso. Adv. Sci.*, Vol. 43, 1894, p. 289, and *Ibid*, Vol. 44, 1895, p.) where the pitted vessels appear to fill with the fungus as soon, if not sooner, than the spirals.

These two diseases of cucurbits are very interesting from a physiological standpoint, and both parasites lend themselves readily to infection experiments, their slightly different behavior being, perhaps, accounted for by the fact that the fungus is strictly ærobic, while the bacillus is facultative anærobic. Whatever be thought of butter or gelatine, it certainly cannot be maintained that the mere presence of these parasites in the lumina of the vessels destroys the carrying capacity of the uninjured walls, and yet they act quite as effectually as gelatine, paraffin, or cocoa butter plugs, causing, when they fill the vessels only incompletely, a flabbiness of the foliage, which is proportionate to the extent of the plugging and to the activity of the transpiration, and which may give place to complete turgor in periods when the transpiration is small (night, early morning, or damp days), and producing, when they completely fill the lumina of the vessels, an entire collapse of the foliage, from which there is no recovery. In case of the cucumber this collapse takes place as soon as the spiral vessels leading into any petiole are filled by the bacillus.

EDITOR'S TABLE.

—PROFESSORS in the scientific departments of our schools should exercise their influence to prevent the spoliation of nature that is going on at so rapid a rate in our country. We do not especially refer at present to forest fires which involve so much financial loss that our state and general governments are moving in the direction of their prevention. In passing, however, we must refer to the railroad companies as delinquents in this matter, and insist that heavy fines be imposed on them in all cases where fires can be shown to have originated from locomotives. We counted from the car windows of a train not long since, twelve distinct fires burning near the track in the space of a few miles, in a forest covered region not far from Philadelphia, and no one appeared to pay any attention to them.

We wish, however, to refer to the destruction wrought near our cities by the uprooting of plants and the breaking off of branches for purposes of decoration of public and private houses. Within reasonable bounds the vegetable world furnishes material for such decoration, but the practice is carried beyond the rich resources of nature to meet. Our woods are being rapidly stripped of ornamental plants for miles all round our large cities. In many regions the *Epigœa repens* is completely destroyed, and the blooms of the dogwood and kalmia no longer appear. Lycopodia are uprooted over large tracts, and must now be brought from considerable distances. Some of the ruin is wrought for church decoration, and the girl-graduate is responsible for more of it. Teachers of the natural sciences can teach their hearers that this cannot go on forever. Especially can they point out that botanical classes should not gather arm-loads of orchids of fastidious habits if they do not wish to see the localities destroyed or the species well nigh exterminated.

The authorities in charge of our public parks might, in some places, profitably change their point of view. A park should not consist principally of graded paths lined with stone curbs or walls, separated by tracts of close shorn grass. Shrubberies of nature's planting should remain, and the vines with which nature festoons the forest should not be cut down. No harm is done if there are places where rabbits may hide, and wild birds may nest. Even an owl or two might be permitted to keep down so far as he or she can, the English Sparrow nuisance. In fact, a park is not necessarily a place from which nature is

excluded. The perpetual clearing of undergrowth means also the ultimate destruction of forest, as the natural succession is thus prevented.

As an offset to this public and private vandalism, we have near our cities a goodly number of citizens who preserve more or less of nature in their private parks. It will be to these to whom we must look to replenish our stock of native shrubs and herbs, if the vandal continues to have full swing elsewhere.

THE forty-fifth meeting of the American Association for the Advancement of Science to commence at Buffalo, N. Y., on August 22d, will be characterized by one feature which is deemed by the society an improvement over previous meetings. No excursions will be made during the working hours of the day during the session, only those occupying evening hours being acceptable. At the close of the meeting the field for such diversions will be clear. The geological excursions have been so arranged as not to conflict with the meetings; and the six scientific societies, which meet about the same time, it is hoped will contribute to the importance of the general gathering. It is anticipated that these arrangements will arrest the tendency to dissipation of energy which has been apparent during the last few years. If the habit of many of the embryologists to absent themselves could be overcome, the full force of the Association would be represented. It is expected that a number of evening lectures will present to the public the latest results of research in America.

RECENT LITERATURE.

Surface Colors :—The object of the little book on this subject[1] by Dr. Walter, of Hamburg is apparently to furnish zoologists, mineralogists, and chemists with an accurate explanation of certain color phenomena which are not as yet universally understood, and which are incompletely treated even in the best text-books on Physics. The keynote of the whole book is given in a single sentence of the introductory chapter. "The intensity of the light reflected from any body may be calculated by Fresnel's ordinary formulæ for colorless substances, in the case of those rays which are slightly or not at all absorbed by the

[1] Die Oberflächen-oder Schillerfarben, von Dr. B. Walter, pp. VIII + 122, Braunschweig, F. Vieweg und Sohn, 1895.

body in question; but for wave-lengths which are strongly absorbed by the given substance, Cauchy's formulæ for the intensity of metallic reflection should be used." It appears from these formulæ that the intensity of the reflected light depends on the index of refraction and on the coëfficient of absorption of the substance presenting the reflecting surface. Since both these factors are different for light of different colors, it is shown that white light must be reflected with some of its "components" relatively weaker than others, *i. e.*, no longer in the proper proportion to give the sensation of white light. The application to the colors seen in the mineral kingdom is illustrated by the example of magnesium cyanplatinite, $Mg\,Pt\,(CN)_4$, where,—as is true of most crystals,—the index of refraction and the coëfficient of absorption vary with the direction in which the light vibrates, as well as with the wave-length of the light. The extent to which true surface color is observable on minerals is not indicated, though the *possibility* of a very wide application is clearly shown.

In the appendices, certain mathematical aspects of the subject are treated in a manner suited to the requirements of physicists.—A. C. G.

The Whence and Whither of Man.[1]—This book comprises a series of lectures dilivered at Union Theological Seminary, with some additional matter. The author discusses the doctrine of Evolution from the standpoint of a theologian. He endeavors to show that the great law of animal and human development as revealed in the sequence of physical and mental development is that those species survive which are best conformed to their environment; that this law holds good in the development of the rational, the dominant faculty in man; and finally, to become higher man he must develop a moral-nature by attaining a knowledge of himself as a moral agent, and while not disregarding the body, he must subordinate its appetites to the higher motives furnished by right and duty. It is in following this line of thought that the author hopes for a definite answer as to the future destiny of man.

The closing chapter deals with the present aspects of the theory of evolution. He here compares the various hypotheses of evolution and considers their merits. He judiciously selects the good elements of all of them, concluding that "each theory contains important truth." He concludes that Nägeli's view of "initial tendencies" is too often undervalued. "My own conviction is steadily strengthening that without

[1] The Whence and Whither of Man. By John M. Tyler, New York, 1896, Charles Scribner's Sons, Publishers.

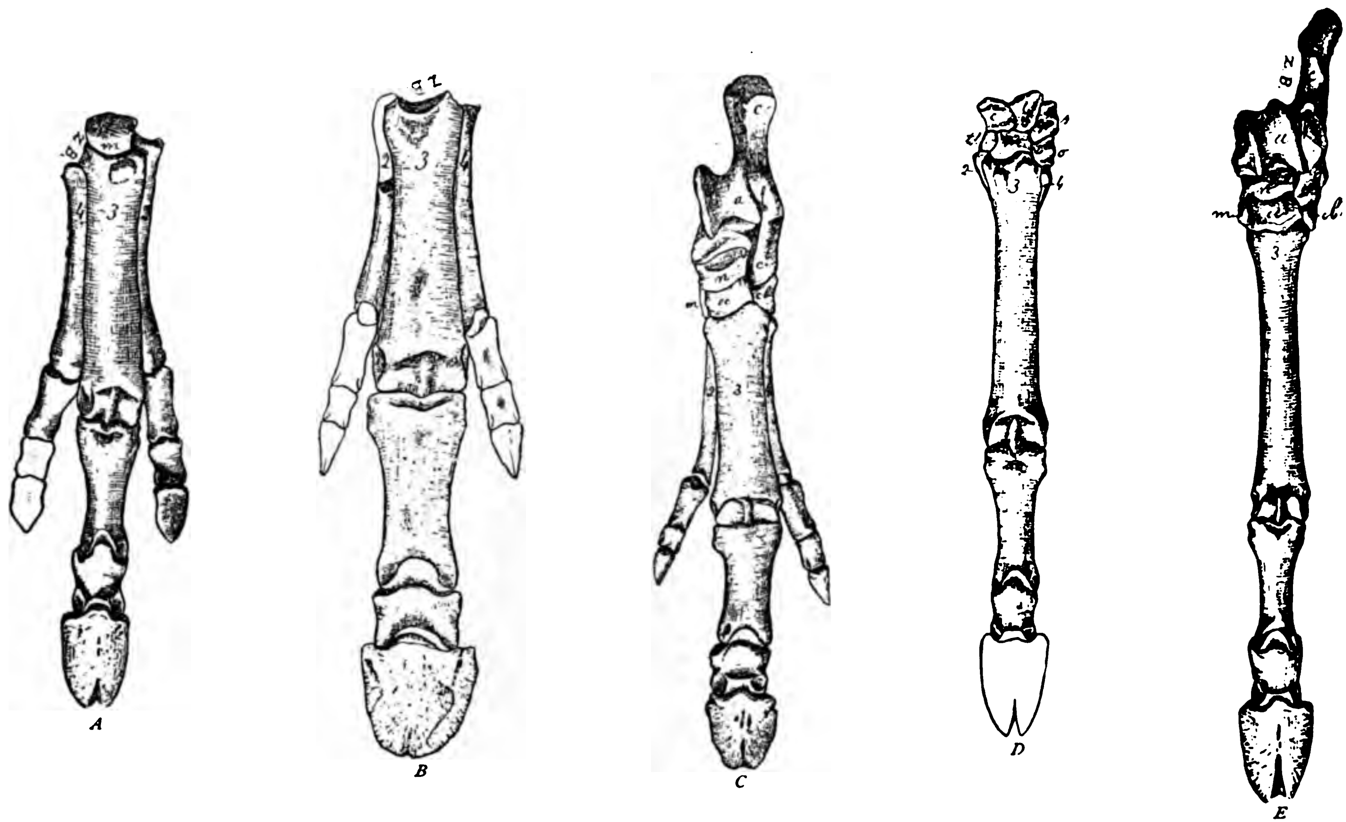
some such original tendency or aim, evolution would never have reached it present culmination in man." He quotes Boveri that "there is too much intelligence in nature for any purely mechanical theory to be possible." It is curious that these authors do not perceive that the sensation of protoplasm, (consciousness), furnishes the basis for the exhibition of the intelligence which they observe, and which has itself undergone evolution coincidentally with the organism. Both orthodox and heterodox evolutionists (theologically speaking) seem equally slow to adopt this view.

Prof. Tyler's book is eminently moderate and reasonable, and will introduce evolution to a large class of readers in an agreeable form.

Cope on the Factors of Organic Evolution.[3]—This book is divided into three parts: I, The nature of variation; II, The causes of variation; III, The inheritance of variation. In the first part it is endeavored to show that variation is not promiscuous or multifarious, but pursues direct courses towards definite ends. This is done by presenting the variations of existing species as to color and structure, and by an examination of the series presented by the forms of vertebrate life in past geologic ages. The latter presentation is a general phylogeny of the vertebrata, with special sections on that of the horse and that of man. The second part is divided into chapters which deal with the physical energies as causes of variation, and the effects of molar motion as seen in variation. These methods of evolution are termed respectively physiogenesis and kinetogenesis. Especial attention is given to kinetogenesis in connection with the phylogeny of vertebrates, since it is in these two fields that most of the original work of the author has been done. The author has demonstrated that the primary cause which has moulded the vertebrate skeleton is molar motion. In the third part, the inheritance of the characters so produced is shown to be the rule, thus demonstrating the inheritance of acquired characters. Theories of inheritance are discussed, and that one which asserts the transmission of energies to the germ plasma is defended. These energies are believed to be the results of a composition between inherited and acquired energies, the whole of them being referred to a class distinct from the inorganic energies, which he has named Bathmic. The last chapter in this part is devoted to a consideration of the relation of consciousness to movements, and hence as a cause

[3] The Primary Factors of Organic Evolution, by E. D. Cope, Professor of Zoology and Comparative Anatomy in the University of Pennsylvania. Chicago: Open Court Pub. Co., Feb., 1896, $2.00.

PLATE X.



Feet of Proterotheriidae from Ameghino. A, fore foot of *Proterotherium cavum* Amegh. B-C, Fore and hind feet of *Diadiaphorus majusculus* Amegh. D-E, Fore and hind feet of *Thoatherium crepidatum* Amegh.

of progressive evolution. The author holds that sensation is a cause of effects which would not appear in its absence, and that its presence conditions progressive evolution. The author holds this to be proven not only by the direct effect of consciousness as observed, but also on the other ground that there is no sufficiency in the inorganic and unconscious organic energies to effect progressive evolution. This is because the well-known tendency of the latter is to the integration of matter and the dissipation of energy, which leads always away from vital phenomena. The author believes the entire vegetable kingdom to be degenerate, its vitality being the expression of automatic energy which derived its self-sustaining character from ancestors endowed with sensation which occupied a position between animals and plants. The Mycetozoa he believes to be existing near relatives of these types.

The book is illustrated by 120 plates and cuts. One of these illustrative of homoplassy, we extract from the chapter on kinetogenesis, with the following explanatory remarks :

" Before reviewing the subject, I cite what is the most remarkable example of homoplassy in the Mammalia which has yet come to the knowledge of paleontologists. Ameghino has discovered in the cenozoic formations of Argentina a group of Ungulata which he calls the Litopterna, and which I regard as a suborder of the Taxeopoda, allied to the Condylarthra (p. 128). Ameghino placed the group under the Perissodactyla, but the tarsus and carpus are of a totally different character, and indicate an origin from the Condylarthra quite independent of that division. The carpal and tarsal bones are in linear series, or if they may overlap, it is in a direction the opposite of that which characterizes the order Diplarthra (=Perissodactyla and Artiodactyla). But the Litopterna present a most remarkable parallelism to the Perissodactyla in the characters of both the feet and the dentition. No genus is known as yet which possesses more than three toes before and behind, and these are of equal length (Macrauchenia Owen). In this genus the teeth are not primitive, but are much modified. The most primitive dentition is seen in the genus Proterotherium (Ameghino) where the superior molars are tritubercular, as in many Condylanthra. In this genus (Pl. X, fig. A) there are three toes, but the lateral ones are reduced, about as in the equine genus Anchitherium (p. 148). In the next genus, Diadiaphorus Amegh., the superior molars are quadritubercular and crested, while the lateral toes are reduced still more, being quite rudimental (figs. B C), as in the equine genera Hippotherium and Protohippus. The superior molars have not progressed so far as in these genera, but are not very different from those of

Anchitherium. In the third and last type (Thoatherium Amegh.) the lateral digits have disappeared from both fore and hind feet (figs. C D), so that the condition is that of the genus Equus (fig. 81), but the splints in the *Thoatherium crepidatum* Amegh. are even more reduced in the known species of horse. The superior molars have not assumed the pattern of the genus Equus, but resemble rather those of Macrauchenia, and could have been easily derived from those of Diadiaphorus.

Here we have a serial reduction of the lateral digits and their connections with the leg, and increase in the proportions of the middle digit and corresponding increases in the proximal connections, exactly similar to that which took place in the horse line, in a different order of Mammalia."

The publishers have done their work well, and are especially to be commended for having made the book of a convenient size to be carried in the pocket or satchel.

The Child and Childhood in Folk-Thought.—(The Child in Primitive Culture); by A. F. Chamberlain; New York, Macmillan & Co., and London, 1896. Pp. x and 464; with bibliography and three indexes; price $3.

Dr. Chamberlain's work is not, as its chief title might lead one to suppose, a mere collection of folk-lore about the child. It is rather an attempt by this means to study the position of the child in primitive society. The author has brought together a great mass of material from every hand, and arranged it systematically under appropriate headings; as a result we find every phase and aspect of childhood represented in his book.

The opening chapters, on the Lore of Motherhood and Fatherhood, have in some places only a remote bearing upon the main topic, but they may be regarded in the light of a general introduction. Following these are a number of chapters which aim to show the attitude of society toward the child; folk-lore on the soul of the child, legends connecting children with animals or plants, stock answers of the adult to the child's questions, superstitions concerning children, etc., together with stories of education and training among uncultured races. A large part of the work deals with the influence of the child upon society—the effect of child-language in modifying adult language; the child's position in many tribes as oracle, judge, physician, or priest, etc. The final chapters are a selection of popular proverbs and sayings bearing upon childhood, from the literature of various races, cultured as well as

uncultured. The bibliography at the end is thorough, if not exhaustive; it consists of over 550 titles, covering the entire field.

The author claims no originality of investigation; but he has culled his material from a host of authorities, and his selections are well made. He has no conclusions to draw; he simply presents the material as data, with a view to a complete survey of the subject. The chief criticism that can be made upon his method is that it frequently leads to a curious intermingling of fables and traditions with actual race customs. Thus in the chapter on the Children's Food is described (p. 150) the practice which holds among several tribes of placing food on the grave of a dead child, to refresh its soul on the way to the spirit-land, and almost immediately after follows the legend of how the infant Hercules obtained immortality. The book is exceedingly interesting; it treats its subject as thoroughly as the breadth of the task together with the limits of the volume permit; and it is wonderfully conducive to further reading.—H. C. WARREN.

Stockham on the Ethics of Marriage.[4]—This book is written with the view of securing an excellent object, the increase of the happiness of marriage. As the authoress is an M. D., and as she treats the subject at the outset with a seeming respect for scientific truth, we anticipated something valuable from her point of view. But we are compelled to say that the grains of truth are overlaid with such a quantity of error, rhapsody and sheer silliness, that we can only recommend the book as a study in feminine psychology. That there is one element of common sense running through it we are glad to admit. The authoress sees nothing degrading or indecent in the sexual relation. For this we must praise her; but it was surely not necessary for her to apologize for her good sense, by pages on pages of religious rhapsody. The gist of her method of promoting marital happiness is that sexual intimacy may take place without completing the act. This proposition is as old as the rational faculty of man; but, as rationality is usually less directed to sexual subjects than to any other, it is quite possible that her advice on this point may do some good. There are some amusing passages. Fearing to appear to fall into the Charybdis of "hedonism" she runs high and dry on Scylla, as follows: "Before and during the time some devotional exercises may be participated in, or there may be a formation of consecration of an uplifting character in which both unite!"

[4] Karezza; Ethics of Marriage, by Alice B. Stockham, M. D., Chicago. A. B. Stockham & Co.

The authoress labors under several physiological errors, which should be pointed out. She thinks in common with the ignorant classes generally, that the orgasm is concerned in impregnation, which is well known not to be the case. She also asserts that the secretion of the testis is produced at the time it is needed for use, an idea promulgated several years ago in a silly book called Diana. This is also untrue; its elaboration requires some days, and when the gland is full the secretion makes its presence known and demands expulsion. The present book should have stated also, that the practice she recommends, which she calls "Karezza," is a most potent stimulant of the secretion in question, and does in some men produce enlargement of the prostate gland and orchitis, so that every man must be in this matter his own doctor. But one will not find logic in this book. In view of what precedes one wonders where the authoress got her degree of M. D., and who is responsible for her education. We must, however, once more commend the spirit of the book, and hope that she will be instrumental in teaching some men and women ordinary temperance. But it must be borne in mind that medical writers chiefly deal with pathological conditions, and that the persons she writes about are mostly abnormal through excess or deficiency.

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General Notes.

MINERALOGY.[1]

Contact Goniometer with two Graduated Circles.—In pursuance of the idea already applied to the reflection goniometer (ref. in this journal, 1895, p. 266) Goldschmidt[2] has designed a contact goniometer with two graduated circles. The horizontal circle carries the support for the crystal, which can thus be rotated about a vertical axis. The vertical circle is a metallic band carrying a moveable block. Through the block a small metal rod passes radially toward the center, and on the inner end of the rod a small plate is fixed. By movement of the crystal about its vertical axis and of the block on its arc, the plate may be brought to parallelism with any face on the upper side of the crystal, Actual contact of the plate with the crystal face is effected by sliding the rod through its block. Readings on the two circles give data for computing the position of a plane, exactly as in the case of the reflection goniometer to which reference was above made.

Crystallographic Properties of the Sulphonic Acid Derivatives of Camphor.—About 17 of these compounds are mentioned by Kipping and Pope[3] with much detailed information concerning the crystallograpy of several of them. As might be expected from the fact that the solutions of many of these substances exhibit the phenomenon of circular polarization, the crystals furnish examples of a number of the less common low symmetry grades. Among these are hemimorphism in the monoclinic system (sphenoidal class of Groth), sphenoidal hemihedrism in the orthorhombic system (bisphenoidal class), and probably hemihedrism in the triclinic system (pedial class). Such crystallographic studies must be of great value to stereo-chemistry.

Optical Properties of Lithiophilite and Triphilite.—On these two minerals Penfield and Pratt[4] have based an interesting investigation of the change of optical properties due to the mutual replacement of manganese and iron in isomorphous mixture. It is found

[1] Edited by A. C. Gill, Cornell University, Ithaca, N. Y.

[2] Zeitschr. f. Kryst., XXV, p. 321, 1895.

[3] Zeitschr. f. Kryst., XXV, pp. 225–256, 1895.

[4] Am. Jour. Sci., L, pp. 387–390, Nov., 1895.

that with increasing percentage of iron the index of refraction increases, while the plane of the optical axes is changed from the base (001) to the macropinacoid (100). A specimen containing 26.58% FeO shows an optical angle of 21° 53′ in the basal plane for thallium light, is *uniaxial* for sodium light, and has an angle of 15° 3′ in the macropinacoid for lithium—a remarkably good example of orthorhombic dispersion. With 35.05% FeO the crystals are found to be negative, whereas those with less iron are optically positive. It is suggested that in the pure manganese molecule, the change may be found so great that the brachypinacoid is the plane of the optical axes.

Native Sulphur in Michigan.—Scherzer[5] reports an occurrence of sulphur a mile west of Scofield, Monroe Co., Michigan. It is found in a stratum of impure cavernous limestone about one to three feet in thickness. The pockets, varying from a fraction of an inch up to three feet in diameter, are often lined with calcite and celestite crystals with bright lustrous masses of sulphur toward the center. The removal of about an acre of this bed has yielded 100 barrels of pure sulphur. The sulphur seems to have originated from hydrogen sulphide which is abundant in the waters of the neighborhood. The hydrogen sulphide, in turn, may be a product of decomposing organic matter.

Leadhillite Pseudomorphs at Granby, Mo.—The occurrence of leadhillite at Granby in the form of pseudomorphs after calcite and galena is made the subject of a note by Foote.[6] Scalenohedrons in a chert calamine rock are composed usually of pure cerussite; more rarely the substance is found to be leadhillite. Galena cubes replaced by leadhillite were also observed. In these cases the secondary mineral is usually mixed with remnants of the original galena, producing a "gray amorphous mass." In a few specimens the leadhillite is pure.

Celestite from Giershagen.—According to Arzruni and Thaddéef[7] the axial ratio of "normal" celestite is $a : b : c = .78093 : 1 : 1.28324$. The mineral from Giershagen, which appears to be chemically pure $Sr\,SO_4$, has the ratio $a : b : c = .77962 : 1 : 1.28533$. The mean of four determinations places the specific gravity at 3.9665. The optical angle of "normal" celestite is given as $2\ V_{a\,Na} = 50°\ 34'$. This investigation adds another to the list of chemically pure compounds whose

[5] Am. Jour. Sci., L, pp. 246–248, Sept., 1895.

[6] Am. Jour. Sci., L, p. 99, August, 1895.

[7] Zeitschr. f. Kryst., XXV, pp. 38–72, 1895.

molecular volume may be considered as accurately known, and allows of comparison between the various physical constants of this and isomorphous substances.

Minerals from the Galena Limestone.—Hobbs[8] gives a detailed description, with many drawings, of the crystallized minerals from the galena limestone of southern Wisconsin and northern Illinois. The habitus of the various crystals is made prominent in the discussion of them. New forms are reported on calcite $(24.0.\overline{24}.1)$, on cerussite (0.25.4), and on azurite (307), $(\overline{2}03)$, $(\overline{2}05)$ and (9.12.8).

Miscellaneous Notes.—Becke[9] shows that the center of symmetry may be used as a fundamental conception in developing the 32 classes of crystal symmetry, notwithstanding the fact of its abandonment by Groth and Fedorow.—Sylvite from Stassfurt, investigated by Schimpff[10] with special reference to the impurities of the same, gave K Cl 99.239, Na Cl .242, $Mg\ Cl_2$ .089, $Ca\ SO_4$ .073, H_2S .0023, residue .108, loss on melting .2847. The foreign substances seem to occur chiefly as inclusions with the mother liquor. These figures doubtless give a very good idea of the amount of impurity present, but the extreme right hand digits must be looked upon as mathematics rather than chemistry.—Igelström[11] finds molybdenum, probably present as Mo_2O_3, in the hematite from the "Sjögrube," Gouv. Örebro, Sweden. One specimen of the same material showed spectroscopically the presence of thallium.—Niven[12] notes the discovery on New York Island of numerous interesting specimens of the rare earth minerals xenotine and monazite. Titanite, epidote, beryl and menaccanite are also mentioned.—The mineral named schneebergite by Brezina[13] on the basis of an apparently faulty qualitative investigation is shown by Eakle and Muthmann[14] to be in reality a very pure lime-iron garnet, or topazolite, instead of a calcium antimonite. The specific gravity is 3.838, and the chemical composition:

[8] Zeitschr. f. Kryst., XXV, pp. 257-275, 1895.

[9] Zeitschr. f. Kryst., XXV, pp. 73-78, 1895.

[10] Zeitschr. f. Kryst., XXV, p. 92, 1895.

[11] Zeitschr. f. Kryst., XXV, p. 94, 1895.

[12] Am. Jour. Sci., L, p. 75, July, 1895.

[13] Vehr. d. k. k. geol. Reichsanstalt, 1880, p. 313.

[14] Zeitschr. f. Kryst., XXV, pp. 244-246, 1895.

	found		calculated for $Ca_3 Fe_2 Si_3O_{12}$
SiO_2		35.45	35.43
Fe_2O_3	32.33	32.11	31.50
CaO	32.58		33.07

—Foote[15] gives some details concerning a new mineral which he proposes to name Northupite. It was found by Mr. Northup in the "tailings" from a boring made at Borax Lake, Cal. The crystals are regular octahedrons reaching rarely 1 cm. in diameter. The substance seems to be a double chloride and carbonate of sodium and magnesium. Cleavage imperfect, H = 3.5 to 4.

PETROGRAPHY.[1]

Volcanic Rocks and Tuffs in Prussia.—In the hills east of Ebsdorf, near Marburg, Prussia, are large areas covered by basalt flows, flows of dolerite, and others of rocks intermediate in character between these two, both of which are pre-Tertiary in age, or at any rate are older than the Tertiary beds with which they are associated. The volcanic rocks are cut by dykes of very basic rock resembling limburgite. The little hill west of Wittelsberg, near the northern edge of the basalt area, and the flank of the hill near Kehrenberg, are composed largely of basalt tuff.

The basalt consists of phenocrysts of augite and olivine in a dense felt of augite microlites, biotite and magnetite, in the spaces between which is a colorless glass containing xenomorphic feldspar, leucite and nepheline. Inclusions in the basalt are very common. They comprise besides fragments of foreign rocks, concretions of olivine and of augite. The olivine concretions always contain more or less bronzite, and usually they are surrounded by a violet-brown rim similar to the rims found surrounding the augite phenocrysts in the basalt. Even those concretions that are composed almost exclusively of bronzite are surrounded by rims of this character. The principal component of this rim is a monoclinic augite, so that it appears here that the bronzite, which must have been one of the earliest separations from the magma, was, after its crystallization, changed into augite. Other concretions show the

[15] Am Jour. Sci., L, pp. 480–488, Dec., 1895.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

alteration of the bronzite into olivine. By complete fusion one concretion, which is thought by the author to have been a bronzite-augite aggregate, has been changed to a mass of rounded augite and olivine grains imbedded in a glass which locally is replaced by nepheline. The alteration of the bronzite, as indicated by the study of a number of sections, is into olivine, augite, magnetite and glass. Among the rare constituents of the olivine concretions are chrome diopside and picotite. The augite concretions or inclusions, consist almost exclusively of a monoclinic augite with which is usually associated a little olivine. In the interiors of the concretions the augite contains fluid enclosures, but toward their peripheries the enclosures are all of glass. Often between the augite grains are little nests of calcite. One of the inclusions observed by the author is abnormal in that it is composed of a small nucleus of augite surrounded by a zone of brown biotite.

Of the foreign inclusions, the author describes two kinds—the calcareous and the granitic. The basalt in the neighborhood of limestone inclusions loses its biotite and magnetite. Nearer the inclusions the augite microlites become light colored and magnetite grains are again developed. At the boundary of the limestone fragment is a rim of large augites, whose ends are directed toward the center of the inclusion. This latter itself is composed of the remnants of calcite grains imbedded in a brown glass, in which are also well formed crystals of a scapolite. The sandstone inclusions have been changed to a mass of quartz grains lying in a brown glass, the whole being surrounded by the usual zone of augite microlites. The granite inclusions first lose their mica. The old feldspar has given rise to newly developed feldspar.

The dolerite seem to occur as a number of small flows that have run together. It presents no special peculiarities. The dyke basalt cutting the tuffs and dolerites sometimes contains well defined crystals of olivine, which occasionally occur as interpenetration twins.

Igneous Rocks of British Columbia.—The petrographical characters of the principal rocks occurring within the area of the Kamloops Map-sheet of British Columbia are described by Ferrier.[1] These rocks embrace feldspathic actinolite schists, diabase porphyrites, harzburgite, amphibolites, diabase tuffs, cherts, gabbros, orthophyres, augite-porphyrites, porphyrites, basalts, pecrite-porphyrites, andesites, trachytes, dacites, diorites, granites, syenites, quartz-porphyries, alnoite and a series of much altered rocks. The descriptions are all brief.

[1] Annual Rep. Geol. Surv. of Canada, Vol. VII, Pt. B., p. 349,

Chalcedony Concretions in Obsidians from Colorado.—Patton[3] describes the occurrence of large opal and chalcedony concretions or geode-like bodies in beds of a decomposed obsidian on Ute Creek in Hinsdale Co., Colorado. The concretions are most common in the upper scoriaceous portions of the flows. Similar concretions were also found in a rhyolite at Specimen Mountain. The concretions are composed of radial fibres of chalcedony. The flowage lines that are common to the rock pass uninterruptedly through them, and in them are trichites exactly like those in the body of the rock. The concretions are regarded as secondary in origin—and as due to the percolation of silica-bearing waters through the rock. The same author publishes some photographs of erosion forms produced by the weathering of the volcanic conglomerates in the San Juan Mountains.

Basic Dykes near Lake Memphremagog.—According to Marsters[4] the Chazy limestones of Lake Memphremagog are cut by granite, olivine, diabase and lamprophyre dykes. The latter comprise dark rocks containing phenocrysts of augite, hornblende or olivine. The olivine, when it occurs, is always situated in the central portions of the dykes. Sometimes its crystals are one and half inches in diameter. Petrographically these rocks are augite camptonites, fourchites and monchiquites. The augite camptonite contains both augite and hornblende in two generations and in varying quantities. Only two fourchite dykes were observed. Their material presents no unusual features. The paper is interesting as bringing to our knowledge another area in which these peculiar and interesting dyke rocks occur.

The Origin of the Maryland Granites.—The last article written by the late Dr. Williams[5] is an introduction to Keyes article on Maryland granites. In this paper the author explains the criteria by which ancient plutonic rocks may be recognized in highly metamorphosed terranes, and applies the principles thus established to prove the eruptive nature of many of the Maryland granites. The pegmatites of the Piedmont plateau were tested by the same criteria, with the result that these too are pronounced to be eruptive. Many handsome plates embellish this portion of the paper. In the main portion of the article Keyes describes the petrographical features of the different types of granite, giving special attention to the original allanite and epidote found in them. There is little that is new in the paper, most of its

[3] Proc. Colo. Scient. Soc , Nov. 4, 1895.
[4] Amer. Geol., July, 1895, p. 25.
[5] 15th Ann. Rep. U. S. G. S., 1895, p. 653.

essential points having already been discussed by Hobbs, Grimsley and others.

Petrographical Notes.—The rocks of the Laurentian area to the north and west of St. Jerome, Quebec, are briefly referred to by Adams[6] as gneisses, anorthosites, amphibolites, limestones, quartzites, etc. Some of the gneisses are eruptive and others are probably sedimentary.

Miller and Brock[7] have found in Frontenac, Leeds and Lanark Counties, Ontario, granites, gabbros, scapolite and pyroxene rocks of Laurentian age cut by dykes of quartz gabbro containing phenocrysts of pyroxene and plagioclase.

Keyes[8] declares that the granites and porphyries occuring in the eastern portion of the Ozarks, in Missouri, "are very closely related genetically, and are to be regarded as facies of the same magma," the porphyry being the upper and surface facies of the granite.

GEOLOGY AND PALEONTOLOGY.

Canadian Paleontology.—In addition to the vertebrates (reptilia and batrachia) and land snails discovered by Sir Wm. Dawson in the interior of erect trees in the coal formations of Nova Scotia, and described by him in various scientific publications, fragments of arthropods have been found in the material collected. These were submitted for examination to Mr. Samuel Scudder who published a preliminary report in 1882, and now, after completing his study, gives these additional facts. A few species of Myriapods show traces of the bases of spines; the ventral plates in Archiulus are very broad; two new species of this genus are recognized; two species of Mazonia are indicated, one of which (*M. acadica*) confirms the separation of this genus from Eoscorpius; a facetted eye taken from a reptilian coprolite shows the presence of a true insect, probably a cockroach.

A report upon the Cenozoic Hemiptera of British Columbia, by the same author, comprises descriptions of nineteen species. Mr. Scudder calls attention to the great variety among these insects. Among the Homoptera, every specimen must be referred to a distinct species, and

[6] Ann. Rep. Geol. Surv. of Can., Vol. VII, J., p. 93.
[7] Can. Record of Science, Oct., 1895.
[8] Bull. Geol. Soc. Amer., Vol. 7, p. 363.

in only one case can two species be referred to one genus. In the Fulgoridae each of the three species belongs to a different subfamily. Another striking feature of the fauna is the size of the individuals which compose it. The majority of them represent the most bulky species of their respective families. The average length of these Cenozoic species of Fulgoridae and Cercopidae is not less than two centimeters, and there are some that are double that length.

The author states that this insect fauna indicates that the deposits in which they occur are at least as old as Oligocene, but no definite statement as to the age of the beds can be made.

A third interesting paper in this series on Canadian fossil insects sums up the present knowledge of the Coleopterous remains of Canada. These have been found in seven distinct localities in that country, and at three very different horizons. The greatest interest attaches to the collection made at an interglacial locality near Scarboro' Ont., which yielded twenty-nine species, and is the largest assemblage of insects ever found in such a deposit anywhere. Forty-five species from the various localities are described by Mr. Scudder. They are referred to 27 genera, 2 of which are new. (Contrib. Canadian Paleontol., Vol. II, Pt. I.)

Jackson on the Development of Oligoporus.—The following is an abstract of the results of the recent studies of the Palæoechinoidea. In Oligoporus the interambulacra terminate ventrally in two plates, which present on their oral faces a reëntrant angle for the reception of a single initial plate of the area. Proceeding dorsally, new plates and new columns of plates are added, accenting by their appearance stages in growth, as he had previously shown in Melonites, until the full compliment of the species is attained. The single initial interambulacral plate of Oligoporus was compared with a similar plate in Melonites, Lepidechinus, young modern Cidaris, etc. At the ventral or younger portion of the corona of Oligoporus there are only two columns of ambulacral plates. The four columns characteristic of the adult are derived from these two by a drawing-out process. The four columns of ambulacral plates of adult Oligoporus are the equivalent of the two outer and two median columns of Melonites. These four columns in both genera are the morphological equivalent of the two columns seen in the ambulacra of Bothriocidaris, Cidaris, etc.

Oligoporus, as shown by the development of both ambulacral and interambulacral areas, is a genus intermediate between Palæechinus and Melonites. During the development of Oligoporus it passes

through a Rhoëchinus stage, and later a Palæechinus stage. Melonites in its development passes through an Oligoporus stage.

An early stage in developing Echinoderms was named the "protechinus" stage. At this stage are first acquired those features which characterize the developing animal as a member of the Echinoidea. The protechinus stage in Echinoderms is directly comparable to the protoconch of Cephalous Mollusca, the protegulum of Brachiopods, the protaspis of Trilobites, etc. The Echinoderm at this period in its growth has a single interambulacral plate (representing a single column of such plates), and two columns of ambulacral plates in each of the five areas. This stage is seen in Oligoporus, Lepidechinus, Goniocidaris and other genera; it finds its representative in an adult ancestral form, in the primitive, oldest known genus of the class Bothriocidar is of the Lower Silurian, which has but one column of interambulacral and two columns of ambulacral plates in each area.

Species of Oligoporus and Melonites with few interambulacral columns are considered the more primitive types, as they are represented by stages in the development of those species which acquire a higher number of columns in the adult.

The structure of the ventral border of the corona of Archæocidaris was described. It presents a row of plates partially resorbed by the encroachment of the peristome, as in modern Cidaris, etc. Ambulacral and interambulacral plates on the peristome were described in Archæocidaris, also teeth and secondary spines on the interambulacral plates of the corona.

This paper contains a classification of Palæozoic Echini based on the structure and development of the ambulacral and interambulacral areas and the peristome. It will be published in the Bulletin of the Geological Society of America.—Science, Nov. 22, 1895.

American Fossil Cockroaches.[1]—This memoir, published as Bulletin 124 of the U. S. Geological Survey, is a revision of the known species of American fossil cockroaches to date. The descriptions of new forms are interpolated in a systematic list of all the species yet recovered from the rocks, and such tables have been added as may enable the student to readily determine any new material. With the publication of this essay all species hitherto described will have been figured.

[1] Bulletin of the United States Geological Survey, No. 124. Revision of the American Fossil Cockroaches, with Descriptions of New Forms. By Samuel H. Scudder, Washingtou, 1895.

The new forms are Paleozoic, and are mostly from two new localities—Richmond, Ohio, and Cassville, West Virginia. There are, however, a number of new species from old horizons.

Tables of the geographical and also of the geological distribution of both American and European genera are given in the introduction, followed by a statement of the characteristics of the Mylacridae and a discussion of some of the anatomical features of paleozoic cockroaches. In this connection the author calls attention to possible mimicry among these old forms of insect life, and figures side by side a cockroach wing and a fern frond found associated in the same beds, to show how close is the resemblance between them in the general distribution of nervures and in outline.

The illustrations comprise twelve page plates and three figures in the text.

The Comanche Cretaceous.—Prof. R. T. Hill has found some outlying areas of the Comanche series in Barber and Comanché Counties, Kansas, and in G County, Oklahoma, and in the Tucumcari region of New Mexico. These strata are identified from paleontological evidence.

The importance of a correct determination of these beds is evident from the following concluding remarks of the author.

"The geology of the outlying areas of the Cretaceous preserved in the scarps of the Plains adds greatly to our knowledge of the distribution, variation, paleontology and history of the beds of the Comanche series, and of the progressive oscillatory conquest of the Great Plains region by the sea in Cretaceous time. The Belvidere (Kansas) beds have revealed the following additions to our knowledge of Cretaceous paleontology: First, a lower stratigraphic occurrence of the dicotyledonous Dakota flora than known, whereby we may now say that dicotyledons make their first appearance before the beginning of the Washita subepoch, instead of in the Dakota as hitherto believed. Second, a similar downward range in the geologic scale of the ichthyic vertebrates of hitherto supposed Upper Cretaceous range. Third, intermingling of these plants and fishes with molluscan species and other vertebrates of the Washita division such as has not hitherto been found in the Comanche series." (Amer. Journ. Sci., Lol. L, 1895).

Kolguev Island, which lies 130 miles southeast of Novaya Zemlya, differs, according to Col. Feilden, in geological structure, both from mountainous islands of its neighbor and from Russian Lapland. The entire elevated region of the island is composed of beds of sand contain-

ing erratic boulders, to a depth of not less than 80 feet, and these sandy beds rest on the Kolguev clays. These in turn are 50 miles long by 40 wide, with a thickness of not less than 250 feet, probably more. This great mass is evidently a glacio-marine deposit. A few molluscan remains were found in it, all well known boreal forms existing at the present time, but no vertebrates nor drift-wood. A collection of erratics made by the author are identified by Prof. Bonney as rocks of Mesozoic age, either Jurassic or Wealden. (Quart. Journ. Geol. Soc., 1896.)

Palæontologia Argentina.—Vols. I (1891), II (1893), and III (1894).—The Museo de la Plata of Argentina has progressed thus far with the publication of monographs illustrative of its magnificent collection of fossil vertebrata of that country. The style of the publication is worthy of the subject; the size selected being folio, and the plates phototype reproductions of the originals, often of the natural size. The whole is issued under the supervision of the director of the Museum Dr. Francisco P. Moreno, who contributes some of the articles in connection with M. Mercerat; while Dr. Lydekker, of London, furnishes the greater number.

The first volume, on the extinct birds of Argentina, consists solely of plates, with pages of names referring to the figures. These plates depict objects of great interest, many of the bones belonging to the extraordinary family of the Phororhacidæ of Ameghino, which seem to be nearly allied to the existing Cariamidæ of South America. Most of these birds are of gigantic size, and their powerful legs and hooked beaks indicate that they were quite competent to maintain their place in the fauna of which they form a part. We have waited for some years before noticing this valuable publication, in hopes that the text would appear. It seems, however, that there is no intention of publishing a descriptive part. Under the circumstances we must regret that names were attached to the figures, for, although figures may give currency to specific names, they cannot do so for names of any higher grade, and a considerable amount of synonymy has been thus created. Dr. Ameghino has also subsequently shown, that in this atlas a good many duplicate names have been given to the same species.

In the second part are published three memoirs by Dr. Lydekker. These include figures and descriptions of Dinosauria and Cetacea from Patagonia, and mammalia Ungulata from the same region. The magnificent plates are accompanied by descriptions, and this volume is therefore more valuable than its predecessor. Unfortunately the de-

scriptions are quite inadequate, and the specimens will have to be more fully described before their characters can be sufficiently known.

The third volume is chiefly occupied with the Edentata, and this memoir is admirably illustrated. The descriptions (by Dr. Lydekker) are rather more full than those of Vol. II, but not full enough. They are marred by frequent supercilious references to Dr. Florentino Ameghino, who is the most competent paleontologist of the vertebrata in South America, and whose descriptions compare very favorably with those of other paleontologists in all respects. His figures are not so good as those of the work now under review, for here we have a case in which the most skilful hand has not had the financial advantages it ought to have had. From our past experience we should say that when Dr. Lydekker states that organic forms are distinct species he is apt to be correct; but when he identifies forms alleged to be distinct, further examination is in order.—C.

BOTANY.[1]

Tilden's American Algæ.—The first century of this distribution by Josephine Tilden, of Minneapolis, was sent out about a year ago, but has not hitherto been noticed in these pages. The specimens are very neatly prepared, and are attached to cards or mica slips. In most cases they contain an abundance of material, but, in a few instances, we might wish for more generous specimens. The species represent the following genera:

Oedogonium (4), *Sphaeroplea* (1), *Hormiscia* (2), *Chaetophora* (4), *Draparnaudia* (3), *Stigeoclonium* (6), *Conferva* (1), *Microspora* (1), *Urospora* (1), *Cladophora* (15), *Pithophora* (1), *Vaucheria* (5), *Botrydium* (1), *Hydrodictyon* (1), *Tetraspora* (2), *Palmella* (1), *Protococcus* (3), *Euglena* (1), *Spirogyra* (10), *Cosmarium* (1), *Porphyrosiphon* (1), *Symploca* (2), *Lyngbya* (2), *Phormidium* (1), *Oscillatoria* (8), *Spirulina* (1), *Gloeotrichia* (2), *Tolypothrix* (1), *Nostoc* (3), *Anabaena* (2), *Merismopedia* (1), *Navicula* (1), *Pleurosigma* (1), *Gomphonema* (2), *Cocconeis* (1), *Nitzschia* (1), *Odontidium* (1), *Synedra* (2), *Fragilaria* (1), *Cystopleura* (1), *Lysigonium* (1).

The introduction of *Euglena* among plants is, in our opinion, a mistake, although one which will probably do no harm, since it will be difficult if not impossible to recognize them from dried specimens.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

Century II is announced to appear soon. We bespeak for it a liberal patronage.—CHARLES E. BESSEY.

The Columbines of North America.—Thirteen species of *Aquilegia* are described as occurring in North America in Robinson's edition of Gray's Synoptical Flora (1895).

These fall into two types, as follows:

A. Old World type, with hooked or curved spurs:

A. brevistyla, Rocky Mountains of British America, and the Black Hills of South Dakota.

A. saximontana, Rocky Mountains of Colorado.

A. flavescens, Pembina and British Columbia to Oregon and Utah.

A. micrantha, southeast Utah.

A. ecalcatata, southwest Colorado.

A. jonesii, northwest Wyoming and Montana.

B. American type, with straight spurs:

A. canadensis, common east of the Rocky Mountains.

A. formosa, Alaska to northern California, Idaho and Utah.

A. truncata, California.

A. caerulea, Rocky Mountains from Montana to New Mexico.

A. chrysantha, southern Colorado to New Mexico and Arizona.

A. pubescens, California.

A. longissima southwest Texas.

It is interesting to note that in Torrey and Gray's Flora of North America (1840) there were but four species described, viz.: *A. canadensis*, *A. formosa*, *A. caerulea* and *A. brevistyla*. It is possible that some of these species may be reduced to varieties upon a more critical study of the genus, but even with the most rigid reduction we should still be left with a large representation of these interesting plants. Their curious beauty and comeliness, with their general distribution, may well warrant the suggestion which has been made to make the Columbine our national flower.—CHARLES E. BESSEY.

Sets of North American Plants.—Two sets of peculiarly interesting North American flowering plants attract the attention of herbarium curators at this time. They consist very largely of species from Florida, that wonderfully rich semi-tropical region whose botanical treasures we are just learning to appreciate. The first is a set of 400 specimens by the veteran collector A. H. Curtiss, of Jacksonville, Florida. A personal examination of the specimens warrants the same high commendation which all of Mr. Curtiss's work has hitherto received.

The second set is published by G. V. Nash, of Washington, D. C., and includes the same number of specimens. A glance at the list shows it to include many rare and a considerable number of new species. Either set would be a valuable acquisition to any college herbarium.—CHARLES E. BESSEY.

Botany in Buffalo.—The Secretary of the Section of Botany (G) of the American Association for the Advancement of Science, Professor George F. Atkinson, of Ithaca, N. Y., is making an effort to provide a good programme for the meeting in August (24 to 28). Titles and abstracts of papers are to be sent to the Secretary not later than July 1, in order that they may be arranged and forwarded to the Permanent Secretary of the Association for printing and distribution. It is the purpose of the Association to issue such a list of Section programmes not less than a month preceding the meeting. Let every botanist who has something of importance send in his title *and abstract* on or before the first day of July.

The second annual meeting of the Botanical Society of America which will be held on August 21 and 22, in connection with the Association, should attract a good number of the more advanced men in the science. Dr. Trelease, the retiring president, will deliver his address on "Botanical Opportunity" at 8 P. M. of the 21st. On the 22d there will be forenoon and afternoon sessions for the reading of papers and discussions.—CHARLES E. BESSEY.

Blanks for "Plant Analysis."—For some time there has been an encouraging decrease in the annual crop of blanks for "plant analysis," and we hoped to be able soon to announce the complete extinction of the species. It appears, however, that there are certain intellectual soils in which they still thrive, in spite of the fact that, like the Russian Thistle, they are outlawed in most communities. We have before us two which bear the date 1896, one from U. O. Cox, of Mankato, Minnesota, and the other from H. J. Harnly, of McPherson, Kansas. If one may distinguish between things which are necessarily bad, it may be said that the first is the better of the two. Its fault (which is fatal) is that it enables the pupil to "analyze" a plant with the least possible thinking: he does not have to remember anything; he merely reads the question, looks at his plant, and makes his entry on the proper line. The second blank (which is "copyrighted") adds to the foregoing much which is confusing and scientifically vicious. Thus the pupil finds the questions "Flowers, Regular or Irregular? Why?" which he is expected to answer in a line just *two and a half inches long!*

Again he is asked, "Flowers, Complete or Incomplete? Why?" and is allowed a line exactly two inches long in which to give an answer to a question before which the wisest botanist may well quail. When will teachers realize that botanists are not made by the use of such "helps" any more than Latin scholars are made by the use of "ponies"?

—CHARLES E. BESSEY.

Botanical News.—The Director of the Missouri Botanical Garden at St. Louis calls attention in a printed circular to the advantages for study afforded by this important institution. Its herbarium includes nearly 250,000 specimens, and its library about 10,000 volumes and 11,000 pamphlets.

A. H. Curtiss, of Jacksonville, Florida, is distributing fine sets of the Marine Algæ of Florida. Each set contains fifty species and is sold for five dollars.

Professor Bruce Fink, of Fayette, Iowa, offers sets of Iowa Lichens, including about 200 species which he sells at the low price of six cents each.

We are glad to see another number of *Pittonia*, the very useful periodical which Professor E. L. Greene issues from time to time. The new part (13) contains papers on the Nomenclature of the Fuller's Teasel, a Proposed New Genus of Cruciferae; New or Noteworthy Species; New Genus of Polemonianae, and New Mexican Eupatoriaceæ—CHARLES E. BESSEY.

ZOOLOGY.

Japanese Leeches.—The discovery of three new land leeches in Japan is of interest to geologists since but one species, *Haemadipsa japonica* Whitman, is all that has been known to occur in that country. The three new species are members of a genus separated from all the genera of land leeches hitherto defined. An account of their external characters and a general outline of their internal organization are presented by Dr. Asajiro Oka in a recent number of the journal published by the Imperial University of Japan. For the new genus the author proposes the name Orobdella. The species of this genus are found in various mountainous parts of Japan, crawling under moss and fallen leaves, or in moist earth, in the same manner as earthworms, which con-

stitute the chief source of their nourishment. Having no jaws, these leeches can neither bite nor suck blood, but swallow the worms entire. *O. octonaria* is one of the largest leeches known. The dimensions of one specimen found by the author is given, length 270 mm., width 14 mm., depth 10 mm.

Dr. Oka adopts the classification of R. Blanchard (1894), and shows the systematic position of Orobdella in the following synoptical table:

Ordo Hirudinea.
- a. Subordo. Rhyncobdellae.
- b. Subordo. Arhyncobdellae.
  - 1. Fam. Gnathobdellidae.
    - Aquatic: gen. Hirudo, Haemopis, etc.
    - Terrestr. gen. Haemadipsa, Xerobdella, Mesobdella.
  - 2. Fam. Herpobdellidae.
    - Aquatic: gen. Herpobdella, Dina, Trocheta.
    - Terrestr: gen. Cylicobdella, Lumbricobdella, Orobdella.

(Journ. Coll. Sci. Imp. Univ. Japan., Vol. VIII, Pt. 2, 1895.).

The Origin of Tail-forms.—The use and meaning of the asymmetrical types of tail-fin which are so commonly met with among fishes—e. g., the upturned tail of the shark and sturgeon, and the downwardly extended fin of the flying-fish, are explained by Dr. F. Ahlborn by comparisons founded on experience in rowing. Every tyro knows the consequences which ensue if he holds his blade too obliquely in the water. If the upper edge is inclined too much towards the stern of the boat a brisk pull upon the handle results in the blade jumping out of the water; if, on the other hand, the blade is inclined too much in the opposite direction, it digs into the water and the oarsman "catches a crab." The relevance of these illustrations is found in the fact that the skeletal support of the asymmetrical tails of fishes is generally such that either the upper or lower border of the fin is more resistant to the pressure of the water than the opposite border, a fact which causes the fin in action to assume an oblique instead of a vertical position. The result of such a disposition is that in those cases where the upper part of the tail is stiffer than the lower, the tail in locomotion is driven upwards, as the oar is driven out of the water (heterocercal tail of shark and sturgeon); while in cases where the lower part of the tail is firmer than the upper, the tail tends, in action, to assume a lower position than the rest of the body (flying-fish). The body of the animal, in fact, is made to swing vertically about a horizontal axis running through the center of gravity: in the first group

the tail becomes elevated above the head, in the second group the head becomes raised above the tail. The utility of these types of organization becomes obvious when the habits of the creatures which exhibit them are considered. The first group consists of bottom-haunting fish, which are thus enabled to give free play to their tails while scouring the sea-bottom in search of food; the second consists entirely of surface-swimming forms which are enabled, by this beautiful adaptation of structure, to swim swiftly beneath the surface of the water without the risk of their tails emerging, and so cause inconvenience and waste of force. The tails of many air breathing aquatic animals, such as the sea-snake and the extinct Ichthyosaurus are constructed upon this latter principle. (Nature, Feb., 1896.)

The Spermatheca in some American Newts and Salamanders.—The term *receptaculum seminis* has been used to designate certain structures in the cloacal wall of the female *Necturus maculatus*, which serve as reservoirs in which the zoösperms of the male are received. In order to have a better understanding of the function of these structures, Dr. Kingsbury undertook a study of the cloaca in the female of six species of Urodeles (American). The chosen species represent five families, and two orders of Batrachia, and present a good series from a purely aquatic to as purely a terrestrial existence. The general result has been a recognition of these organs in one form or another in all the species under observation, but there is no unity of structure, hence the term *receptalum seminis* is not strictly applicable, and the mononym *spermatheca* is proposed instead. In some forms many spermathecas would be recognized.

In Diemyctylus, Amblystoma and Necturus the spermathecas assume the form of individual tubules. In Amblystoma the tubules are arranged around depressions. In Spelerpes, Plethodon and Desmognathus consists of a tubular depression of the cloaca into the end of which the clustered tubules open.

As to how the spermatozoa find their way into these resting places, the author suggests that while the theory of Pfeffer of "positive chemotaxis" is highly probable, yet it is also possible that the entrance of the zoösperms may be solely due to their own activity assisted by muscular contractions of the cloaca and spermatheca.

The results of Dr. Kingsbury's observations are thus summarized:—

1. In the genera Necturus, Amblystoma, Diemyctylus, Plethodon and Desmognathus, spermathecas are found in the dorsal wall of the

cloaca of the female, containing zoösperms. Internal fertilization is therefore proven for these forms.

A spermatheca occurs in Spelerpes; in the single specimen examined (taken in the fall) no zoösperms were contained.

In Necturus, Diemyctylus and Amblystoma, there are several tubules or spermathecas opening upon the cloacal epithelium, which serve as reservoirs for the semen.

In Desmognathus, Plethodon and Spelerpes, there is a single mesal spermatheca.

The condition in Spelerpes would seem to indicate that the organ in these latter genera equals the group of tubules found in the first genera plus and exaggerated and modified depression of the cloacal epithelium, such as occurs in Amblystoma.

2. No gland-like structures in addition to the spermatheca occur in the female of Plethodon and Desmognathus.

3. In all the remaining genera a ventral cloacal gland is present.

4. In Amblystoma, Spelerpes and Necturus, in addition to the spermatheca tubules, other tubules occur on the dorsal side of the cloaca.

5. The secretion of the cloacal glands is employed at the time of ovulation.

6. The three glands of the male recognized in the Triton, the cloacal, abdominal and pelvic, occur and are well developed in the five genera examined. This suggests that by all of these spermatophores are deposited.

7. A résumé of the literature and foregoing facts points to a uniform mode of mating and fertilization in all urodeles.

8. Dorsal and ventral ciliated tracts occur in the male of all the genera examined. Cilia in the cloaca of the female were detected only in Amblystoma and *Plethodon glutinosus*, where the tract was not as extensive as in the male. (Proceeds. Amer. Microscop. Soc., Vol. XVII, 1895.)

Zoological News.—A second species has been added to the genus Opisthoteuthis founded by Verrill to receive a West Indian species named *O. agassizi*. The new acquisition was obtained by a Misaki fisherman with a hook at a depth of about 25 fathoms in Iagami Bay, Japan. It is described and figured by Dr. Ijima and S. Ikeda under the name *O. depressa*. (Journ. Coll. Sci. Imp. Univ. of Japan, Vol. VIII, Pt. 2, Tokyo, 1895.) This genus is characterized by the fact that the alimentary canal passes directly through the body, instead of

returning to issue near the mouth. Ferrill regards it as the most primitive form of the Cephalopoda.

A new genus of Cottoid fishes from Puget Sound is described by Mr. E. C. Starks. The type species, *Jordania zonope* is in the Museum of the Leland Stanford, Jr., University. (Proceeds. Phila. Acad. Nat. Sci. [1895] 1896).

Mr. J. A. Allen emphasizes the fact that the change of color in the plumage of birds without moulting is due to the gradual wearing off of the light colored edges of the feathers, combined with the more or less blanching of the color of certain parts. Exposure to the elements and friction also produce more or less marked change in color. The author prefaces his remarks with a brief history of origin and persistence of the theory unwarranted by the facts that the feathers of birds change color with the season independent of the process of moulting. (Bull. Amer. Mus. Nat. Hist., Vol. VIII, 1896.)

ENTOMOLOGY.[1]

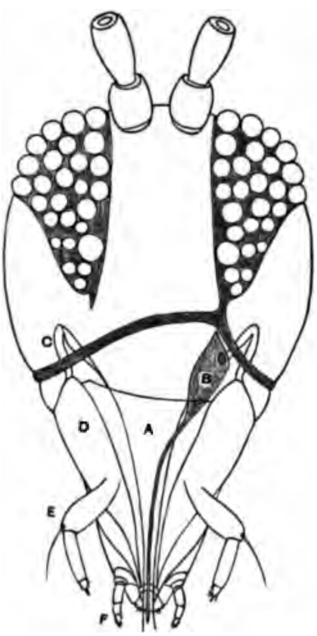
The Asymmetry of the Mouth-parts of Thysanoptera.—In the Bulletin of the Essex Institute, for 1890, Vol. XXII, the writer published a brief account of some peculiarities he had observed in the mouth-parts of members of this order of insects, and ventured in explanation, the hypothesis that in these insects the mandible of the right side of the head is wanting, and that the parts commonly regarded as mandibles are lobes of the maxillæ. Subsequently the writer called this anomalous condition of the mouth-parts to the attention of members of the Entomological Club of the American Association for the Advancement of Science (Indianapolis meeting, August, 1890) and presented slides showing the peculiarities described. (See Canadian Entomologist, 1890, Vol. XXII, p. 215.)

Nothing, so far as I know, has appeared in American literature since that time with reference to the matter, and the old view concerning the structure of the mouth seems to be still current. In Prof. J. H. Comstock's excellent manual, recently issued (1895) the labrum is represented as perfectly symmetrical, the parts considered by him to be mandibles are incompletely represented, and no mention is made of

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

any unusual feature of the mouth structure. In giving the characters of the order, he says: "The mouth-parts are probably used chiefly for sucking; they are intermediate in form between those of the sucking and those of the biting insects; the mandibles are bristle-like; the maxillæ are triangular, flat, and furnished with palpi; the labial palpi are also present."

I have just examined a copy of Jindrich Uzel's "Monographie rádu Thysanoptera," 1895, perhaps the most extensive work yet published on the Thysanoptera, in which the view of the mouth structure described by me in the Bulletin of the Essex Institute and before the Entomological Club is adopted, though Uzel is disposed to take a different view of the homology of the unpaired mouth-part. His words are (Ibid, p. 25): "In der Höhlung des Mundkegels bewegen sich die Mondibeln in Form zweier Stechborsten und der unpaare Mundstachel (wohl ein umgebildeter epipharynx) welcher linkerseits liegt und den für die Thysanopteren characterischen Unsymmetrischen Bau der Mundwerkzenge bedingt." In order to show more clearly what the unpaired part is like, I have made a drawing from his Figure 161, Tab. IX, which is here reproduced.



Of the interpretation Uzel is disposed to put upon the unpaired part, I have only this to say: It is plainly closely adapted to the left side of the head, and the parts belonging to the region in which it lies are closely adapted to it. It is very evident that it was made to fit in the angle formed by the hard parts of the head on the left side; the labrum is, on this side, shortened and otherwise suited to accommodate it. A reëxamination of my slides shows the adjustment more complete even than represented in Uzel's figure. Coupled with this is a manifest deficiency in the head on the right side at the place where a corresponding structure should be. It is evident that something is lacking on the right side. If the unpaired organ is an epipharynx that has been displaced, why should the cranial structure of the right side be altered?

The further question arises, why should an epipharynx be pushed to one side and completely shaped to the structures there?

I have suggested that the pair of slender parts, called by Uzel and others mandibles, may be lobes of the maxillæ, and urged in explanation that they are attached to parts regarded by everybody as maxillæ, and besides that they are composed of two divisions (Professor Comstock does not represent the basal piece at all and hence the slender distal part appears in his figures as if free from the maxilla). Uzel figures this pair of mouth organs, as I have done, attached to the bases of the palpus-bearing parts, and as composed of a short basal piece and a long slender distal one. He says nothing of their jointed character but represents an articulation in the right one of his figure 161. They appear to me to be two-jointed and with this true, to consider them mandibles is to assume a departure from the one-jointed condition of the mandible prevailing in Hexapoda.

In the proceedings of the Entomological Club of the American Association (Can. Ent., Vol. XXII, p. 216), I am reported as stating that two unmistakable tarsal claws are present in Plæothrips and that the vesicle is probably a modified pulvillus. Prof. Comstock says: "The tarsi are two-jointed, bladder-like at the tip, and without claws." Uzel, on the contrary, states that claws are more or less developed in all Thysanoptera. "Beine Kurz; der eine-bis zweigliedrige Tarsus am Ende mit zwei mehr oder wenlger deutlichen Klanen, welche an Blase anwachsen."

Prof. Comstock's work has been quoted simply because it represents the established American view of the structure of Thysanoptera, a view which must certainly be changed in some particulars. To what extent the asymmetry referred to has been studied by foreign entomologists I am unable to say, since I have not been so situated as to be able to keep close track of the foreign literature. Thus far I have seen no reference to it except that in Uzel's work.

Explanation of the Figure.

Front view of head of *Aeolothrips fasciata*. A, the unsymmetrical labrum; B, the unpaired mouth-part (mandible, according to my interpretion, epipharynx according to Uzel); C, lobe of maxilla (mandible of Uzel and other authors); D, the maxilla; E, the maxillary palpus; F, the labial palpus.—H. Garman.

A New African Diplopod Related to Polyxenus.—While collecting insects in the darker parts of the Liberian forests, I have on

a few occasions noticed what appeared to be large individuals of Polyxenus of a dark brownish color, running about on the smooth leaves of the shrubby undergrowth, several feet from the ground. To preserve and carry to America specimens in satisfactory condition is not easy, and hence the present purpose of describing the external features of one found yesterday and having nearly all the bristles still in place.

SAROXENUS g. n.

Body minute, tapering caudad.

Head rounded, not as broad as the first segment; between the eyes with an anterior crescentic tiara of long upright serrate bristles: on each side between and above the eyes a short curved line of similar hairs elsewhere the head is smooth.

Eyes of a few (six?) small ocelli clustered on lateral prominences of the head.

Antennæ long and slender, distinctly clavate; sixth joint longest and much the thickest; seventh slightly longer than any of the proximal; eighth joint distinct, minute, several times smaller than the seventh.

First segment with six tufts of bristles, two in front, two behind and one on each side; the dorsal tufts are broader transversely; the lateral are raised on large projections, as in Polyxenus, and include more numerous and longer bristles.

The following six segments have each four tufts of similar bristles, two lateral and two posterior, the latter broad, as on the first segment; the bristles are longer and the tufts larger on posterior segments.

Last segment with a nearly complete transverse row of divergent bristles just in front of the dense brush of much finer, closely compacted bristles which compose the terminal fascicle.

Saroxenus scandens sp. n.

General color dark grayish brown, the terminal fascicle nearly white; in alcohol and under the microscope, the bristles of the head and segments are seen to be dark brown; the distal joints of the antennæ and legs are pinkish brown, and the exposed portions of the integument have a tinge of the same color; integument generally waxy or dirty white, and transparent so that the contents of the alimentary canal are visible as a dark line; eye spots dark brown.

Segments 8, though the specimen may not be mature; ten pairs of legs.

Length 3.5 mm., or with the terminal fascicle 4 mm.; width 1.2 mm., including the bristles.

Locality, Running about on the leaves of undergrowth, in the forest on Cape Mesurado, Liberia.

Under sufficient magnification the bristles of the head and segments appear as round hollow structures with about four longitudinal rows of very fine appressed teeth directed distad. The bristles of the terminal fascicle are more slender and have for a part of their length large appressed spines in opposite pairs something as shown by Latzel for *Polyxenus lagurus*. Nothing was seen similar to the apices of the hairs as figured by the same author.

This new genus is to be distinguished from Polyxenus and Lophoproctus[2] by the form of the antennæ and the distribution of the dorsal setæ. In Polyxenus the antennæ are short; in Lophoproctus they are long, but the apical joint is subequal to the penultimate.

Polyxenus has two transverse dorsal rows of rather remote short clavate and strongly serrate setæ, while Lophoproctus has a single row. The type of the latter genus is eyeless, although Mr. Pocock proposes to iuclnde a species with eyes, *Polyxenus lucidus* Chalande.

From the West Indes Mr. Pocock has described another Polyxenus[3] which, to judge from the drawing, has four tufts of setæ on each segment, and also a scattering row along the posterior margin. The antennæ are said to be very long, but appear not to be clavate, and the relative proportions of the joints are not stated. It is probably the type of a new genus having affinities with the African rather than with the European forms.

By the discovery of Saroxenus the distribution of the Pselaphognatha is considerably extended. Should members of the group be found in other tropical regions there will be added assurance of the antiquity of the subclass, and of the probability of relationship with such fossils as Palæocampa.—O. F. Cook.

Monrovia, 1 Feb., 1896.

North American Crambidæ.—Dr. C. H. Fernald publishes as a bulletin from the Massachusetts Agricultural College an important Monograph of the Crambidæ of North America. The author has long been recognized as tne leading authority on the micro-lepidoptera. The new genera Eugrotea and Pseudoschœnobius are characterized as well as several new species. The bulletin is admirably illustrated by three plates in black and white and six plates in colors, beautifully printed. This will certainly prove one of the most satisfactory entomological publications ever issued from the Agricultural Colleges.

[2] Pocock, Ann. Mus. Civ. Genova, XXXIV, 506.

[3] *Polyxenus longisetis*, Journ. Linn. Soc., XXIV, 474.

New Mallophaga.—Much the most important paper as yet published in America concerning the Mallophaga is the recent contribution from the Hopkins Seaside Laboratory, in which Prof. V. L. Kellogg treats of New Mallophaga, with special reference to a collection made from Maritime birds of the Bay of Monterey, California. In the 140 pages of print the author presents descriptions and figures of one new genus and thirty-eight new species of Mallophaga, together with twenty-two species previously described by European authors, but now, with few exceptions, first determined as parasites of American birds. In addition, the paper contains an excellent general account of the Mallophaga and fourteen admirable plates. It can be obtained for 50 cents by addressing The Registrar, Stanford University, California.

Entomological Notes.—Professor D. S. Kellicott publishes[4] the second part of his excellent Catalogue of the Odonata of Ohio. It deals especially with the species of the southern part of the State.

In Bulletin 32 of the Iowa Experiment Station, Messers. Osborn and Mally treat of the chinch bug, four-spotted pea-weevil, the imbricated snout-beetle and other injurious species.

Bulletin 62 of the Virginia Station contains a discussion of the San Jose Scale, by Wm. B. Alwood.

In Bulletin No. 2 of the Technical Series from the U. S. Division of Entomology, Mr. L. O. Howard publishes a careful account of The Grass and Grain Joint-worm Flies and their Allies, being a consideration of some North American Phytophagic Eurytyminæ.

In the issue of the Entomologist's Record for May 1st Mr. J. W. Tutt begins an interesting series of articles upon Mimicry.

In Bulletin 69 of the Ohio Station, the Chinch Bug is discussed at length F. M. Webster.

Prof. S. W. Williston publishes a useful Bibliography of North American Dipterology, 1878–1895, in the January Kansas University Quarterly. In the same issue W. G. Snow gives a List of Asilidæ supplementary to Osten Sackens Catalogue of North American Diptera, 1878–1895.

[4] Jour. Cin. Soc. Nat. Hist., XVIII, 105–114.

EMBRYOLOGY.[1]

Protoplasmic Continuity.—Prof. Hammar, of Upsala,[2] emphasizes by figures and description the connection of the cells of the egg of a cleaving sea urchin known to Selenka and others, but hitherto regarded as of no importance. He finds a thin outer layer on the cells of the early and later cleavage cells and even on the cells of the blastula. This layer is seen both in living and in preserved and sectioned material. Its appearance is not that of a membrane but, the author thinks, rather that of an "ectoplasmic" outer part of the protoplasm of the cell. This outer layer is very thin and might be easily overlooked.

It extends continuously over the entire egg and as it seems to be a part of each cell, all the cells are thus held together by a continuous outer pellicle that the author thinks is a protoplasmic layer.

This actual connection of the cells at their outer surfaces, if really a protoplasmic connection, should, as the author insists, be of great importance in the interpretation of the results of experimentation upon echinoderm eggs. He suggests that it offers a suggestion towards the explanation of the interaction believed to exist between the cells of a cleaving egg. Moreover such a connection would make clear why very different results have been obtained after shaking eggs and separating the cells *more or less*.

Cell Studies in Annelid Eggs.—Prof. E. Korschelt, of Marburg,[3] has made a most detailed and thorough study of the maturation and fertilization of the eggs of the small polychætous annelid, *Ophryotrocha puerilis* with special reference to the number of chromosomes concerned in cell divisions at different phases of the life history.

Many of the interesting facts described cannot be here referred to, but only some of those that bear upon the question of the value of chromosomes as permanent individuals.

The number of chromosomes found in dividing cells in the adult is *four*, in certain ectodermal, entodermal and mesodermal structures. This same number is found in the cells of the ovary and of the testis, the ancestors of the eggs and sperms. The same number is found in

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Archiv f. mik. Anat., Marz 2, 1896.

[3] Zeit. f. wiss. Zool. 60, Dec. 31, 1895, pps. 543–680, pls. 28–34.

the early stages of cleavage and, as a rule, in the later stages and in the blastula, but in the later stages of cleavage and in the blastula there are often cells that contain *eight*.

In the maturation of the egg four chromosomes come out of the net work of the resting nucleus and eventually four go into the first polar body and two into the second. This is brought about as follows: The four very long chromosome loops shorten and divide lengthwise into four cleft rods. When these come to the equatorial region of the first maturation spindle they have again closed together so as to form four simple rods. These separate in pairs and move towards the poles of the spindle without presenting any true mitotic division. The first maturation division is, therefore, a reducing division. Yet the first polar body receives four chromosomes, since the pair that approaches that pole divides, as if opening out where previously split, and thus four rods are formed. The same takes place at the inner pole and four are left for the second maturation spindle. In the second polar body two chromosomes enter by moving away from the other two left in the egg. As it cannot be determined whether the pair entering the second polar body are two halves of one of original ones or halves of two original ones it is not certain whether the second maturation division is a reducing or an equating division.

Though the chromosomes are usually short rods or elongated granules during the maturation division, there are many eggs in which they appear as long, bent or horse-shoe shaped rods.

Some exceptions to the above account must be emphasized as showing the inconstancy of number of chromosomes resulting from lack of synchrony between chromosomal divisions and other phenomena of the cell.

Thus in some cases the first polar body has but *two* chromosomes, since the preceding division of chromosomes is left out. In others *eight* chromosomes are found at the equator of the first polar spindle, formed by a precocious division of the four chromosomes!

Fertilization takes place normally just after the eggs are laid and the sperm enters, while the first maturation spindle is still patent. In abnormal cases fertilization may take place inside the parent which is hermaphrodite and may ripen sperms and eggs simultaneously. Such cases, however, lead to abnormal cleavages and even to fusion of separate eggs, and seem due to some pathological state of the egg.

When the sperm enters the egg radiations are formed behind it, and later in front of it, so that the middle piece of the sperm may be re-

garded as introducing the centrosome or the archoplasm, and it is probable that the sperm revolves through 180°.

The male and the female pronuclei both move toward the centre of the egg and combine, but not till they have both gone through complex and similar changes, including the appearance and dissolution of an enormous nucleolus. The two nuclei finally fuse when each contains *two* long, thread-like chromosomes.

The centrosome or archoplasm of the maturation spindle disappears and that of the sperm divides and furnishes the first cleavage spindle. At the equator of this spindle are found the four chromosomes, two of male and two of female origin. Each splits lengthwise and the eight separate, so that each daughter nucleus obtains two chromosomes of male and two of female origin.

For many important facts not mentioned here the reader is referred to the two hundred remarkably clear figures and the judicial statements found in the original.

PSYCHOLOGY.

A Study in Morbid Psychology, with some reflections.— (*Continued from page 518*). With regard to the religious (?) experiences of Ansel Bourne, I am not so shallow as to think we can determine in the case of hallucinatory voices whether or no the phenomena are entirely subjective. An attitude of what the late G. J. Romanes has called " pure agnosticism " seems the only philosophical one in these difficult cases.[1] There has arisen a dogmatism in science as narrow and as mischievous as that of the straitest sect amongst theologians.

[1] "No one is entitled to deny the possibility of what may be termed an organ of spiritual discernment. In fact to do so would be to vacate the position of pure agnositicism *in toto*, and this even if there were no objective or strictly scientific evidence in favour of such an organ, such as we have in the lives of the saints, and in a lower degree, in the universality of the religions sentiment." A Candid Examination of Religion, p. 149, G. J. Romanes.

"Scientific men, as a class, are quite as dogmatic as the strictest sect of theologians. They professed to be agnostics, at the very time they were egregiously violating that philosophy by their conduct." Ibid., pp. 107–9.

But we can, I think, establish it as a law that "supernatural revelations" invariably take their colour from the preconceived ideas of the recipients. Probably, upon any hypothesis, they could do no otherwise.

One antecedent factor, in sudden conversions from the worldly to the religious life is often found in the physical effect of a severe illness; also those who have been rich in spiritual (?) experiences have often had indifferent health from childhood, with a liability to neurotic attacks. But there are too many exceptions to spiritual (?) experiences being the result of either temporary or permanent ill health, to enable us to say that such experiences are simply the result of disordered health, though they may be coincident with it.

I suppose that Socrates would universally be accepted as a type of moral and physical healthiness. Yet throughout his life he was subject to the promptings of what he himself calls an "inner, divine voice," which warned him if any evil were likely to befall him. In his sublime address to the judges who had just condemned him to die Socrates said that his "inner divine voice" had given him no warning of evil when he had left his house that day on which he was condemned to death, yet as it had always hitherto warned him of danger even on the most trifling occasions, he took its silence to mean that death was a good and not an evil. For even if death be a dreamless sleep, is it not a real and precious boon; and if it be as some say (and as Socrates himself hoped) only a passage from one state of being to another, to a place where all who have left this life are assembled, what greater good could man desire? To attain such happiness, Socrates would himself die many times.[1] Only in his own mind could the great philosopher seek for evidence of the one Supreme Being; for he could not believe in the popular theology which accepted gods who had committed acts which would have been disgraceful in the vilest of men. Therefore, it is not surprising that his "inner divine voice" did not profess to come from any supernatural power, but simply warned him of evil.

In the case of Mahomet we have a man of great strength of constitution, but one who was subject to strange attack, whether of epilepsy, catalepsy or hysteria is still a subject of doubt. What is certain is that he had a tendency to see visions, and suffered from fits which threw him at times into a swoon *without loss of inner consciousness.*

Through his intercourse with certain holy ascetics of the desert known as Hanifs, Mahomet became possessed with a profound sense of dependence on the omnipresent and omnipotent God. He withdrew to

[1] Apologia, XXXI, XXXII.

the solitudes of the bare and desolate Mount Hira, and meditated there with prayers and ascetic exercises. This state of things continued for many years, when in the month of Ramadan [which it will be remembered entails the severest form of fasting] the final revelation came which converted an illiterate Cameldriver[3] into one of the great religious teachers of the world. As he was repeating his pious exercises and meditations on Mount Hira the "divine voice" came to him. The angel Gabriel held a silken scroll before him, and bade him, though he could not read, recite what stood written in it. The words with which Gabriel had summoned him remained graven on his heart, and are found—as Mahomet at least imagined he heard them—in the 96th Sura of the Koran.

Mahomet returned to his wife Khadijah in great distress imagining he was possessed. But she comforted him and impressed upon him the belief that he had received a message from God.

Yet his doubts returned again and again, and reached a distressing height so that he was tempted to cast himself down from Mount Hira and this conflict lasted for two or three years.

Then one day he came to Khadijah in a state of great excitement exclaiming "Wrap me up, wrap me up!" (which was done when he fell into a fit, or swoon) and then the angel Gabriel appeared a second time, and revealed to him the Sura beginning "O thou enwrapped one!" [4] Henceforth there was no interruption and no doubt, the revelations followed without a break, and the Prophet was assured of his vocation.

The revolt of the idolatrous arabs against a creed of pure monotheism caused Mahomet at one time to yield to the temptation to humour them, and this temptation took the form of voice from the evil one, causing him to say in the pulpit that two of the heathen goddesses were sublime beings whose "intercession might be hoped for." His auditors were surprised and delighted, but the prophet went home disquieted. In the evening Gabriel came to him and Mahomet repeated the new *Sura*, whereupon the angel said "What hast thou done? Thou hast spoken in the ears of the people words I never gave thee." Mahomet now fell into deep distress fearing to be cast out from the Lord. But the Lord took him back to his grace and raised him up; the *Sura* of diabolical suggestion was erased, and the fury of the idolatrous party broke out with fresh violence. Mahomet was long and salutarily humbled by the remembrance of his temptation and fall, but he never abandoned faith

[3] It is still a subject of dispute amongst Moslems whether Mahomet could read it seems, however, more probable that he could not.

[4] Sura, 75 of the Koran.

in his vocation, and through evil report and good report believed that he had a Divine message to deliver.

The earliest and simplest accounts of the life of Buddha [Siddhartha Gautama] all agree in describing the four visions which led to the renunication, by that religious teacher, of all the greatest goods the heart of man could desire. Some accounts make all four visions appear on the same day, others on different days, but all agree is making the four visions phantoms which were visible only to Buddha and his charioteer Channa. As Buddha was driving in his pleasure grounds he was struck by the sight of a man utterly broken down with age; on another occasion by the sight of a man suffering from a loathsome disease, and some time afterwards by the horrible sight of a decomposing corpse. Then an ascetic appeared walking in a calm and dignified manner, and the charioteer explained to the young prince the character and aims of the ascetics.

[5]" Subjectively though not objectively," says Mr. Rhys Davids, " these visions may be supposed to have appeared to Gautama," and undoubtedly at this time the mind of the young Rajput had become deeply stirred. The birth of his son did not deter Gautama from his resolution to lead an ascetic life, so that he might some days return to his loved ones not only as husband and father but as teacher and saviour; and on the night of the full moon in the month of July the young chief left his father's home, his wealth and power, his wife and child behind him, and with Channa as his sole companion, went out into the wilderness to become a penniless and despised student and a homeless wanderer. It would take too long here to attempt to explain the reasons which made the visions of Buddha naturally relate to the sorrows and emptiness of life, and not to the joys and promises of a future state. The great object to be attained was to put an end for ever to the cycle of births and deaths to which all human beings were considered subject, and to pass this life in such a manner that complete absorption into the World-Soul (Nirvana) should follow death.[6] The aim was *not* that conscious personal immortality, or that rejoicing in the love of a God who loves his creatures, which is the strong desire of the heart of Western peoples.

But in order to teach (what to him seemed) the way of salvation to his fellow creatures, Gautama made the greatest and most complete self sacrifice ever recorded of any human being; and for this great renunciation his memory cannot be too highly honored.

[5] T. W. Rhys Davids, Buddhism. Enc. Bri., Vol. IV.

[6] The Nirvana of Buddhism is simply extinction, op. cit., p. 434, and note 1.

Augustine, the great Bishop of Hippo may be taken as a type of the '*mens sana in corpore sano.*' He was a man who drank deeply of all joys, both of the body and the mind, which the cup of life could offer. Yet his great powers and commanding intellect did not prevent his hearing a "divine voice" which then and there influenced him to take up the religious life. It is true that the mind of Augustine had been deeply exercised by the search for truth, which ever seemed to elude his grasp. Plato and St. Paul opened the way for higher thoughts, and words of the latter were driven home with irresistible force to his conscience, as with his friend Alypias he was studying the Pauline epistles. The thought of divine purity fought in his heart, with the love of the world and of the flesh which were sore temptations to a man so admirably fitted to enjoy both. He burst into a flood of tears, and going out into the garden flung himself under a fig tree that he might give his tears full vent, and pour out his heart to God. Suddenly he heard a voice calling him to consult the scriptures. "Take up and read, take up and read." He left off weeping, rose up and returning to his house took up the volume from Alypius, and read in silence the words to be found in Romans XIII, 13th and 14th verses. Augustine adds "I had neither desire nor need to read further. As I finished the sentence the light of peace had poured into my heart and all the shadows of doubt dispersed. Thus hast Thou converted me to Thee . . . . standing fast in that rule of faith which Thou so many years before had revealed to my mother." (Confessions, VIII, 30). This appears to have been the only occasion when a hallucinatory (?) voice was heard by Augustine, but its influence lasted for his whole life.

For that experience which points to the state commonly known as ecstacy,[7] I shall take the experience, not of a saint, nor of a prophet, but of a plain American citizen of our own day, a locomotive engineer who worked chiefly in Ohio and Indiana.

[7] Neoplatonism was a philosophical religion. in no way founded on any revelation real or imagined. Its great expounder Plotinus says simply of his own experience of "ecstacy" [that is of the sense of absorption into the Divine Beings] "I myself have experienced it but three times." But his pupil and disciple Porphyry says that on four occasions during the six years of their intercourse Plotinus attained to the ecstatic union with God.

It is surely contrary to the true scientific spirit to ignore this strong, overmastering instinct of the human mind towards union with some "Power, not itself, which makes for righteousness," and which appears in equal strength in the Hindoo, the Mahometan, the mediæval saints, the German mystics; and in the Neoplatonists—who had no "religious superstition" to influence them, and no hell to terrify them, but who were possessed only with the sense of an overwhelming need for union with the Divine.

This engineer, whose name was Skilton, was engaged with two other men in unloading a freight car. Just as he had lifted a barrel out to the platform he saw a "person standing" "on his right hand clothed in white" "and with a bright countenance"; and "putting his hand on my shoulder." "He said 'Follow me'." A long description ensues of the happy and beautiful place to which this personage led him; a place where the inhabitants did not converse by sound but knew each other's thoughts on the instant, and where he saw his mother, his sisters and his child. In fact the heaven it was natural for the percipient to to imagine, though—be it remarked not the heaven of harps and white nightgowns of popular theology—but such a heaven as the seer might naturally desire. Mr. Skilton describes the earth on his return as appearing to be seen from a great height, trees buildings, etc., gradually came into sight, and finally he reached the car which he had begun to unload, and then the guide vanished. "I spoke then" he says (just as I opened my watch and found it had been just twenty-six minutes that I had been engaged with that mysterious one) and said I thought I had left this world for good. One of the men said: "There is something the matter with you ever since you opened the car door; we havn't been able to get a word out of you" and that I had done all the work of taking out everything, and putting it back into the car. I told them where I had been and what I had seen, but they had seen no one. He adds: "This I count the brightest day of my life, and what I saw is worth a life time of hardship and toil. Being in good health, and busy about my work and my mind not more than ordinarily engaged on the great subject of eternal life, I consider this a most extraordinary incident."[8]

If it be said there is no corroboration of Mr. Skilton's statements neither is there any corroboration of any other similar statement; and if I have chosen his experience from among many others, it is simply to show that whatever religions (?) experiences occurred to persons in past times, *occur in precisely the same way now.*[9]

As an exemplification of the law that all spiritual (?) experiences are coloured by the prepossessions of the percipients, I may take the numerous cases of the alleged appearances of the Virgin Mary. The apparition to Bernadette Soubirons is only one amongst many, and in this case as in most others expectant attention is not a factor. The Virgin appeared to Bernadette when she was certainly not expecting to

[8] Communicated to Professor James of Harvard by Mr. Skilton.

[9] The exaggerations found in *legendary* times constitute a totally different branch of study.

see anything; and the crowds of pilgrims who have gone to Lourdes in a state of vivid expectant attention have seen nothing.

I have no space to describe the many instances I have met with of recent appearance of the Virgin Mary, under circumstances where there can be no reasonable doubt of the good faith of the percipients; nor do I recall any circumstances of religions exaltation accompanying these visions. Where pious Catholics see the Virgin Mary or the saints, equally devout Protestants see Christ or an angel, and both see spirits which they believe to be those of the blessed dead.

My own mind in regard to the question of religions experiences is in the state of suspense of judgment,—the state of "pure agnosticism" advocated by G. J. Romanes. [I need hardly say that this is *not* the agnosticism of scientific orthodoxy].

If in the present stage of our study of these phenomena we leave the stage of pure agnosticism, one of two hypotheses must, it seems to me, be accepted. Either such experiences are due (*in persons otherwise rational and capable of carrying out the business of life*) to some "organ of spiritual experience," or they are *all* in the domain of morbid psychology. The evidence is of a similar nature in all these cases, whether of Paul, or Mahomet, or Ansel Bourne, or Bunyan or Socrates or Mr. Skilton; the hallucination (?) visual or auditory or both is only manifest to the percipient, and to him appears objective. In the case of what are called "collective hallucinations"—which are not uncommon, we have also only the evidence of the percipients, who see what others do not see. The great difficulty in accepting the hypothesis that there is some real communication possible from "a Power not ourselves which makes for righteousness," is the frequent association of religions hallucinations with insanity. But if there be an "organ of spiritual insight" it can act only subject to the general functions of the organism, and must be liable to partake of its disorders; it can only be the frail machinery through which an Unknown Energy is acting.

So I leave the question, the most important, it seems to me, with which Science can deal. The history of Ansel Bourne is extracted from the Proceedings of the Society for Psychical Research.

ALICE BODINGTON.

ANTHROPOLOGY.[1]

Mr. Keane on Paleolithic Man.—Mr. A. H. Keane in his recent publication "Ethnology" takes serious exception to my denial of there having been a paleolithic period, and says "Paleolithic necessarily antedates Neolithic Culture." In what does this necessity consist? Why should Paleolithic precede Neolithic Culture? The names it is true signify old and new, but are at best arbitrary, being a suggestion of Sir John Lubbock to distinguish a supposed "chipped" from a "polished" stone period. Mr. Keane says "where there is a time sequence, the chipped stones being of ruder and simpler formation, naturally precede the more perfected polished objects." The proof of a "time sequence" is by no means a settled question, this assertion being negatived in one way or another by every writer on Archeology. The chipped stones are not "ruder" than polished stones, nor are they "simpler" in shape, material or facility with which the shape may be given. I have only attempted to discuss the subject from a technical standpoint and from the writings on the subject generally, from either of these points, however, or from both together I contend my position is sustained. Chipping stone is a more difficult mechanical process than grinding and pecking stone, it is more complicated in its minutiæ, involving, it is true, blows with a hammer, the difference being that the chipper's blow is of necessity more deliberate, slower and of necessity more accurate than the blow given in pecking. A doubt of the accuracy of this proposition may be solved by taking a flint and a diorite and attempting with any hammer to shape them. If ordinarily careful the diorite will be worn into shape, while on the other hand the chances are many to one that the flint is destroyed before completion.

Mr. Keane objects that "European archeologists are asked to reconsider their own conclusions." Undue weight being shown to have been given certain evidence, European archeologists owe it to themselves to reconsider their conclusions. Up to a recent period it was believed generally that to shape a Neolith or ground implement was more difficult than it was to shape a Paleolith or chipped implement, and such difficulty was used as the main evidence upon which to support the theory of a chipped preceding a polished stone age. Having been shown that the contrary was the case, one would presume

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

European archeologists would seek to examine their error without being forced to do so. Mr. Keane says "There is necessarily a time sequence wherever the two cultures have been developed." The mistake the author falls into is in declaring that to chip a stone or to grind and peck a stone constitute two cultures, for experiment proves the contrary; try to chip a diorite and you only shape it by powdering the surface, for it does not chip, try to batter a flint and it breaks through the ordinary lines of cleavage and is destroyed, for it does not peck.

Again the author of Ethnology says "that until it is shown that fire arms are as old as paleoliths, no European archeologist will ever believe that polished implements are as old as the chipped stones." Though this will hardly pass as scientific argument we would say, here again Mr. Keane is in error, for instances of chipped and polished stones being found in the same layer with fossil bones has been recorded on too many occasions to leave room for doubt to one who would decide on written authority alone. Up to this time Mr. Keane has argued in favor of the simpler process preceding the complex, but here he says "it is a fallacy to suppose that the easier process comes first," and instances "transport by wheeled vehicles or by steam as immeasurably easier than pack animals." If we are to construe the word "easier" as being synonomous with "simpler" it opens a new field of argument to assert that the complex precedes the simple in machinery, or that the machine of many parts is the ancestor of the machine of few parts, this proposition will not meet with many supporters.

My views concerning the mechanical status of primitive peoples has been formed solely from experiment with primitive tools and reading the literature of archeology. The technology of archeology appears to be little understood in Europe, its importance being almost ignored, as is evidenced by the apparent inability to grasp the plainest mechanical propositions. The results of my experiments are sustained by my field discoveries as they are by research in the library, and considered from any of these positions the fact that an identical mechanical culture produced, chipped or polished stone appears to be indisputable. The earliest cave remains show man to have made the best use of material at his command. Where he had flint he chipped it; if on the contrary he lived in a region of metomorphic stone he would of necessity hammer it into shape; if horn and ivory were plentiful he would saw and grind it. If one will make experiments with primitive implements in reproducing primitive work they will not fail to appreciate the correctness of the views here expressed. As illustration take

material from Magdalen Cave, from Les Eyzies, from St. Acheul, from Moustier, from Chelles, even from Cissbury or from where one will, and try to make from it arrow heads of a different type from that usually found in the locality from which the material is brought and examine the result. One finds that flint chips within certain limits, for it depends even more on the material than on the workman as to the shapes the nodules work into. The difference in the tool with which the stone is worked is secondary to the texture of the stone. One of the best illustrations of this is in the obsidian spear heads from Easter Island. They are of a gritty texture, extremely rude, fully as rude as the rudest paleolith, and are chipped almost entirely from one side. Try to improve the shape of one of these implements and rude as they are, failure is the inevitable result; try to chip it from the wrong side and it breaks through and destroys the specimen, the best and most expert workman cannot improve its shape. Take, however, one of the obsidians from Mexico of even texture and to shape it in most graceful form is most easy, but with such material it would be almost impossible to imitate the rough arrow heads of Easter Island. The same stone varies enormously in its fracture in different layers, yet archeologists do not appear to have noticed the fact.

Only a few years since it was argued that paleolithic man was primitive man, man low in the scale of human development, to-day paleolithic man is apparently only primitive as a flint chipper, but an artist as a bone or ivory worker; the fact that technically considered the work necessary to shape a so-called "Baton of Command," itself probably a chipping tool, was identical with the chief work on the neolith (grinding) does not appear to be appreciated by those who believe in a paleolithic period. Five years ago or little more it was hotly denied that pottery belonged to the paleolithic period, and this was insisted upon until the accumulation of proof was so overwhelming that by many it was admitted, yet even now many deny that pottery is as ancient as many of the paleolithic cave strata.

J. D. McGuire.

Cave Exploration by the University of Pennsylvania in Tennessee.—*Preliminary Report.*—To learn that the remains of Plistocene Man have been abundantly found in the caves of Europe; that equally significant remains of later savage, barbarous and civilized peoples have been similarly discovered in the caves of Europe, Asia and Africa; and that the remains of the Indian and the recent White Man have been found in caverns in North

America; warrants the supposition, that in the subterranean floor deposits of the new world, the problematic existence of Plistocene Man might be soonest and easiest demonstrated, while with hardly less ground we may urge as valuable testimony in the American region the absence of such remains in significant underground shelters. Not unreasonably such absence, occurring invariably at these immemorial halting places of men and animals, might indicate that Plistocene Man had never existed in the adjacent regions.

By this course of reasoning and investigation the University of Pennsylvania has sought to solve definitely the question first to attract and last to puzzle American students—How long has Man existed in the New World? Striving to limit the speculations of archæologists, the work has proceeded by degrees to reconcile with geology their study of pre-Columbian peoples, which, fascinating as it is, has lacked thus far subdivisions, landmarks and starting point, while an effort to eliminate, through the investigation of significant caves, one region after another from the field of search, has sought to narrow the area of possible discovery from the point of view explained. Having shown on the one hand that certain caverns like the fissure at Port Kennedy, (right bank of Schuylkill River, 3 miles below mouth of Perkiomen Creek, Montgomery County, Penna.,) containing in large quantity the remains of Plistocene animals without relics of Man, are geologically ancient, on the other hand, a fact of much significance has been demonstrated for the first time, namely, that a considerable number of other caves are modern, since their floors, well supplied with the refuse of Indians and later White Men, below which remains of geologically older peoples would not have been lacking in Europe, have failed to reveal any relic of Plistocene Man.

In these several instances the geologically modern remains (human) and the geologically ancient remains (animal) have lain apart in distinct caves, and hence less availably for comparative study, but the recent expedition in Tennessee, resulting in the examination of three caves in which the old and new deposits lay in juxtaposition, has enabled us to push the question farthur by studying the relation between the ancient and modern strata where, at their point of contact, it was most significant.

More broken and scattered even than at the remarkable tomb of extinct animals at Port Kennedy, were the remains of the Tapir, Peccary, Bear and smaller Mammalia at Zirkel's Cave, (left bank of Dumpling Creek, about 5 miles above its mouth in French Broad

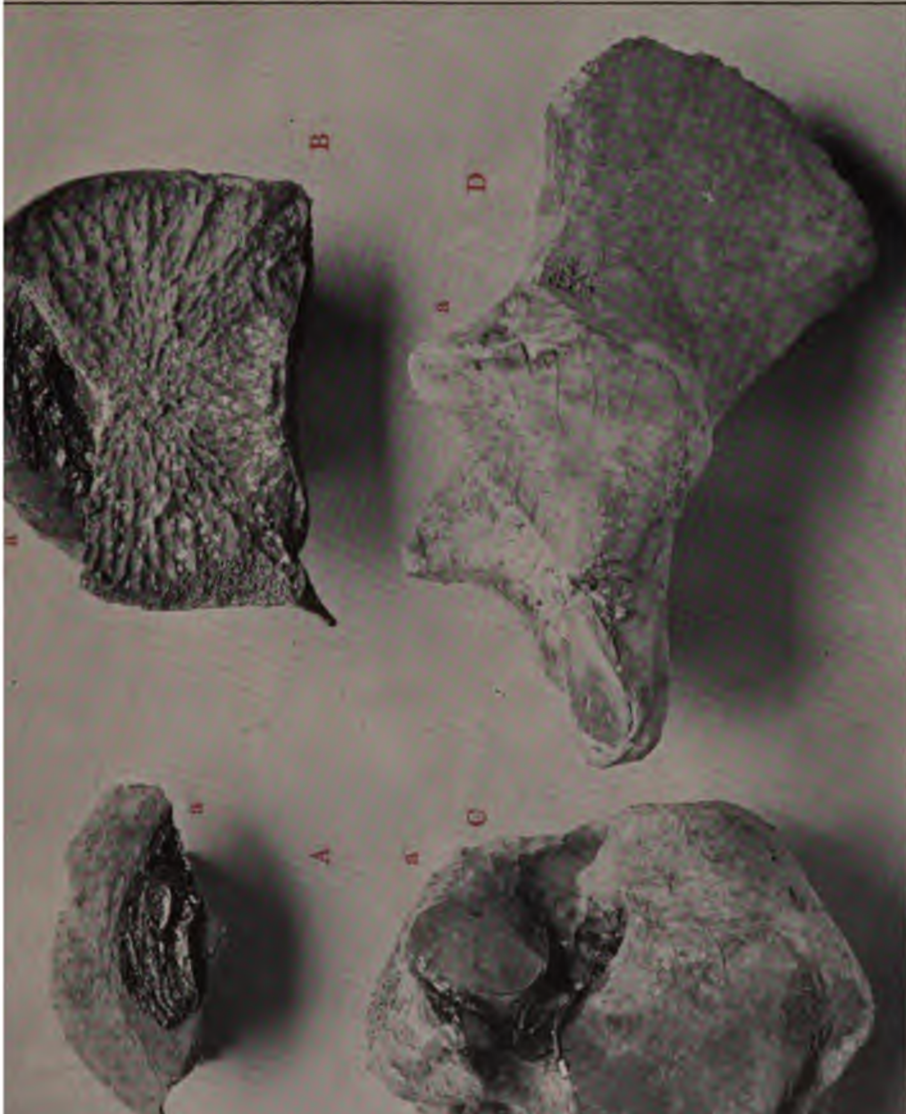
River, Jefferson County, Tennessee,) visited by Professor Cope in 1869. Dislocated as before after the flesh had rotted, the bones were crushed by a force which had split them into fragments, and were deposited with a mass of clay mixed with lime, which filled the descending cave. Hardened finally into breccia not easily broken with the pickaxe, this bone bearing earth had disappeared at many points to make room for a deposit of cave earth containing the remains of the Rattlesnake, Woodchuck, Opossum, Rabbit and Cave Rat. It is the important relation of this latter modern earth, with its bits of mica and Indian pottery, to the older breccia that will constitute the material for a final report.

Previous examination, in 1893, at the Lookout Cave, (left bank of the Tennessee River, one-quarter of a mile below Chattanooga Creek, Hamilton County, Tennessee,) had revealed the bones of the Tapir and Mylodon in the lowermost zone of a floor deposit of Indian refuse, and upon the recent expedition the cave earth with its "culture layer" was entirely removed for 58 feet inward from the entrance to settle beyond doubt the relation of these fossils to the Indian remains resting upon them. At this significant spot, where again the Plistocene and recent deposits lay in contact, and where the specimens found were labeled according to their position, whether from the black (modern) earth above or the yellow (ancient) earth below, a completed examination should decide whether Man had or had not encountered the Tapir and Mylodon in the Valley of the Tennessee.

After a visit to "Indian Cave" on the Holston River, Carrol's Cave, and the Copperas and Bone Caves, near Tullahoma and Manchester, Tennessee, a new set of conditions was presented at Big Bone Cave (1 mile from the left bank of Caney Fork and about 2 miles above its mouth in Rocky River, Van Buren County, Tennessee.) There the bones of the Gigantic Fossil Sloth (*Megalonyx*,) still retaining their cartilages, were exhumed from a dry deposit of the refuse of Porcupines and Cave Rats, mingled with fragments of reeds used as torches by Indians in a gallery 900 feet from the entrance, thus presenting us in the final summing up of this strange evidence a new notion of the relation of the modern Indian to this extinct animal, whose remains outnumber all its fossil contemporaries at Port Kennedy.

Thanks are due to Dr. William Pepper, to the Board of Managers and to Professor E. D. Cope for their kind co-operation in the expedition thus finished, which, at a cost of $300, has presented the Museum with the specimens now under examination. These, if not attractive,

PLATE XI.



Megalonyx jeffersonii with articular cartilages at a. A, B, Vertebral epiphyses. C, Astragalus. D, Calcaneum.

are important. For Paleontology they mark in the bone breccia of Zirkel's Cave, a distinct stage in the Plistocene series, while for Anthropology they represent data which account for the presence of Man together with the bones of the extinct *Megalonyx*. They explain the relics of savages and the remains of Plistocene mammals at two caves situated in the Eastern Valley of Tennessee at a height of about 600 to 700 feet above the sea and within earlier reach of an overwhelming ocean in Champlain time, and again at a third cave, which, 300 feet higher on the continental floor and looking westward from the slopes of the Cumberland table-land, stands for that part of the Appalachian region whither animals and Man (if he existed) might have found convenient refuge when lower areas sunk, as is alleged, beneath the level of the invading waters.—HENRY C. MERCER.

Aldie, June 4, 1896.

SCIENTIFIC NEWS.

The proposed general synopsis of the Animal Kingdom (Das Thierreich) to be issued by the German Zoological Society, is one of the greatest undertakings ever planned in the line of bookmaking. It is proposed to give a short general account of each group, and following this is a synopsis of all existing forms, including those which have recently become extinct. The general editor of the whole series is Prof. Franz Eilhard Schulze of the University of Berlin, and he is assisted by the following department editors: Prof. O. Bütschli, Protozoa; Prof. C. Chun, Coelenterata; Prof. M. Braun, Plathelminthes; Prof. J. W. Spengel, Vermes; Dr. W. Kobelt, Mollusca; Dr. W. Giesbrecht, Crustacea; Prof. R. Latzel, Myriapoda; Prof. F. Dahl, Arachnida; Dr. H. Krauss, Orthoptera; Mr. A. Handlirsch, Neuroptera, Hemiptera; Dr. H. J. Kolbe, Coleoptera; Prof. C. W. Della Torre, Hymenoptera; Dr. A. Seitz, Lepidoptera; Prof. J. Mik, Diptera; Prof. F. Blochmann, Brachiopoda; Prof. E. Ehlers, Polyzoa; Prof. J. W. Spengel, Tunicata; Dr. G. Pfeffer, Fishes; Dr. O. Boettger, Batrachia and Reptila; Prof. A. Reichenow, Birds, and Prof. L. Döderlein, Mammals. These will be assisted by a host of collaborators for special groups, and the names of these, as far as announced, assures us of the

most authoritative treatment. The whole work will be of enormous extent, and it is estimated that it will take twenty-five years for its completion. It will be issued in parts of an average size of 48 pages, and the price charged to subscribers for the whole series will be 70 pf. (17½ cents) for each "signature" of 16 pages; single subjects will be sold at an advance of ½ above this price. It is estimated that the Flatworms will occupy 4 parts; the Crustacea 11, the Hymenoptera 13; the Mollusca 15, the Reptiles 3, and the Birds 16. The total will be over 120,000 pages, and the series, when complete, in large octavos, will occupy not far from 30 feet of shelf room. The publishers of the series are the well-known R. Friedländer & Sohn, of Berlin, and they have already issued a sample number, embracing the group of Heliozoa, treated by Dr. F. Schaudim, of Berlin. This occupies 24 pages. It is expected to begin regular publication with the year 1897, and the parts will be issued as fast as possible, without regard to their sequence in the whole work.

It will probably be interesting to know that a party of five from Cornell, under the direction of R. S. Tarr, will accompany Lieut. Peary on the trip to Greenland this summer. The party will be so constituted that the results will cover the several fields of natural history, although the main object will be the study of geology, and especially glaciation.

The next meeting of the American Microscopical Society, August 18–20th next, is to be held in the Carnegie Library Building, Pittsburg. The local Committee on Arrangements is organized: C. C. Mellor, Chairman; Mayness Pflaum, Secretary and Treasurer, and C. G. Neilnor, Chairman Finance Committee, either of whom will be glad to give members and others desiring to attend all necessary information. As soon as sufficient arrangements are made, special announcements will be mailed to all members.

Dr. R. Wagner, of Strassburg, has been appointed assistant in Vegetable Physiology in the University of Munich, and Dr. A. Y. Grevillius, of Upsala, assistant in Botany at Minster.

The Royal Belgian Academy of Sciences has recently elected Professors E. Strasburger, E. D. Cope, E. J. Marey and Sir A. Geikie to honorary membership.

Prof. M. Treub, Director of the Botanical Gardens, at Buitenzorg, who has been spending some time in Europe, has returned to Java.

Dr. A. Fleischmann has been advanced to the position of Extraordinary Professor of Zoology in the University of Erlangen.

The Royal Irish Academy recently elected Sir Joseph Lister, T. G. Bonney and Sir W. H. Flower to honorary membership.

Dr. G. A. J. A. Ondermann, Professor of Botany in the University of Amsterdam, has retired on account of his great age.

Mr. A. Lawson, botanist and director of the Cinchona plantations in the Madras district, died at Madras, February 14th.

Prof. D. Barfurth, of Dorpat, goes to the University of Rostock as Ordinary Professor of Comparative Anatomy.

Dr. H. Ph. Foullon von Norbeck has been appointed Chief Geologist to the Austrian Geological Survey.

Dr. G. Rörig, of Berlin, goes to the University of Königsberg as Extraordinary Professor of Zoology.

Mr. H. M. Drummond-Hay, ornithologist and ichthyologist, died recently at Perth, Scotland, aged 82.

Dr. J. Briquet, of Genoa, has been appointed Director of the Delessert Herbarium in that city.

Mr. J. H. Ashworth succeeds Dr. Hurst as Lecturer on Zoology in Owens College, Manchester.

Mr. A. S. Olliff, entomologist, died in Sydney, N. S. W., December 29, 1895, aged 30 years.

W. L. Sclater, of Eton College, goes to Cape Town as Curator of the South African Museum.

Dr. Thilenius has been made privat docent in Anatomy in the University at Strassburg.

Dr. C. Herbert Hurst, of Manchester, goes to Dublin as assistant to Prof. A. C. Haddon.

Dr. A. Smirnow, of Kazan, goes to the University of Tomsk as Professor of Histology.

Col. Plunkett has been elected Director of the Science and Art Museum in Dublin.

Dr. A. Bogdanof, Professor of Zoology in the University of Moscow, died in April.

Dr. G. Fatta has been appointed assistant in the Botanical Institute at Palermo.

Dr. R. Oestreich is now privat docent in Anatomy in the University of Berlin.

Dr. Szymonowicz is privat docent in Histology in the University of Cracow.

Dr. Gruner, of Jena, goes on an expedition to Togo, German West Africa.

Prof. Ph. C. Sappey, the well known anatomist, died in Paris, March 13th.

A. Duvivier, student of Coleoptera, died in Brussels, Jan. 14, 1896.

F. Ludy, coleopterologist, died March 1st.

PLATE XII.

Sumerian Tablets from Nippur. From Hilprecht.

THE
AMERICAN NATURALIST

Vol. XXX. August, 1896. 356.

THE OLDEST CIVILIZED MEN.

By E. D. Cope.

Recent explorations in Babylonia have given us much information as to the characters and customs of the oldest civilized people of whom we have any knowledge. The earlier explorations were conducted by M. de Sarzec, French consul at Bagdad, and the report of his work was issued in a magnificent folio in joint authorship with the distinguished anthropologist of Paris, M. Leon Heuzey, beginning in 1889. A little later the department of Archeology and Paleontology of the University of Pennsylvania sent out Messrs. J. P. Peters and J. H. Haynes to make excavations further south in the Euphrates plain. They selected Nippur or Nufar as the point of research, and work has been continued there from 1888 to the present year. The climate of this place is very trying, and the character of the people dangerous, but Mr. Haynes on whom much of the actual labor fell, obtained an amount of material which in quantity and quality equals that obtained by the museums of London and Paris.

The Philadelphia material has been investigated by Dr. Herman Hilprecht, Professor of Assyrian and comparative Semitic Philology in the University of Pennsylvania, and he

has published two memoirs of great interest in the transactions of the American Philosophical Society, the second of which was issued in the present year. From this memoir and the previous one of De Sarzec and Heuzey I compile a few facts regarding the physical characters and habits of the earliest inhabitants of Chaldea, the Sumerians or Accadians. The information on these points is obtained largely from statues and picture-carvings on tablets of a dark limestone, found by De Sarzec at Tel-lo, and by Haynes at Nippur. The figures of animals of known species are so characteristic as to prove that the artists possessed a true eye for form. We may infer that their delineations of man are equally accurate, and that the conspicuous characters which they exhibit are trustworthy delineations. The general resemblance between the features depicted show that we have to do with an interesting and peculiar race.

In the numbers of the NATURALIST for January and February, 1893, Mrs. A. Bodington gave our readers a sketch of the Sumerian question. She followed the belief which had gained currency at one time, that these people were of Mongolian origin. Others have suggested that they were African. The drawings and statues described by Heuzey and Hilprecht show that these ideas were quite unfounded. I reproduce one of the latter from Hilprecht (Plate XVI, l. c.), which is known as the stele of Ur-Inlil. Ur-Inlil was the high priest (or padesi) of Nippur, and he is represented as making an offering to some god on the upper half of the drawing. On the lower half a goat and a sheep are followed by two men, one of whom carries a vessel on his head, the other carries a stick (Plate XII). Another tablet from Nippur displays the same kind of men, and they are also represented on eleven tablets figured by De Sarzec and Heuzey from Tel-lo.

That these represent a race advanced in civilization is clear. They built temples and palaces on huge plateaus constructed of brick. They carved statues and vessels and made pottery. Especially they left records of their history on numerous cylinders and tablets of clay of which many have been preserved. They formed organized armies armed with spears, bows, and shields. What relation did these people bear to the people

of Nineveh whose monuments were revealed to Europe by the labors of Rawlinson, thirty-five years ago? Heuzey declares them to have been older than the Assyrians, and this position is proven by Hilprecht, who believes their earliest king whose name is preserved in the records of Nippur, Enshagsagana, to have lived 4500 B. C. Many kings intervened between him and Sargon I with whom Assyrian history for a long time commenced. These people were predecessors of the Assyrians of Nineveh, and gave them their cuneiform characters, but they differed from them in customs, and to some extent in language. One marked difference of custom, was the fashion of shaving the hair from all parts of the head excepting the eyebrows. Everyone knows on the contrary that the Ninevites took great pride in their hair, and that both on the calvarium and face it was curled and arranged with great care. The figures also show that the Sumerians did not practice circumcision as most Semitic and some other races have done.

The shaving enables as to get a pretty good idea of the form of the head and face. The skull is oval, rather long and flat, and probably mesaticephalic. The jaws both upper and lower, are remarkably small, giving an extreme orthognathic type The nose is remarkably long, prominent and curved, with a good bridge. The eyes are large, horizontal, and not bridled. The cheek-bones are not large, and in the supposed gods, where the hair remains, and in a few other unshaved portraits, the beard is abundant, and the ends of the hair of the calvarium curled up. The figure of the body is robust, broad and rather short. The extensor muscles, i. e. gluteus, quadriceps, and gastrocnemius are well developed.

From the above it is evident that no thought of Mongolian (=turanian) or Ethiopian relationship can be admitted. After a study of some of the least characteristic heads broken from statues, M. Heuzey remarks, that "the evidence is not sufficient to demonstrate the existence of Turanians in Chaldea." These people are clearly of the great Indoeuropean subspecies of man (*Homo sapiens caucasicus*), so that the question reduces itself to one of the determination of their race position. Are they Aryans, or Semitics? using these two terms as covering all the forms of the greater subspecies to which they belong. In the

determination of these minor divisions of man, physical characters begin to fail us. We can only say that if the term Aryan is used for the western peoples generally, the Sumerians differ from them in the direction of the Semitics by their large oval eyes and hooked noses. On the other hand, the small and delicate jaws are not features of Semitic peoples. But the people of Persia or Iranians, hold very much this intermediate position between the two peoples. We scarcely know the shape of the jaws and chins of the Ninevites for they are never shaved. So far as the visible features go they resemble the Sumerians. It is on all grounds to be supposed that the people of Nippur and Tel-lo are the primitive Aryans of the Iranian or Persian race, and ancestors of the Ninevites.

In any case it is evident that we have in these most ancient of civilized people, a type of man as high as any that has since appeared from the point of view of physical evolution. The extreme orthognathism; the prominence of the nose; the reduction of the cheek bones, the full beard; and the well developed extensor muscles of the leg, prove this. *Homo sapiens caucasicus* had reached his full characters on the plains of the Euphrates 6400 years ago.

The relation of time and race of the oldest civilizations to the prehistoric peoples, is a problem which will doubtless be solved in time. Did the Neolithic people exist in Europe contemporaneously with the Sumerians of Chaldea? The only light that can be thrown on the question is as follows. The Sumerians were not stone people, but bronze people. They had no knowledge of iron. No search has been made for the remains of animals which were their contemporaries, but several species are clearly represented on their sculptures. The most common are the lion and the ox (*Bos taurus*, not the buffalo). There is a good drawing of a gazelle in the collection of the University of Pennsylvania. The goat and sheep represented on the accompanying Plate XII, are species now existing in Persia. The goat is near the *Capra ægagrus* of the mountains of East Persia, the ancestor of the domestic goat; and the sheep is apparently the wild sheep of the same region *Ovis vignei*. So from a paleontological point of view, the Sumerians were quite modern.

ON THE ROLE OF ACID IN THE DIGESTION OF CERTAIN RHIZOPODS.

BY JOHN C. HEMMETER, M. D.,[1]

In the "Annales de l'Institut Pasteur," for 1890 and 1891, there are two papers by M. Felix le Dantec on "Researches on the intracellular digestion among the Protozoa," which are detailed accounts of systematic experimentation concerning the occurrence of acid in the digestive vacuoles of Protozoa.

In 1889, E. Metchnikoff published a discussion of the reaction of plasmodia to ingested litmus, also in the "Annales de l'Inst. Pasteur."

Miss M. Greenwood and E. R. Saunders, in the "Journal of Physiology," Vol. XVI, 5 and 6, 1894, have published an exhaustive account of the function of acid in Protozoan digestion, of which the following brief abstract is considered necessary before proceeding to the original part of this report.

It was found that while these protozoa ingest solid matter constantly and promiscuously, such matter has a determinate fate. If it is innutritious it is ejected after lying in contact with the animal's substance for a length of time which varies with many changing conditions. Nutritious matter, on the other hand, during enclosure in food vacuoles undergoes profound change, and this change is effected by something passed out of the protoplasm into the vacuole, acting in a fluid medium and by its presence making that medium deserving of the name "secretion." In Actinospaerium, also, and in *Amoeba proteus*, digestion in like manner is effected, not by direct contact with the acting protoplasm but by some constituent of a fluid, the formation of which the presence of food alone is potent to bring about. These protozoa depend upon the solution of proteid for nourishment. Starch undergoes no digestive change, and the value of ingested fat globules is doubtful.

The following is a report on the role of acid in these digestive vacuoles. For method of observation, it may be briefly

[1] Phil. D. Etc. Baltimore, Md.

stated, that plasmodia and Vorticellidæ were watched for periods which varied from one to fifteen days; large plasmodia were isolated or preserved in concave slides. Even on plane slides the pressure of the cover slip was slight enough to allow of the emission of short pseudopodia in planes at right angles to the plane of extension of the slide and the animals, by means of pipettes, were transferred to fresh water daily.

In a synopsis of the work of Greenwood and Saunders, in a previous report bearing on this matter, in the Journ. of Physiol., the changes undergone by litmus, Congo red and alizarin sulphate, and the solution of the globoids of aleurone grains, which are composed of a delicate nitrogenous capsule enclosing pure calcium and magnesium phosphate, were described. It was emphasized that the outpouring of acid is unaccompanied by any digestive change on nutritive matter; ingesta may indeed be stored for many hours in vacuoles before they are dissolved, or digestion may follow rapidly on ingestion. But the formation of the digestive vacuole, whether immediate or delayed, is preceded by the development of acid reaction and followed by its diminution. Bearing in mind that litmus is changed from blue to red not only by free acid, but also by unsaturated compounds of acid with the products of digestion, *i. e.*, acid salts. And that Congo red changes to blue in presence of free acid only. It is apparent that the diminution of acid in a digestive vacuole is at first due to a combination with the products of digestion, for at this stage any litmus accompanying ingesta is still red, while Congo red has reverted to that tint from blue. Here free acid is absent but acid salts are present. But later on the vacuoles and ingesta, reddened by litmus, become violet and blue so that finally acid and acid combinations are alike absent. That the acid is at one time free is indicated unmistakably by the striking development of violet colors in solids stained with Congo red. Now as the amount of acid present at any moment must be very small, and this being so, that the change in Congo red should be speedy and striking suggests that it is an inorganic acid but it is probable that to emphasize such an inference would be hasty.

In most of the existing records of Protozoan digestion there are indications that the process shows irregularity in its outset and progress. It is not easy to foretell the immediate fate of ingested matter though of its ultimate fate there may be little doubt. There may be marked inhibition of digestive activity even after free ingestion. In plasmodia ingested nutrient matter may be actually discharged after very imperfect digestion. One of the most puzzling phenomena, however, that has been described by all observers in this field, has been termed by Greenwood and Saunders the stage of storage. This process consists in the preservation of ingested food masses, which on first enclosure have been surrounded by liquid within a vacuole, in a shrunken seemingly very acid state. At times 100 non vacuolate, acid ingesta may lie within the substance of a vorticella, whilst active digestive solution is going on in other food vacuoles at same time.

The storage of nutritious ingesta for hours and days in a condition in which acid indicators give evidence of an acid condition, whilst the same kind of nutritious material will undergo rapid digestive solution in an adjacent vacuole, naturally excites one's curiosity. For a long time I had been looking in vain for some explanation of this phenomenon when an accident gave opportunity of viewing it in a new light.

The plasmodia of a large mycetozoon, most probably *Lamproderma scintillans*, had been under observation for about three weeks. Some of these amoeboid organisms were so large as to more than cover the field of vision when objective D and apochromatic eye-piece, No. 4 of Zeiss were used. They showed a habit of devouring everything in their vicinity in the ditch water in which they were cultivated, as a result of which they were at times so filled with debris that no accurate observations were possible. It was planned to transfer them gradually by pipettes into clearer and clearer water and by starvation compel them to rid themselves of the dirt they contained. This proved successful and after 8–18 days of transferring the plasmodia were in practically clear water, free from algæ, infusoriæ, gregarines, bacteria, etc., and the usual

fauna and flora of ditch water. It was a surprise to find that dried egg albumen stained or ingested with litmus and Congo red, under these new conditions, was as a rule promptly dissolved in the vacuoles, taking from 5–24 hours for completion of the digestive act. The same occurred with stained globoids of aleurone grains of ricinus and with stained torulæ. These experiments were repeated many times on many different individuals, and though food ingesta were occasionally observed in a stage of storage, this was the great exception.

The scarcity of storage vacuoles in such plasmodia that had been kept in clear water for nearly a week and given opportunity to disgorge the debris with which they were loaded was conjectured might be brought about by two factors:

(1) The first was that the process of clearing and transferring them to distilled water (in which they do not thrive as well as in Pasteur's fluid with ½ % Na cl) the organisms had been starved and in a sense were too hungry to store food particles, but went to work at them immediately. There is no method conceivable by which such a supposition could be put to experimental test, for which reason it cannot be contradicted or proved.

The second supposition was that (2) absence of storage vacuoles might be caused by absence of bacteria, for in their normal environment the Protozoa are generally in close company with swarms of *Bacterium termo*, zooglea of micrococci and manifold spirilli and other schizomycetes, and by cultivation they had been brought into an almost aseptic, sterile environment.

The latter hypothesis is capable of experimental testing. For if bacteria will produce the phenomenon of storage then the supplying of septic food will be all that is requisite to add to the sterile solution. As a matter of fact it will be found that this is exactly what will happen. In a plasmodium that had shown 8 storage vacuoles in 24 hours of observation in a solution of ½ % sodium chlorde (in distilled water) in which it had been kept one week, 48 storage vacuoles were observed in the next 10 hours on supplying dried albumen dust, moistened with the zoogloea from a Hay infusion.

Vorticellidæ which take in food particles readily are remarkably free from bacteria in their food vacuoles. Amoeba and plasmodia alike exercise to some extent a selective ingestion. Greenwood and Saunders claim to have watched *Amoeba proteus* for 14 days when surrounded with *Bact. termo*, vibrios and micrococci and the absence of bacteria from the endosarc was remarkable. They are taken in, it would seem, as unavoidable accompaniments of surrounding food only. Bacteria are not recorded to have been observed ingested by protozoa per se.

Another evidence of selective ingestion has been mentioned by Dantec, *l. c.*, as distinguishing between inert and living matter. Active monads or groups of spirilli are placed in marked vacuoles of ingestion, containing much of the acid secretion in comparison to inert matter which is usually invested very closely. We therefore have some evidence for assuming that plasmodia and Vorticellidæ distinguish between inert food and bacteria.

(1) Bacteria are rarely ingested except as unavoidable accompaniment of food. (2) Inert food, free from bacteria, is invested closely. Septic food within wide vacuoles. (3) In sterile environment, food in the stage of storage is the exception; in environment of bacteria, storage in acid vacuoles is frequent. I have brought these facts before you in this incomplete form, because the results are fairly uniform, and with the hope of stimulating further observation of the matter. These studies require no apparatus outside of the microscope and acid indicators. The general suggestion drawn from the result has a wider bearing than one would at first sight assume. For if further study will confirm that the ingestion of bacteria constantly prolongs the stage of maximum acidity from the usual time of 24 hours to several days in rhizopods. The suggestion is that the purpose of the acid is one of (disinfection) killing off bacteria.

There is a general uniformity of opinion that the presence of acid is unaccompanied by any digestive change on nutritive matter, which may be stored for many hours before it is dissolved and Greenwood and Saunders intimate that the endo-

sarc secretes some zymogen which perfects the digestive secretion.

The object to which the acid would seemingly serve in these organisms, which may be said to be on the very threshhold of life is the same which Bunge ascribes to it in man. Bunge's view is that the HCl has no other purpose than the sterilization of food. " Why should a chemical substance be placed in the entrance to the digestive tract," he asks, " in exactly the strength necessary for the destruction of bacteria which is directly antagonistic to the chemical reaction in which the main work of digestion must be carried on? The proteids are more readily converted into a solution lower down in the intestine and in an alkaline medium than by pepsin and acid. The object of the acid is, according to him, then, one of sterilization. This view cannot be denied, at the same time it must be admitted that HCl serves also a digestive purpose.

In the Rhizopods experimented upon, the observations of Greenwood and Saunders could be confirmed concerning the fact that while the acid is secreted in the food vacuoles under the stimulus of all ingesta; the true digestive vacuole which occurs only under the stimulus of nutritive matter apparently contains something besides an acid, perhaps an enzyme. The change in the acid indicators is as regards time and intensity of color transformation to all observation alike. There seems to be the same amount of acid in a storage vacuole as in a vacuole causing active solution of proteid matter, in close proximity to it, hence the assumption of an additional zymogenic substance in the latter is justifiable. As the amount of acid in one of these vacuoles is very small, and the change in Congo red to blue is speedy and striking, lends belief to the suggestion of Greenwood that the acid is an inorganic one. Why the protoplasm around a storage vacuole will not secrete zymogenic matter, though acid is clearly present in it, and at the same time this enzyme must be accepted to be present in a vacuole in which, close to the former, active digestion is going on is a question difficult to approach. If it can be demonstrated that all or most storage vacuoles contain some substance, living or inert, which is hostile to the economy of the Rhizopod and against which it protects itself by intensely acid

investment of the enemy for a prolonged period, a new and interesting light will be thrown on this phenomenon.

In the "Centralblatt für Bacteriologie, Parasitenkunde u. Infektions krankheiten, Vol. XIX, p. 785, Dr. C. Gorini describes a method for cultivating *Amoeba zymophila* on a solid medium which in this case is the potato. It is certain that Amoebae will grow on old and new potatoes with alkalinization. This would offer an easy and convenient method of cultivating them. It should be emphasized that it is almost impossible to produce cultures of amoeba that are absolutely free from bacteria. A. Celli in the Centralbl. f. Bacteriologie, Bd. XIX, p. 537, describes a number of futile attempts to obtain such cultures. For our purpose it is not essential that the amoebic cultures should be absolutely free from bacteria, a relative, approximate sterility is sufficient to demonstrate the scarcity of storage vacuoles in the amoebae and plasmodia in such environment. Celli's favorite solid medium is a preparation made from Fucus Crispus with 5 per cent Sterilized Water, with or without Bouillon, but always made alkaline. To 10 c.c. culture medium, 1 c. c. of an $\frac{N}{10}$ Solution of Potassium hydroxide or 4–5 c. c. of a saturated solution of Sodium Bicarbonate. This culture medium of Fucus after it is made in the manner that Agar is generally prepared solidifies readily.

In the same Journal, Centrbl. für Bacteriologie, Band XIX, p. 258, Dr. M. W. Beyerinck describes a solid medium for amoebic cultures made from solidified agar by diffusion of the soluble organic substances in it into superimposed distilled water, which process requires about two weeks and repeated sterilization and subsequent addition of salts suitable to formation of nitrites.

I have no experience with these methods and have always found that for my purpose a solution of a little wheat bread in distilled water kept in a small flat dish under a glass cover was all that was required to have Amoeba and plasmodia of mycetozoa constantly on hand. The dish must be kept on a little earth and not in too bright a light and at a constant temperature. This simple culture medium, which of course is unsuitable for pure cultures was suggested by Prof. Reichert of the University of Pennsylvania.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

BY ERWIN F. SMITH.

I.

It is scarcely fourteen years since Dr. Robert Hartig declared that there were no diseases of plants due to bacteria.[1] Two years later Dr. Anton de Bary, unquestionably one of the most learned and critical botanists the world has ever known and the foremost student of cryptogamic plants, expressed the belief that bacterial diseases of plants were of rare occurrence, and suggested as a partial explanation the fact that the tissues of plants generally have an acid reaction.[2] In his *Vorlesungen über Bacterien*, published in 1885, he expresses much the same opinion,[3] and cites only four diseases, viz., Wakker's hyacinth disease, Burrill's pear blight, Prillieux's rose red disease of wheat grains, and the wet rot of potatoes, described by Reinke and Berthold. Concerning the first of these four diseases he says: "Successful infection experiments and exact study of the life history of the bacterium are still wanting." Respecting the second he contents himself with briefly summarizing the statements made by Prof. Burrill. Of Prillieux's micrococcus he says: "Its importance as a cause of disease cannot be determined with any certainty from the brief account. It

[1] "Für die Krankheitsprocesse der Pflanzen kommen sie durchaus nicht in Frage, etc." Hartig: (*1*) *Lehrbuch der Baumkrankheiten*, 1882, p. 27.

[2] "Bacteria parasitic on plants have scarcely ever been observed, a fact to which R. Hartig has already drawn attention. One reason for this may be that the parts of plants have usually an acid reaction." De Bary: (*2*) *Vergleichende Morphologie und Biologie der Pilze Mycetozoen und Bacterien*, 1884, p. 520; English ed., p. 481.

[3] "According to the present state of our knowledge parasitic bacteria are of but little importance as the contagia of plant diseases. Most of the contagia of the numerous infectious diseases of plants belong to other animal and plant groups, principally, as already noted, to the true fungi." De Bary: (*3*) *Vorlesungen ueber Bacterien*, 1885, p. 136.

may turn out to be only secondary, appearing as a saprophyte in consequence of injuries previously received." Concerning the wet rot of potatoes he states that ordinarily it is a secondary phenomenon following the attacks of the parasitic fungus *Phytophthora infestans*, but admits that exceptionally potato tubers may become wet rotten without the presence of *Phytophthora*, and that "the above named observers succeeded in producing the appearance of wet rot in sound potato tubers by inoculations with their bacteria; in agreement with which stands a recent experiment of van Tieghem, who succeeded in totally destroying living potato tubers by means of *Bacillus amylobacter* when he introduced this into the interior of the tuber and maintained the same at a high temperature (35°)."

In the second edition of his *Lehrbuch*, published in 1889, Dr. Hartig modified his statements somewhat, expressing essentially the same opinions as de Bary. The yellow rot of hyacinths is recognized as a bacterial disease, although rather doubtfully in as much as it is said not to attack sound, well-ripened bulbs, under normal conditions, but only when they have received wounds or been attacked by fungi, especially by a hyphomycete which is said to be an almost constant accompaniment of the rot. The wet rot of potato tubers is admitted to the list, but with the statement that it is mostly a secondary matter, following the rot of stem and cells due to *Phytophthora infestans*. One other bacterial disease is mentioned, viz., pear and apple blight, with the suggestion, however, that it may have been erroneously attributed to bacteria, since the fungus *Nectria ditissima* produces in the bark numerous little bacteria-like gonidia.

Such was the general opinion on this subject down to within less than a decade. Even to-day the majority of well educated botanists would find nothing to contradict in the statement that there are very few diseases of plants distinctly attributable to bacteria. As a matter of fact, however, there are in all probability as many bacterial diseases of plants as of animals.

Various explanations have been advanced to account for this freedom or supposed freedom of plants from bacterial parasitism. As we have already seen, de Bary was inclined to ascribe

it in good part to the acid reaction of vegetable tissue. Dr. Hartig's view is best expressed in his own words:[4] "Whereas the processes of decay, and most of the infectious diseases of man and animals, may be traced to bacteria, the plant organism is protected against them by the peculiarity of its structure, and especially by the absence of circulatory channels for conducting the nutrient fluids which could serve to distribute any lowly organisms which might happen to be present in the food. It is only by means of the vessels and intercellular spaces that they can distribute themselves in any great numbers in the body of the plant, for in other cases they have to pass through the cellulose or woody cell walls, which offer great resistance to their attack. In addition to this, the vegetable juices, most of which show an acid reaction, are unfavorable to their growth. As a matter of fact, bacteria have hitherto been found only in the tissues of plants whose cells are parenchymatous in character and possessed of very delicate walls, as for instance, bulbs and tubers."

For several years Ph. van Tieghem experimented with one or more, probably several, bacteria, called by him *Bacillus Amylobacter* and believed to be the specific agent in the decomposition of cellulose. In 1879,[5] he stated that all the cells of all plants are equally dissolved by it in the meristematic stage but that as soon as the tissues have become differentiated profound differences are noticeable. The cellulose of many plants is dissolved by it but that of mosses, sphagnums, hepatics, lycopods, fern leaves, and stems and leaves of phanerogamous aquatics proved resistant. This behaviour of water plants is "une nécessité d'existence." In 1884,[6] he made a number of additional similar statements. The tubers of the potato, the seeds of beans (first swelled in water and then inoculated directly into the substance of the cotyledons), and the fruits of cucumbers and melons rotted quickly when infected with this organism. Inoculated leaves of Crassulaceæ and stems of Cac-

[4] Hartig: *Lehrbuch.* 2nd. Edition. English translation, p. 37.

[5] Van Tieghem: (*4*) Sur la Fermentation de la Cellulose. *Bull. de la Soc. Bot. de France*, 1879, pp. 25 to 30.

[6] Van Tieghem: (*5*) Développement de l'Amylobacter dans les plantes à l'état de vie normale. *Ibid.*, 1884, pp. 283–287.

taceæ resisted until plunged under oil when they decayed quickly. Aquatics resisted: "By means of a Pravaz syringe I have injected a drop full of the spores of Amylobacter into the lacunary system of several submerged aquatics (Vallisneria, Helodea, Ceratophyllum) but always without result. The plant remained healthy in all its parts."

These papers of van Tieghem are often cited, but they have little substantial value. Undoubtedly he believed that he was experimenting with pure cultures, or, at least, that the results obtained were due to *Bacillus amylobacter*, but such is, to say the least, very improbable. *B. amylobacter* is now believed to be strictly anaerobic, and incapable of any action on cellulose.[7]

More recently Julius Wiesner has divided all plants into two classes, ombrophobic and ombrophylic plants, according as they are or are not readily injured by prolonged rains or exposure to stagnant fluids.[8] His experiments show that the aerial parts of some plants rotted very quickly when exposed to continuous artificial spray while similar parts of other plants proved very resistant, remaining sound for weeks (62 days in case of *Tradescantia guianensis*). The same contrast was observed when leaves of the two sorts of plants were placed in stagnant water, the former lost their turgor and rotted in a few days, the latter proved much more resistant. Many land plants have this power of resistance and all water plants, also all underground parts, even the roots of plants having very susceptible foliage. As additional confirmation Wiesner states that when meat infusions are left to themselves they always decay much sooner than when fragments of ombrophylic plants are placed therein. Ombrophobic plants in water or meat infusion also decay less rapidly when mixed with fragments of ombrophylic plants than when left to themselves. This decay is more rapid in the dark than in light, especially

[7] Prazmowski: (*6*) *Untersuchungen ueber die Entwickelungsgeschichte und Fermentwirkung einiger Bacterien-Arten.* Leipzig, 1880, pp. 23–37.

[8] Wiesner: (*7*) Ueber ombrophile und ombrophobe Pflanzenorgane, *Sitzungsb. K. Ak. d. Wissenschaften*, Math.—Naturw. Classe. Wien., 1893, Bd. 102. Abt. I, pp. 503–521. See also Wiesner: (*8*) Pflanzenphysiologische Mittheilung aus Buitenzorg (III). Ueber den vorherrschend ombrophilen charakter des Laubes der Tropengewächse. *Ibid.*, 1894, Bd., 103, pp. 169–191.

Wiesner's hypothesis is somewhat different. It has been known for some time that various essential oils and other vegetable products, e. g. thymol, salicylic acid, benzoic acid, tannin, quinine, oil of cinnamon, oil of peppermint, etc., exert a powerful restraining influence on the growth of many bacteria; and it is not improbable that a great variety of bactericidal and protective substances occur in plants. On the other hand there may be and probably are bacterial parasites capable of thriving in the very plants which Wiesner found most resistent to continuous spray, to the saprophytic bacteria of stagnant water, and to those of decaying meat infusions, the exact conditions under which any given microorganism will thrive being determinable only by experiment. It must also be remembered that the physiological requirements of bacteria often become profoundly modified to suit changed environments, and that all parasites have undoubtedly descended from saprophytic forms. Prof. Wiesner has, however, opened up a very inviting field and its further investigation by some careful experimenter, trained in bacteriological methods, might lead to very interesting discoveries.

Most of the recent books on vegetable pathology devote a chapter to the bacterial diseases of plants, but these books have not been written by bacteriologists and consequently the statements given are usually very meager and unsatisfactory, and forcibly illustrate the fact that no one can write acceptably on a subject with which he is not familiar, not even if he possesses a logical mind and has read all the "authorities." Excepting Prof. W. Migula, who reviewed the subject briefly but somewhat carefully in 1892,[10] and Dr. H. L. Russell, who gave a brief summary in tabular form the same year at the end of his Thesis,[11] no one seems to have gone over the field critically since de Bary's time, although there is now a considerable body of literature. It is proposed, therefore, in the following pages to examine the literature of this subject from the standpoint of the

[10] Migula: (*10*) *Kritische Uebersicht derjenigen Planzenkrankheiten, welche angeblich durch Baktrien verursacht werden.* Semarang. Midden-Java. 1892. Exp. Sta.

[11] Russel: l. c., pp. 36–41.

modern bacteriologist, sifting as far as possible the wheat from the chaff, and arranging all in an orderly way for convenient reference. The utility of such a piece of work, if well done, can scarcely be questioned, since it must set into sharp relief the gaps in our knowledge and tend to stimulate further research.

The work of the early investigators already mentioned was done before the perfection of modern methods of bacteriological research, and in a time of general scepticism which some of us well remember. It is therefore in no way discreditable that many of their conclusions should be found untenable when tested by the more rigid requirements of the science of to-day. They worked under great difficulties and did as well as could be expected even of men of genius, better, indeed, than many of us would have done. Certainly, as pioneers in a difficult field they deserve great credit.

As much cannot be said for some of the more recent workers who with every opportunity in the way of literature, including numerous manuals of bacteriology, and with laboratory facilities for learning the fundamentals of bacteriological research on every hand in every land, have been content to publish second and third class work, exactly like that preceeding the discoveries of Pasteur and Koch and the development of modern methods. One might suppose these people to have been in a deep sleep for the last twenty years, they take so little note of what has been going on. I shall have frequent occasion to consider papers of this class in the course of these pages and shall not fail to point out their worthlessness, to discourage imitators, if for no other reason. It goes without saying that such publications do not advance science, nor in the end in any way contribute to the reputation of the individual. They are thoroughly discreditable, and in case new species are erected, are little less than criminal, considering the present overburdened and chaotic state of systematic bacteriology.

Thanks to the itch for species making, systematic mycology is generally cited as the most desperately confused and perplexing branch of natural science, but mycology is a highway turnpiked and provided with arc lights in comparison with the wilderness of systematic bacteriology. Of the thousand or

more forms which have been studied and named, or designated by letters or figures or vernacular names,[12] probably not one-tenth can be identified with any certainty owing to the meagerness of the descriptions. The older descriptions are particularly bad, and the effort to decide what was meant by these old names, for which somebody will by and by be strenuously claiming inalienable rights of priority, is usually time thrown away. There is quite enough to do in bacteriology, as every one knows who is dealing at first hand with its hard problems, without wasting precious energy in striving to guess what was meant by two and three line descriptions. All descriptions which do not *describe*, and there are many such, ought to be wholly ignored, and no species recognized as worthy of a place in literature unless so characterized as to be identifiable by others. A plea of this sort in the higher branches of botany or zoology would be a subject for laughter. Bad descriptions are however, so much the rule in bacteriology that it is no laughing matter but rather a great evil urgently demanding reform. It is a state of affairs which has come about naturally enough considering the way in which bacteriology has developed[13] but which would not now be tolerated for a moment in phanerogamic botany or in most branches of zoology and the continuance of which in bacteriology it is incumbent on every honest worker to limit and discourage in all possible ways. The best way in science, always, is to speak out plainly, and to join hands for the advancement of a good cause. Bad work should be ignored and "new species" relegated to limbo unless the descriptions conform to the requirements of modern bacteriological science, meaning by this expression the consensus of opinion among experienced and careful investigators everywhere. If there were some such agreement among the better class of workers, the improvement in systematic bacteriology would become very marked. The volume of publication would, indeed, decrease noticeably but this of itself

[12] About 650 species are mentioned in (*11*) Schizomycetaceæ, by de Toni and Trevisan in Saccardo's, *Sylloge Fungorum*, VIII. published in 1889, but this is not complete.

[13] All the early systematists built upon a foundation of sand, i. e. upon pure morphology.

would be a great advantage, and the quality of the work would more than correspondingly improve. Only in some such way can the strong tendency toward trashy publication be eliminated and light and order evolved from the present chaos.

With few exceptions, vegetable pathology seems to have been specially unfortunate in the class of persons who have devoted themselves to the study of bacterial diseases. While the bacterial side of animal pathology has had its Pasteur and Koch, its Esmarch, Hueppe, Flügge, Gaffky, Frænkel, Pfeiffer, Lœffler, Duclaux, Metchnikoff, Chamberland, Roux, Welch, Sternberg, Smith, Prudden, and a host of other skilled experimenters, scarcely less eminent, and has made correspondingly great progress, the study of the bacterial diseases of plants has been principally in the hands of botanists without special laboratory training in bacteriology and even destitute in some cases of an elementary knowledge of right methods of work. The great development of modern bacteriology is attributable largely to the discovery that human diseases are due to these organisms, and to its consequent alliance with medicine, but there is no reason why the same rigid scrutiny of methods and sharp calling in question of statements which have led to such brilliant results in animal pathology in recent years should not be applied in the same way to vegetable pathology. Accurate experimentation and trustworthy results are from a purely scientific standpoint quite as desirable in one field as in the other.

Two things are especially to be kept in mind in describing any bacterial disease of plants: (1) The pathogenesis must be worked out conclusively; (2) If the organism is named, it must be so described that it can be identified by any competent bacteriologist no matter where it is found.

The four requirements under the first head, i. e. *Pathogenesis*, are now generally recognized to be as follows:

A. Constant association of the germ with the disease.

B. Isolation of the germ from the diseased tissues and study of the same in pure cultures on various media.

C. Production of the characteristic symptoms of the disease by inoculations from pure cultures.

D. Discovery of germs in the inoculated, diseased tissues, re-isolation of the same, and growth on various media until it is determined beyond doubt that they are identical with the organism which was inoculated.[14]

Not one of these steps can be omitted. Possible sources of error beset the investigator at every step, and anything short of a rigid demonstration cannot be accepted as proof. A. is usually quite easy, involving only the careful microscopic examination of abundant material, stained and unstained. B. was made possible by the improvement of methods, *i. e.* by the use of solid media, and especially by the discovery of the method of isolation by means of plate cultures. C. is quite easy, provided the right organism has been obtained and this be inserted into the proper tissues under the right conditions to insure growth. The fulfillment, however, of these conditions often involves long and vexatious delays, and taxes the acumen of the investigator to the utmost. D. serves as a check on all the preceding work, showing that there has been no unintentional mixing of organisms, and that the results obtained were actually due to the supposed cause. For the sake of brevity these four rules of practice will be referred to in the following pages simply as A. B. C. and D. What weight shall be given any specific statement depends of course on the reputation of the individual. Some men are so careful of their reputation and so little given to making unwarranted statements that their simple word goes a long way even when the statements themselves seem improbable, whereas the elaborate explanations of other men, if the asserted facts are at all out of the ordinary, have to be taken with a grain of salt.

The requirements under the second head, i. e. *Description of the organism*, are more numerous, and are embraced under two general divisions of very unequal value, namely *Morphology* and *Biology*. In the classification of the higher plants and animals morphology has been accepted from time immemorial

[14] A series of successful reinoculations is always very desirable and becomes indispensable in case the infections are obtained on plants grown in a locality where the disease prevails naturally. Of course, numerous un-inoculated plants, known as "checks" or "controls," must always be reserved for comparison.

as answering all the requirements of systematists, but such is far from being the case when it comes to the description of bacteria. These minute organisms, which are among the lowest and simplest forms of living things yet discovered by man, are, within the commonly accepted generic limits, so morphologically similar as very often to be indistinguishable with any certainty even under the highest powers of the microscope. As supplemental, therefore, to morphology, and even in many cases as a complete substitute for it, we must have recourse to *Biology*, viz. to the behaviour of the living organism under a variety of known, artificially prepared conditions, such for example as the peculiarity of its growth on various culture media, its thermal death point, its ability to ferment various sugars, the chemical products of its growth, its pathogenic power, etc. Morphologically identical organisms often differ so widely and constantly in their biological peculiarities that there can be no question about their being distinct species, or as to the real value of this means of classification. Probably it also has value, hitherto overlooked, for the differentiation of higher plants and animals, especially for determining the limits of polymorphic or closely related species.

It is not my intention in this place to mention all the biological tests which should be applied to any species for its proper characterization. These are being added to constantly by an army of trained workers in all parts of the world, and my own views of what is at present necessary, or at least highly desirable, will be sufficiently evident in what is to follow. Very likely, also, as knowledge increases, some of the tests which are now generally held to be important will be shown to have little specific worth.

This, however, appears to be a good place to insist on accuracy in all the details of bacteriological work, especially in the preparation of culture media, and on explicitness of statement so that other investigators may know just what was done and how it was done, and thus be able to repeat the experiment. When all details of work are suppressed the inference, naturally enough, is that the writer was ignorant or else that he desired to conceal something not specially to his credit, and which if

plainly expressed might millitate against the value of his work. Either horn of the dilemma is equally bad. Some, however, who are desirous of doing good work in this field, or at least appear to be conscientious workers in other lines, do not seem to be aware of the necessity for extreme care in the preparation of culture media and the management of cultures. As a matter of fact, many bacteria are extremely sensitive to slight changes in the composition of the media in which they are grown or to other conditions within the control of the experimenter, and this appears to be true especially of the pathogenic species. Hence the many conflicting statements about the same organism. A few examples will render my meaning plainer. The careless exposure of cultures to bright sunshine may destroy the organism. An organism supposed to come from diseased tissues or from a culture, and which is being examined in a cover glass preparation, may have been derived actually from a contaminated staining fluid. The apparently simple matter of slightly unclean test tubes or flasks may lead to error, e. g. antiseptic influences may be at work, or preciptates may be thrown down and subsequently mistaken for bacterial growth. Some kinds of glass are unsuited to delicate bacteriological work, the culture fluids being contaminated by substances dissolved out of the walls of the beakers, tubes, and flasks. Tyros, of course, are liable to mistake almost anything for bacteria or to find them anywhere (See a long paper by Bernheim on (*12*) Die parasitären Bacterien der Cerealen, in *Münch. med. Wochenschrift*, 1888, pp. 743–745 and 767–770, and comments on the same by Buchner and Lehmann, *Ibid.*, 1888, p. 906, and 1889, p. 110). Boiling culture media, after it has been compounded, in open beakers or in loosely plugged test tubes or flasks may unwittingly lead to its concentration. The use at different times of different peptones, or grades of gelatine, of unlike per cents of gelatine or agar, of varying grades of acidity or alkalinity, of impure chemicals, of different concentrations of the nutrient media, and of different methods in its preparation all tend to render comparative studies impossible. A large source of error in the differentiation of species by means of sugar fermentation experiments has been the employment of bouillon

containing undetected muscle sugar. Even when preliminary tests are made with some gas-producing bacillus there is still an opportunity for error, provided the tests are carried on only for a day or two. No bouillon should be judged free from sugar and safe for use until in fermentation tubes it has been subjected for at least a week to the influence of *Bacillus cloacæ* or some other organism producing an abundance of gas from grape sugar. If at the end of this period no gas has developed, and the transfer of a loop of fluid from such a tube into another fermentation tube containing a dextrose-bouillon sets up an evolution of gas, then the first bouillon may be used with confidence. Again, if cane sugar is sterilized in an acid bouillon at least a part of it is *inverted*, i. e. changed into dextrose and fructose, and fermentation results obtained therefrom may be due to the presence of any one of three sugars. Bouillon should always be made distinctly alkaline before cane sugar is added. Many of the older fermentation experiments are worthless on account of neglect of such precautions, to say nothing of some recent ones. Again *Bacillus tracheiphilus* grows not at all or feebly on nutrient gelatine as ordinarily made, or in media which is acid beyond a determinable slight degree, and if only such media were used the erroneous conclusion might be reached that it could not be grown outside of the host plant, whereas it grows freely in artificial media, even on gelatine, when the right conditions are established. *Bacillus amylovorus* grows well in some gelatines and refuses to grow in others. Even comparatively slight changes in the acidity or alkalinity of the culture media often have a marked effect on the growth of certain organisms, while others, *e. g.*, *Bacillus cloacæ*, are able to grow in almost any medium. Many bacteria prefer alkaline media, and some are very sensitive to the presence of acids, while a variety of bacteria commonly met with in water will not develop at all if the medium is rendered strongly alkaline. Other organisms grow well in acid media.[14a]

[14a] For a striking illustration of the effect on the growth of water bacteria of comparatively slight charges in the reaction of gelatine, see a recent table by George W. Fuller, in a paper entitled: (*13*) On the proper reaction of nutrient media for bacterial cultivation.—*Journal of the American Public Health Association*, Concord, N. H., Oct., 1895, p. 393.

Even the slightly varying acidity of steamed slices from different potato tubers may exert a marked effect on the growth of certain sensitive organisms. On this account some bacteriologists have advised discarding the potato altogether. I have myself found the potato a very useful substratum for the growth of both fungi and bacteria. All comparative tests on potato ought, however, to be made on cylinders or slices cut from the same tuber, and in every case the reaction, acid, neutral, or alkaline, should be carefully recorded. The behavior of the organism on a variety of tubers should also be determined, before deciding that it is something new. It has been thought by some that the best nutrient substance for a parasite must be, unquestionably, the juices of the host plant but this does not follow since there are all grades of parasitism, and even if it did, there are several chances for error in its employment, e. g. the nutrient juices are usually sterilized by steam heat and this may cause a number of chemical changes resulting in a compound very different from the living plant and entirely unsatisfactory as a culture medium, as many have learned by experience. Again, for some particular reason, even the juices of the plant when sterilized at ordinary temperatures by filtration, may be less well adapted to the needs of the parasite than well made beef bouillon or ordinary nutrient agar. In general, the standard culture media of bacteriology should be tried first. Some bacteria can be cultivated only on special media or at special temperatures, or under unusual conditions. *Bacillus subtilis* will only grow in the presence of free oxygen; *Bacillus amylobacter*, *B. tetani*, and *B. carbonis* will only grow in the absence of oxygen. Winogradsky states that his nitrifying organism obtained from European soils will not grow in the ordinary culture media and thrives best in solutions of inorganic substances, and on silicate jelly. *Bacterium tuberculosis* can be cultivated only in bouillon and on blood serum and nutrient glycerine agar, and at temperatures above 30°C. *Bacterium influenzæ* also flourishes at blood heat and can only be grown, it is said, in the presence of red blood corpuscles or in media containing yolk of eggs; other organisms have thus far refused to be cultivated at any temperature or on any artificial medium, e. g. *Bacterium lepræ* and *B. syphilitis*. Some bacteria

are destroyed at temperatures at which careless workers frequently pour their agar plates, while others refuse to grow at ordinary temperatures or even at blood heat, grow best at 50°--60°C., and are not killed until the temperature exceeds 70° or even 75°C. Finally, a race of *Bacterium anthracis* incapable of producing spores has been developed by growing the organism in media containing phenol; another non-virulent race bearing swollen, terminal spores, "drumsticks," by growing the organism in compressed air; and still another race destitute of virulence by cultivating it at temperatures above 40°C. These are not exceptional cases, similar care being necessary in all directions if one would avoid erroneous conclusions.

Naturally, every successful experimenter will vary his culture media in all sorts of ways in order to learn as much as possible of the organism under consideration, but at the same time he will determine its behaviour on the standard media, and will keep a very careful record of all that he does. The bacteriologist should make it an invariable rule to repeat every experiment two or three times, at the very least, and generally after an interval of some months or years he should repeat all his experiments. Even then he will fall into errors enough. He certainly should proceed with as much care as the chemist, and in many directions the work passes naturally over into chemistry. If quantitative or volumetric analysis requires all sorts of precautions and excess of care to avoid errors, no less does this youngest of all the sciences.

A few words respecting the occurrence of bacteria in normal plant tissues will be in place before concluding these general remarks. It goes without saying that such minute and universally distributed bodies as bacteria are likely to be found at times almost anywhere, even in plant tissues which seem to be healthy, just as they may sometimes occur in the blood stream of healthy animals, but they are not normally present in the tissues of plants. All carefully conducted experiments have led to this conclusion. The reader who wishes fuller information may consult papers by Laurent,[14b] Buchner,[15] Lehmann,[16]

[14b] (*14*) Sur la pretendue origine bacterienne de la diastase. *Bull. de l'Acad. roy. de Belgique*, T. X., pp. 38–57.

[15] (*15*) Notiz betreffend die Frage des Vorkommens von Bacterien in normalen Pflanzengewebe. *Muench med. Wochenschrift.*, 1888, pp. 906-907.

[16] (*16*) Erklarung in Betreff der Arbeit von Herrn Dr. Hugo Bernheim, etc. *Ibid*, 1889, p. 110.

Fernbach[17] Vestea,[18] Kramer,[19] and Russell.[20] Even when purposely introduced into living tissues they refuse to grow or spread but little and finally die out,[21] unless they possess specific pathogenic power in which case the result is quite different.

The diseases which will be discussed in the following pages may be divided into four classes:

(1). Diseases of clearly established bacterial origin.

(2). Diseases which appear to be constantly associated with bacteria and which are probably due to some specific organism, but full proof of which has not been furnished.

(3). Diseases said to be more or less closely associated with the presence of bacteria and ascribed thereto, but in which little or no proof has been brought forward to establish the causal relation.

(4). Communicable diseases which have been ascribed to bacteria but associated with which no organism has been found and which are probably of non-bacterial nature.

On the whole it would perhaps be more logical to divide the following pages into four chapters in the way I have specified, but for practical reasons it has seemed better to discuss all of the diseases of a given plant in one place. I have, therefore, arranged the material by hosts, but will at the close try to summarize the whole subject in the manner above indicated.

It will certainly be some time, probably many years, before we have anything like a permanent scheme of classification for the bacteria. Our knowledge is still too incomplete. Meanwhile, we have to do the best we can with the present systems, all of

[17] (*17*) De l'absence des microbes dans les tissus vegetaux. *Annales de l'Inst. Pasteur*, 1888, pp. 567–570.

[18] (*18*) De l'absence des microbes dans les tissus. *Ibid.*, 1888, p. 670–671.

[19] (*19*) Bakteriologische Untersuchungen ueber die Nassfäule der Karfoffelknollen. *Osterreichisches landw. Centralb.* I, Heft 1, 1891.

[20] l. c.

[21] Lominsky: (*20*) On the parasitism when introduced into plants of some disease-causing microbes (Russian). Wratch., 1890. No. 6, pp. 133–135.

Russell: l. c.

Kornauth: (*21*) Ueber das Verhalten pathogener Bakterien in lebenden Pflanzengeweben. *Centrb. f. Bakt., Parasiten-Kunde, u. Infectionsk.* I Abt., Bd. XIX, No. 21, 8 Juni, 1896, pp. 801–805.

which are more or less arbitrary and unsatisfactory, and all of which are liable to be set aside at any time. I have here adopted Migula's system[22] which seems to me very convenient, and on the whole the most satisfactory of any that has yet appeared.

Before proceeding to the body of this review it only remains to say that every effort has been made to deal impartially with the material in hand, and to present the essential ideas of the writers as concisely and accurately as possible. To this end the original papers have been consulted in every instance, unless otherwise stated in the text. So much vexation over wrong references has been experienced in time past by the writer that he has himself been at special pains to give full and accurate citations. It is to be hoped, therefore, that the reader will have no difficulty in finding the original papers. An endeavor has also been made to bring the subject fully up to date but it is quite likely that some worthy papers may have been overlooked, owing to the many languages and the ever increasing number of places of publication.

THE MEANING AND STRUCTURE OF THE SO-CALLED "MUSHROOM BODIES" OF THE HEXAPOD BRAIN.

By F. C. Kenyon, Ph. D.[1]

In looking at a series of sections of the brain of a hexapod, especially of a hymenopteron, the most notable structures are two pairs, one to each side, of large cup-shaped bodies of "Punkt substanz," or, what in the light of our present knowledge of nerve structure is better denominated fibrillar substance. Each of these cups is filled to overflowing with cells having large nuclei and very little cytoplasm. From the under surface

[22] Migula: Schizophyta: (22) Schizomycetes. *Die Natuerlichen Pflanzenfamilien* (Engler u. Prantl). I Teil. 1 Abt. a, Lief. 129. 8vo. p. 44, Leipzig, 1896. This is the forerunner of a larger work soon to be published by Gustav Fischer, Jena.

[1] Clark University, Mass.

each of these cups or "Becher" there descends into the general fibrillar substance of the brain a column of fibrillar substance which unites with its fellow of the same side to send a large branch obliquely downwards to the median line of the brain and an equally large or larger branch straight forwards to the anterior cerebellar surface. (Fig. B.)

Long before our present methods of sectioning and staining had found general application in the study of animal structure, or as early as 1850, the French naturalist, Dujardin, discovered these bodies in transparent preparations *in toto* of the brains of certain Hymenoptera and Orthoptera. From their somewhat folded appearance he describes them as "lobes à convolutiones," and compared them with the convolutions of the human brain, and even thought them associated with hexapod intelligence. Fourteen years later, Leydig, using the same methods confirmed Dujardin's discovery in working with the brains of the ant, bee, and wasp, and described them as "gestielter Körper." In 1875 Rabl-Rückhard identified the bodies in *Gryllus italicus*, *Locusta viridissima*, and *Dycticus verrucornis*, and correctly described the form of the "cup" under the term "Rind Körper." The very next year ('76) Dietl's application of the section method to the subject confirmed and perfected previous descriptions, and, struck with the resemblance to mushrooms, he adopted the name of "Pilzhutförmiger Körper," a conception later used by Packard (mushroom bodies) and by Bellocici ('82) (corpo fungiformo).

As to the intellectual function of the bodies, not all of the early writers supported Dujardin's inference. They were supposed to be connected with sight; but Rabl-Rückhard showed that they are fully developed in a blind African ant (Typhlopone). Dietl was loth to acknowledge an intellectual function, even though he found the organs more highly developed in Hymenoptera than in Orthoptera. But Forel ('74) adhered to Dujardin's supposition, and showed that among Hymenoptera even of the same species the bodies are most prominent where one usually recognizes most intelligence, as in the worker bees and ants, while they are small in the females and the males. Brandt ('76) two years later in a note on the brain of Hymen-

optera makes the same observations as to the differences in the same species, while Berger ('78) considered the structures as "organs of projection of the first order."

The supposition of Dujardin obtained its best support so far as the older methods would avail in the comprehensive work of Flögel ('78) covering the whole group of hexapods. Here, one may see at a glance that the development of the structures largely coincides with the development of intelligence, as shown by the following abridgement of his table:

A. The four cups completely developed.	
1. Very highly developed,	*Vespa.*
2. Large with rim,	*Apis, Formica Pompilus, Ichneumonidæ.*
3. Without rim,	*Blatta.*
4. Very small,	*Cossus, Sphinx, Vanessa.*
B. Cups incomplete.	
5. Walls and cells so reduced as hardly to be recognized as cups,	*Tenthredo, Cynips.*
6. Reduced to two small heaps,	Many small butterflies.
7. Wall a broad plate,	*Forficula, Acridium,* etc.
8. Wall (fibrillar substance) absent.	
(*a*) Cells in 4 groups,	*Dycticus.*
(*b*) Cells in 2 groups, distinguishable by comparison with neighboring cells,	*Aeschna.*
(*c*) Not so distinguishable,	*Tabanus.*
C. Cups unrecognizable even as rudiments,	*Hemiptera.*

If such a superior neural function is indicated by the testimony and work of the earlier writers, it may well be asked whether recent neurological methods will bring out the structure of the hexapod brain as well as they have that of the other invertebrates and that of the vertebrates, and whether they will lend this view support. First, it may be noted that the physiological experiments of Binet ('94), which are those of

Faivre very much bettered, demonstrate that a hexapod may live for months without a brain, if the subæsophageal ganglion, or better, ventro-cerebron, is left intact, just as a vertebrate may live without its cerebrum. Faivre long ago showed that this ventro-cerebron is the seat of the power of co-ordination of the muscular movements of the body. Binet has shown that the brain is the seat of the power directing these movements. A debrainedhexapod will eat when food is placed beneath its palpi, but it cannot go to its food even though the latter be but a very small space removed from its course or position. Whether the insect would be able to do so if the mushroom bodies only were destroyed, and the antennal lobes, optic lobes, and the rest of the brain were left intact, is a question that yet remains to be answered. In Binet's experiments neither olfactory nor visual stimuli can be transformed into motor impulses. Were it possible for them to be so transformed, my studies to be noted in a moment cause me to think that Binet's results would be very materially altered.

Now, as to my studies. During the winter just past with no little patience I endeavored to apply the bichromate of silver method to a study of the brain and general nervous system of the common honey bee, the more detailed result of a portion of which will be published a little later. The endeavor was rewarded by a considerable degree of success, the main facts being determined, though there are many details left for future studies. Others have tried to employ the same general method, but owing to a lack of proper store of patience or to their setting about the task wrongly have failed. Among them must be counted Binet ('94), with whom, however, there seems to be a defect in the conception of both the Golgi and the Erlich methods. For he sets the former aside as inconstant, uses the latter, without, however, apparently obtaining any very good results. He complains that preparations by the Erlich method (and the Golgi method might be included) leave out many details, and never seems to think that a sufficient number of preparations will supply those details and thus allow the whole to be determined. This is the more unfortunate, since his dependence upon the old methods has led him to give detailed

importance to phenomena that are relatively unimportant, and has resulted in a somewhat misty conception of the structure of the hexapod ventral nervous system.

One of the very first things that an impregnation of bee brains with bichromate of silver enabled me to make out was the structure of the mushroom bodies with their cells. These cells stand out in sharp contrast to all other nerve cells known, though they recall to some extent the cells of Purkinje in the higher mammals. Each of the cells contained within the fibrillar cup seeds a nerve process into the later, where it breaks up into a profusely arborescent system of brahchlets, which often appear with fine, short, lateral processes, such as are characteristic of the dendrites of some mammalian nerve cells. Just before entering the fibrillar substance a fine branch is given off that travels along the inner surface of the cup along with others of the same nature, forming a small bundle to the stalk of the mushroom body, down which it continues until it reaches the origin of the anterior and the inner roots mentioned at the beginning of the paper. Here it branches, one branch continuing straight on to the end of the anterior root, while the other passes to the end of the inner root. Throughout its whole course the fiber and its two branches are very fine. Nearly the whole stalk and nearly the whole of each root is made up of these straight parallel fibers coming from the cells within the cup of the mushroom bodies. What other fibers there are enter these bodies from the side, and branch between the straight fibers very much as the dendrites of the cells of Purkinje branch among the parallel fine fibers from the cells of the granular layer in the mammalian cerebellum. These fibers are of the nature of association fibers.

From the olfactory or antennal lobe, from the optic ganglia there are tracts of fibers that finally enter the cups of the mushroom bodies as shown by Viallanes and by my studies with the Golgi method and also with a Formol-copper-hæmatoxylin method of staining. Besides these tracts the Golgi method has enabled me to make out another tract, unknown before, passing down the hinder side of the brain from the cups to the region above the œsophagus, where it bends forwards and comes in

contact with fibers from the ventral cord, which exists, although Binet was unable to discover any "growth of fibers connecting the cord with the brain."

The fibers entering the cups from the antennal lobe, the optic ganglia, and the ventral region, spread out and branch among the arborescent endings of the mushroom body cells.

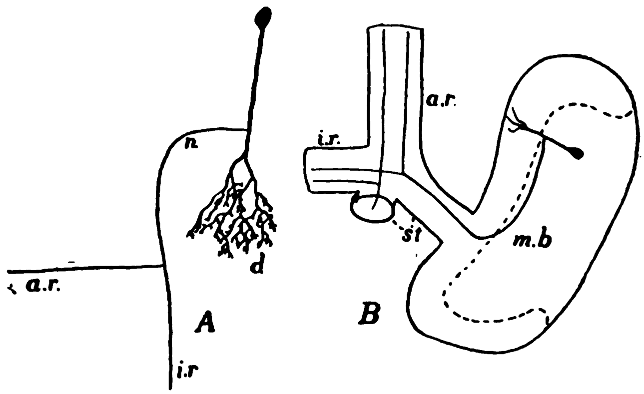


Fig. .—A. An "intellective" cell from the mushroom body. n, neurite; d. dendrite; a.r., anterior branch of the neurite; i.r., inner branch of the neurite.
B. Mushroom body of right side from above. The outer one, m.b, viewed in section; the inner one is cut off, leaving the stump of the stalk st. a.r., anterior root; i.r., inner root; m.b., cup.

The fibers branching among the parallel fibers of the roots and the stalk lead off to lower parts of the brain, connecting with efferent or motor fibers, or with secondary association fibers, that in their turn make such connections. This portion of the circuit has not been perfectly made out, though there seems to be sufficient data to warrant the assumption just made.

Such fibers existing as described there is then a complete circuit for sensory stimuli from the various parts of the body to the cells of the mushroom bodies. The dendritic or arborescent branches of these cells take them up and pass them on out along the parallel fibers or neurites in the roots of the mushroom bodies as motor or other efferent impulses.

This, however, is not all. For there are numerous fibers evident in my preparations, the full courses of which I have not been thus far able to determine, but which are so

situated as to warrant the inference that they may act as association fibers between the afferent fibers from the antennæ, optic ganglia, and ventral system and the afferent fibers. There is then a possibility of a stimulus entering the brain and passing out as a motor impulse without going into the circuit of the fibers of the mushroom bodies, or, in other words, a possibility of what may be compared to reflex action in higher animals.

It appears then that the supposition of Dujardin is well supported by the finer structure of the hexapod brain. For it is evident from the details known since the publication of Flögel's paper, that the cells composing the mushroom bodies have been very highly differentiated in some of the hexapods, and this in just those forms living the most complex lives. No such bodies are to be found in the lower forms. I have never seen them, nor any indication of them, in the Thysanura, Chilopoda,[2] Scolopendrella, the Pauropoda and other Myriapoda, nor in any of the Crustacea that I have thus far examined. Without doubt an application of the Golgi or methylen blue methods would reveal elements in some these forms that might be compared with the cells of the mushroom bodies; but they would probably be found not so completely differentiated from other fibers as they are in the honey bee and other Hymenoptera. It may be mentioned that one does not recognize such cells in the cray-fish and the crab as figured by Retzius and Bethe. And it scarcely need be said that no such elements are shown in Retzius' figure of the brain of *Nereis*.

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EDITOR'S TABLE.

The Zoological Section of the American Association for the Advancement of Science at its meeting in Springfield, Mass. in August, 1895, adopted a series of resolutions which are printed in the volume of the Proceedings recently issued (p. 159) and which are here reproduced. They were adopted with but one pertinent objection from a distinguished member of the section. This objection was that the method of determining priority of publication recommended in the resolutions was applicable to questions of nomenclature only, which was regarded as an object of a value secondary to the determination of date of discovery of matters of fact. While the fixing of date of the latter was admitted to be of great importance, it was contended by the friends of the resolutions, that the manner proposed by them was applicable to all possible cases, and that in fact the resolutions prescribed the best method of determination of priority. The mode proposed was stated to be in accord with that customary among authors and publishers generally, and that special groups of authors could not in practice sustain rules different from them. The resolutions are as follows.

Whereas: The date of publication is a question of fact to be determined by examination, and not by an arbitrary ruling: and

Whereas: In the world at large the date of publication of books is the date at which they are printed; and

Whereas: The adoption of any other date of publication would have no practical effect for this reason, and for the following additional reasons; viz.:

First; the majority of publications are not distributed, but are sold;

Second; the distribution when it occurs may be rendered ineffective by accidents such as loss of mails, fires, etc.;

Third; distribution by individuals may be delayed or prevented by absence from home, sickness or death;

Fourth; distribution by governments of their publications is often delayed for routine reasons;

Fifth; the actual date of mailing will be often impossible to ascertain with precision, owing to lack of record and irregularity in the period of transmission; and

Whereas: The determination of the date of printing will generally depend on the records of the printing office and the testimony of several persons, while the time of mailing will be known generally to but one person;

RESOLVED: *First.*—The section of Zoology of the American Association for the Advancement of Science recommends that the date of the completion of printing of a single issue be regarded as the date of publication;

Second.—That the Section recommends that such date be printed on the last signature of all publications, whether books periodicals or "separates."

RESOLVED: (1) That the Section of Zoology of the A. A. As. S. is impressed with the desirability of introducing the custom of placing all publications on record at some central agency together with the date of publication. (2) That a committee be appointed to obtain the approval of these resolutions by publishing societies at home and abroad. (3) That a copy of these resolutions be transmitted to the British Assoc. Adv. Science; the Zoölogical Society of London; Australasian Assoc. Adv. Science; Association Francaise; Société Zoologique de France; Versamml. der Deutscher Naturforscher, n. Aertzte; Zoologisches Gesselschaft; and the International Congress of Zoology held at Leyden.

To act as the committee above referred to, the President of the Section appointed: S. A. Forbes, Champaign, Ill.; E. A. Birge, Madison, Wis.; W. A. Lacy, Lake Forest, Ill.; George Dimmock, Canobie Lake, N. H.

The above resolutions were adopted by very large majority vote. A proposition to regard as the date of publication, the date of receipt at the central agency of record was introduced. This was not approved, as it was evident that no private arrangement made by naturalists could supersede the customs long since current in the world of authorship.

The American Association for the Advancement of Science has a peculiar custom which it seems to us might be improved. This is the use of the term vice-president to designate the presidents or chairmen of the respective sections. This expression gives use to confusion, as these officers are not the vice-presidents of the sections, but the presidents. If the expression vice-president of section so and so is used, a president is supposed, who does not exist. To avoid conflict with the title of the president of the Association, the term chairmen might perhaps be used for the so-called vice-presidents, but actual presidents of the sections.

The decimal system of record, called the Dewey system in library catalogues, appears to the management of the Naturalist to be the best method which has yet been devised. It, therefore, follows Natural Science and La Revue Scientifique in adopting it.

RECENT LITERATURE.

The Structure of Solpugids.—That indefatigable student of the Arachnida Mr. Henry M. Bernard has presented us with a valuable account[1] of the general structure of these little known forms. And yet while we can praise the statement of facts, as a whole, we would point out that the paper contains a number of theoretical points, which have, in our estimation, no sufficient basis.

The Galeodidæ, of which over 50 species have been described, are confined to the warm portions of both hemispheres, and though abundant in certain regions, they are comparatively rare in collections; possibly from the fact that they are, by popular consent, accorded most poisonous qualities. They, alone of all the Archnida, show a distinct "head" while they also have a "thorax" divided into three segments, and these points have led many authors to look upon them as forming a transition between the Archnida and the Hexapods. They also possess stigmata in the thoracic region, a condition only paralleled in the Arachnida in certain of the mites.

In his paper Bernard takes up first the external anatomy and the interesting features here are: the interpretation of the cephalic lobes as the lateral regions of the first segment which have been changed in position with the transfer of the cheliceræ; and he further tries to find them in the cephalic lobes of embryos of other Arachnids, a view with very little in morphology to support it. The beak is interpreted as fused labium labrum, neither of these, as the name of the first might imply, being appendicular in nature. The ocular tubercle is regarded as the only remnant of the original dorsal surface of the head, the rest having been displaced by the upward and backward movement of the cephalic lobes; and, from this, the median eyes are regarded as the more primitive, the lateral as secondarily acquired. The descriptions of the limbs, as well as of the apodematous skeleton affords little to abstract, except that the author suggests that since specialized poison organs are absent the poison may come from setal-pores on the chelicerse; and that, at any rate, the idea of their poisonous nature should not be set aside without further experiment. As little need be said of the account of the hypodermis or of the muscular systems.

The account ef the nervous system is disappointing. Although sections were cut (cf., p. 345) no use of them appears to have been made

[1] Trans. Linn. Socy. London, Zool. Vol. vi, pt. 4, 1896.

in the study of the topography of the system and we are left absolutely in the dark as to the presence of ganglia in front of those of the chelicerae; a point of no little importance. The eyes receive hardly more satisfactory treatment, owing to the unsatisfactory condition of the specimens. No vitreous body was found in the median eyes while the retinal cells showed no rods, and no grouping of these into a rhabdem was seen. The lateral eyes vary in size, shape, and arrangement and are described in some cases as having fused on either side of the head, although no evidence is presented of such fusion. The pedipalpal organs, reversible sacs on the tips of these appendages are described in detail and are clearly sensory as are the "racquet organs" on the last pair of thoracic appendages.

The alimentary canal opens by the mouth at the end of the beak, the opening being fringed with a strainer of bristles, while the œsophagus, in front of the œsophageal collar, is modified into a "sucking stomach." The midgut is provided with gland, like diverticula and although they are grouped into those of the cephalothorax and abdomen, all clearly belong to one series, but those of the abdomen are remarkable not only from the number but from the fact that they empty into a collecting duct on either side and these ducts, in turn, empty into the intestines near the base of the abdomen. The Malpighian tubules are well developed and are described as emptying into the midgut, and Bernard accepts the views of Loman that these organs in the Arachnids cannot be homologous with the similarly named structures in the Hexapods. The heart has retained 8 pairs of ostia, while there are indications of another segmental chamber in front. From in front an aorta carries the blood forward and "appears to discharge the blood directly on to the central nervous system. There are no indications of the circumneural vessels like those of the Scorpions and of which Mr. Bernard holds, in some respects, peculiar views.

The respiratory system affords more that is interesting. The observations of previous students that there are three pairs of stigmata (and sometimes a fourth unpaired) is confirmed. Of these the first pair open behind the coxæ of the second pair of legs while the others compare with the anterior pulmonary openings of the Scorpions. Arguing from the conditions of the blood-vessels (and more from his preconceptions of the phylogeny of the respiratory organs Bernard concludes that there were originally two other tracheal openings in the thorax. There then follow some interesting but inconclusive remarks upon the primary number of stigmata in different Arachnids. While dealing with these respiratory structures the author deals with the question of

the origin of tracheæ from lung books (p. 375) and accepts the view that the former were the more primitive, the latter secondary, and reinforces it with the remark that this view "arrived at by comparative morphology, has recently been confirmed by embryology. Janorowski has discovered that the tracheal invaginations of Spiders first from branched tracheal tubes and that the lung books are a secondary specialization." And this without the slightest reference to the results of Simmons (since amply confirmed by Purcell and Brauer) which are directly the reverse. It is to be said in passing that the thoracic stigmata of the Solpugidæ, like those of the Acarina, are the greatest difficulty presented to those who believe in the Limulus-Arachnid theory, but the author dismisses the results of Wagner in this connection with the remark "that all conclusions based upon transitional phenomena of single specialized types will have ultimately to be tested by a profounder and more extended comparative study of existing forms."

The coxal glands, naturally have much attention. The external opening occurs between legs 3 and 4, the duct is long and convoluted while the gland itself is described as a great mass of tubules. These organs he is still inclined to think the derivatives of setiparous sacs, a view which "has hitherto met with no favor." Regarding the fact that they may be cœlomic in character he merely refers to Lauries observations on the scorpion and says that until this be confirmed the bulk of evidence seems to point to the coxal glands as a blind ending tube. And again (p. 381). "I freely admit that these arguments would have but little weight as against direct embryological evidence, if that evidence were really satisfactory." Certainly the results of Grobben, Kishenonyi, Lebinsky, Kingsley and especially those of Brauer are confirmative of those of Laurie, all showing the coxal glands are derived from the cœlomic wall and are the purest of mesoderm (if there be such a layer) and that their external opening is a subsequent formation. For the opposite view, held by Bernard, there seems not the slightest evidence.

After a few remarks upon the genital organs the author presents an attempt to elucidate the phylogeny of the Arachnida, and it is here that we are most at variance with him. It is impossible to go into his argument in detail. It all rests upon the attempt to derive every existing Arthropod structure from structures already present in the annelid ancestor, setiparous sacs apparently playing the most important point. These coxal glands, tracheæ, poison glands, stink glands, spinning glands cement glands, maxillary glands, salivary glands, etc., are all referred

back to the setiparous gland of the annelid; yes further, the hairy bodies of the Solpugids and Mygalidæ are direct inheritances from the annelid setæ. Scorpio is not primitive but rather a specialized form. In some of his statements of fact he also seems to be in error. Thus he says (p. 398) "What actual evidence we have as to the character of the abdominal limbs [in the primitive Arachnid] shows that they were filamentous jointed appendages like those on the cephalothorax." On the contrary in Scorpions (cf. Brauer, Patten) which, with all deference to Mr. Bernard, we continue to regard the most primitive of existing Arachnids, they appear in the embryo as flat lamellate limbs. Again (same page) he says that the sensory plates on the pectines of the scorpion are on the ventral and not on the posterior face of the limb. On the contrary they are on the posterior side as the figures of both Patten and Brauer show. But what we have most to criticise is the failure to refer to opposing views or corrections of previous statements. Thus he refers to "stigmatic scars" along the whole length of the abdomen of the Pseudoscorpions, scars which bear another interpretation. He speaks of the entostemite as ectodermal, without stating that a portion of it is mesodermal (Schimkewitsch), while we have referred to other cases above.—J. S. K.

The Bears of North America.[1]—A new classification of the bears of North America is proposed by Dr. Merriam. This classification is based on the study of more than 200 skulls, including about 35 skulls of the huge bears of the Alaska coast region. The number of full species recognized by Dr. Merriam is ten: 4 of the Black Bear group; 2 of the Grizzly group; 3 of the big brown bears of Alaska, and the Polar bear. Four of these species are new; (1) the gigantic fish-eating bear of Kadiak Island and the Alaskan Peninsula, *Ursus middendorfii* Merr.; (2) the large brown bear of Yakutat Bay and the coastal slope of the St. Elias Alps, *Ursus dallii* Merr.; (3) the large brown bear of Sitka and the neighboring islands, *Ursus sitkensis* Merr.; and (4) the Florida Black bear, *Ursus floridanus* Merr. The Sonoran Grizzly and the Norton Sound Grizzly are considered as subspecies only. The Alaskan bears fall into 2 distinct groups. (1) *U. sitkensis* and *U. dallii*, which resemble the Grizzlies in the flatness of their skulls, but are larger and differ from them in color and dentition; and (2) *U. middendorffii* which differs markedly from all other American types, and closely resembles the Great Brown Bear of Kamschatka. Merriam's synopsis is illustrated by figures of the skulls of the different species.

[1] (Proceeds. Biol. Soc., Washington, April, 1896.)

As an account of the North American bears this paper is far in advance of anything hitherto published.

The difficulty of distinguishing several species of the typical Ursi in North America has not been so much the absence of characters among themselves, as the intermediate position of the old world *Ursus arctus* with regard to them. Middendorff's studies of this species convinced him that it varied in size 33 per cent. of the largest dimensions, and in other respects, but he could not refer the varieties to more than one species. With these very elaborate studies as a basis, J. A. Allen and A. E. Brown in subsequent years could only see in the North American grizzly and black bears, geographical races. The fault then of Dr. Merriam's paper is, that he has not given any account of the relations of our bears to the intermediate series of the Old World.

Dr. Merriam is a genus fancier, and he bids fair to adopt all of the names of his illustrious predecessor Dr. J. E. Gray of the British Museum. Thus he adopts Gray's name, Thalarctos for the polar bear on characters which do not exist. He dallies with Euarctos for our black bear for equally poor reasons. We must admit, however, that Dr. Merriam does for the first time give satisfactory characters with which to distinguish this species from the *Ursus arctus.*

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General Notes.

PETROGRAPHY.[1]

The Eruptives and Tuffs of Tetschen.—Two interesting articles on the area of crystalline rocks east of Tetschen on the Elbe, have appeared simultaneously. The first, by Hibsch, is a description of the Tetschen[2] sheet of the map of the Bohemian Mittlegebirges, and the second by Graber,[3] is on the fragments and bombs occurring in the tephrite tuffs of the region.

The volcanic rocks of the district are interbedded basalts, tuffites, tuffs and tephrites, of which the fragmental rocks are in greatest abundance. Augitites also occur as sheets, and camptonites as dykes in upper Cretaceous marls. The older igneous rocks are granitites and diabases that are associated with clay slates, probably of Cambrian age. Analyses of each of these rocks are given but the rocks are not described in detail. The greater portion of the author's article deals with the volcanic rocks. The tuffs are composed of basaltic and tephritic fragments of the coarseness of sand in some cases, and in others of

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Min. u. Petrog. Mitth., XV, 1895, p. 201.

[3] Ib., p. 291.

pieces several feet in diameter. These are cemented together by finer portions of the same substances, among which have been deposited zeolites, carbonates, opal and other secondary minerals. Some beds of this tuff are so filled with large fragments of basalt, tephrite, etc., that the rock composing it has been called the "Brocken Tuff." It is to the study of the fragments in this tuff that Graber's paper is devoted.

The basalts and tephrites constitute sheets and lava streams that are interstratified with the tuffs and sediments. Among the former rocks are noticed feldspathic, leucitic and nephelinic varieties, besides in several places magma-basalts. In addition to sheet basalts, dykes and chimneys of this rock have also been observed.

The rocks in all their forms are normal in their development. The author regards contact action around the chimneys as the safest criterion by which to distinguish these forms from denuded sheets and flows. The tephrites comprise hauyn-tephrites, in which hornblende and aegerine are present, nepheline-tephrite, including trachytic and andesitic varieties, and leucite-tephrite composed of phenocrysts of augite, plagioclase and grains of magnetite in a groundmass of these same components, and leucite, biotite and nepheline.

The augite consists of two generations of magnetite and augite in a glassy base. Its analysis gave:

SiO_2	TiO_2	P_2O_5	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	H_2O	Moisture	Total
43.35	1.43	1.54	11.46	11.98	2.26	7.76	11.69	.99	3.88	2.41	.59	=99.34

The feldspathic basalt and the andesitic tephrite are the only rocks that seem to have affected the sediments with which they are in contact. Quartzites are changed to aggregates of quartz grains in a glass matrix, where the action is not extremely severe, and to an aggregate of interlocking quartz grains where it has been intense. The article closes with an account of the detailed results of analysis of ten specimens of the volcanic rocks.

Graber's article is devoted principally to a description of the fragments found in the Brocken-tuff. These are all tephritic rocks, among which andesitic, leucitic and phonolitic types are recognized. The characteristics of the components of all these types are portrayed in great detail, especial care being given to the descriptions of the augite and the plagioclase. The phonolitic tephrite is characterized by the presence of nosean, which is in irregular grains. In the andesitic tephrite, which is the most basic variety, the porphyritic augite has an extinction angle $c \wedge C$ of 58°–62°, in the leucitic type its extinction is

52°–56° and in the phonolitic type, the most acid variety, it is 50°–53°. In each of the types labradorite and sometimes oligoclase phenocrysts are common, but the feldspar of the groundmass differs in character in the different types. In the andesitic type it is oligoclase, in the leucite variety andesine, and in the phonolitic type sanidine.

A Nepheline-Syenite Bowlder from Ohio.—Miss Bascom[4] has found in the drift near Columbus, Ohio, a bowlder which consists of nepheline-syenite porphyry. The rock is composed of large phenocrysts of oligoclase and smaller ones of nepheline, augite, hornblende and olivine in a groundmass composed of plagioclase and orthoclase laths, hornblende, biotite, augite and magnetite in a feldspathic matrix.

Crystalline Rocks of New Jersey.—In a report on the Archean Highlands of New Jersey, Westgate[5] states that the northern half of Jenny Jump Mt., Warren Co., consists mainly of gneisses with a small area of crystalline limestone, diorites, gneisses, etc. The gneisses are granitoid biotite-hornblende varieties, biotite-gneisses and hornblende-pyroxene gneisses. In the first named variety the prevailing feldspars are microcline and microperthite, and in the pyroxene gneisses plagioclase and orthoclase. The gneisses are cut by pegmatite dykes, amphibolites and diabases.

Associated with the white crystalline limestones are fibrolite and biotite gneisses, hornblendic gneiss, amphibolites, gabbros, norites and diorites, most of the latter of which show evidence of an eruptive origin. Another type of rock often found associated with the limestones is a quartz-pyroxene aggregate, in which the pyroxene is a green or white monoclinic augite. The limestone, the fibrolite and biotite gneisses and the quartz-pyroxene rock are thought to be metamorphosed sediments.

Simple Crystalline Rocks from India and Australia.—Judd[6] gives us an account of several simple crystalline rocks from India and Australia. One is a corundum rock composed principally of corundum grains with rutile, picotite, diaspore and fuchsite as accessory constituents. The corundum is in part pale colored and in part strongly pleochroic. The grains of the latter extinguish together producing with the former a micro-poicilitic structure. One of the specimens examined came from South Rewah and the other from the Mysore State.

[4] Journ. Geol., Vol. IV, p. 160.

[5] Ann. Report State Geol. of New Jersey for 1895. Trenton, New Jersey, 1896, p. 21–61.

[6] Mineralogical Magazine, Vol. XI, p. 56.

Associated with the corundum in the Mysore State is a fibrolite rock. A tourmaline rock from the Kolar gold field in the same State and from North Arcot and Salem in Madras, consists of twisted and bent tourmaline fibres in a matrix of smaller fibres of the same substance. In the neighborhood of Bingera, New South Wales, two rocks are found as dykes cutting serpentine. One consists almost exclusively of green garnets and the other of picotite. The former contains also gold and chrysocolla.

The Weathering of Diabase.—Mr. Merrill[7] describes the changes that have been effected in a granular diabase at Medford, Mass., during its disintegration into soil. Bulk analysis of the fresh and the weathered rock yielded the following results:

	SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign	Total
Fresh	47.28	20.22	3.66	8.89	7.09	3.17	.77	2.16	3.94	.68	2.73	=100.59
Weathered	44.44	23.19	12.70		6.03	2.82	.52	1.75	3.93	.70	3.73	= 99.81

The disintegration of the rock is accompanied by a leaching out of its most soluble constituents. Assuming that the alumnia has remained unchanged in quantity in the course of the disintegration, the percentage of each constituent lost in this process is shown to be as follows:

SiO_2	Al_2O_3	Fe_2O_3 FeO	CaO	MgO	MnO	K_2O	Na_2O	P_2O_5	Ign
18.03	.00	18.10	25.89	21.70	41.57	29.15	12.83	11.39	.00

The paper is full of valuable suggestions that cannot be even referred to in these notes.

Petrographical Notes.—Transitions from massive anorthosites into augen gneisses and into thinly foliated gneisses and transitions from olivine gabbro into hornblende schists are briefly described by Kemp[8] in a preliminary article on the dynamic metamorphism of anorthosites and related rocks in the Adirondacks.

Pirsson[9] suggests the use of the word anhedron to express the meaning usually expressed in the phrase 'hypidiomorphic form.' An anhedron is a body with the physical constitution and properties of a crystal but without the crystallographic form. The term may be conveniently applied to the crystalline grains in rock masses.

[7] Bull. Geol. Soc. Amer., Vol. 7, p. 349.
[8] Bull. Geol. Soc. Amer., Vol. 7, p. 488.
[9] Ib., Vol. 7, p. 492.

GEOLOGY AND PALEONTOLOGY.

The Limestones of the Jenny Jump Mountains, New Jersey.—Accompanying the report on the Archean Geology of New Jersey, by Mr. J. E. Wolff is a paper by Mr. L. G. Westgate on the Geology of Jenny Jump Mountain, chiefly interesting on account of the conclusions reached by the author concerning the crystalline limestones of that region.

The area under consideration embraces the northern half of Jenny Jump mountain in Warren county, New Jersey. This mountains lies along the northwestern border of the highland area, and is a sort of outlier or peninsula reaching into the later Paleozoic rocks. The main ridge of the mountain consists of gneisses; the limestone occurs at its extreme northeastern end, with outcrops along the southeast border of the mountain.

The author discusses in detail the position, lithology and relations to the crystalline limestones in other parts of New Jersey, and reviews the views of previous writers as to the age of the Sussex county limestone, which has generally been considered the type and representative of other localities. Mr. Westgate's views are given in the following summary:

"The crystalline limestones of Warren county are believed to be distinct from and older than the blue magnesian limestone of Cambrian age, which occurs along the northwestern side of the New Jersey Highlands. They are believed to be distinct, for the following reasons."

"1. They differ lithologically from the blue limestone in being thoroughly crystalline, and in containing large amounts of accessory metamorphic minerals."

"2. They are intimately associated with and apparently interbedded with the older gneisses; and gneisses occur also interbedded in the limestone."

"3. They show no intimate association in areal distribution with the blue limestone, nor any tendency to grade into it."

"4. The metamorphic changes to which the white limestones have been subjected are general in their nature, and are not due to the action of the eruptives by which they are cut; so that no sufficient agent is at hand to account for the supposed change from blue into white limestone."

"The white limestones are believed to be older than the blue Cambrian limestone, because (1) they occur in intimate association with the gneisses which are of admitted pre-Cambrian age, and because (2) they have been subjected to general metamorphic forces resulting in great changes, of which the neighboring blue limestone shows no traces."

"That the other crystalline limestones of New Jersey are of the same age as those of Warren county, has not been proved. The theory has generally been that they are. If they are, and if the position taken in the present paper is valid, then the crystalline limestones of Sussex county, and of other places in New Jersey, would also be, as they have generally been supposed to be, of pre-Cambrian or Archean age." (Ann. Rept., New Jersey State Geologist for 1895. Trenton, 1896.)

Unios from the Trias.—Four new Triassic Unios are described by Mr. C. T. Simpson. The collection of which they form a part was obtained from the Dockum beds, a formation underlying the Staked Plains of Texas. Taken as a whole, these Unios closely resemble in form, and are apparently nearly related to those of the Jurassic beds of North America, while 3 of the species bring to mind most strongly the species which now inhabit Europe and western Asia, and a small group belonging to the Mississippi area. The variety of characters displayed by these Triassic Unios go to show that the genus must have been well established at the time the Dockum beds were laid down, thus tending to overthrow Neumayer's theory that the Unionidæ were derived from the genus Trigonia, which probably does not date back to a period earlier than that of the shells under consideration. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1895.)

The Cadurcotherium.—M. Boule calls attention to the recent discovery of the lower jaw of a Cadurcotherium (Gerv.) at Barlière (Haut-Loire). The specimen denotes an animal of the size of a small rhinoceros. It was found in oligocene arkoses associated with a fine mandible of *Elotherium magnum*, and fragments of Aceratherium, and the remains of turtles. Until now Cadurcotherium has been represented by isolated teeth and fragments of mandibles. The new find is important, showing the animal to be unique among its contemporaries. It presents certain resemblances to South American types—noticably Astrapotherium of the Patagonian Eocene, but is, according to Osborn really related to the rhinocerontic genus Metamynodon.

Notes on the Fossil Mammalia of Europe, V—The Phylogeny of Anoplotherium.—The early attempts at the construction of a phylogeny of the even-toed ungulates, included the genus *Anoplo-*

therium, which was considered by Paleontologists of twenty-five years ago, as a primitive form, especially in its foot structure, *Anoplotherium* certainly possesses a number of primitive characters in its manus and pes, such as the separation of the metatarsals, and the non-fusion of the podial elements, but the inadaptive reduction of its digits, as pointed out by Kowalevsky and the peculiar position of the pollux and hallux, excludes the possibility of placing *Anoplotherium* in the direct line leading to any of the living Artiodactyla.

I propose in this short paper to attempt to prove, that *Anoplotherium* has been probably derived from *Dacrytherium*, a closely allied genus, but whose foot structure is normal and which resembles that of many of the early Eocene Artiodactyla such as *Cainotherium*. Prof Cope[1] suggested that *Cebochœrus* may have been the ancestor of *Anoplotherium*, but the structure of the skull in *Cebochœrus*, is already quite modernized, nearly as much so as in the true pigs, consequently I am inclined to think that we shall have to look for some other form as ancestral to *Anoplotherium*.

The general form of the skull in *Dacrytherium* is like that of *Anoplotherium*, however, in *Dacrytherium* there is a strongly pronounced preorbital fossa, which is absent in *Anoplotherium*. The crowns of the upper teeth in *Dacrytherium* are low and primitive in structure. They exhibit rounded external crescents, which are not at all angular. In *Anoplotherium*, especially the large species, the crowns of the superior true molars are more lengthened than in *Dacrytherium* and the external crescents are angular and broad. We see this change in many mammalian phyla from extremely low crowned molars, to those which are tending to the hypselodont condition. As regards the intermediate stage, between *Dacrytherium* and *Anoplotherium*, as to the height of the molars, this is found in the genus *Diplobune*.

The lower true molars of *Dacrytherium* exhibit two internal cones, which is the normal number in the Artiodactyla. It is interesting to record, that I have noticed in a number of young jaws of *Dacrytherium* in which the true molars were just coming through, that the antero-internal cusp, which is single in the adult, shows a slight reduplication, which is the normal condition in *Diplobune*. The division of the metaconid is carried still further in the largest species of *Anoplotherium*, although I have examined many jaws from the Phosphorites of the *Anoplotherium*, and I can confidently state, that all gradations exist between the complete isolation of the two antero-internal cusps of the typical forms of *Anoplotherium*, and the single condition of these cusps,

[1] Artiodactyla, AMERICAN NATURALIST, Dec., 1888, p. 1083.

which is found in the supposed ancestral genus, *Dacrytherium.* Accordingly I am not acquainted with any good generic character at present, which will distinguish the so-called genus *Diplobune* from *Anoplotherium,* as in many cases in jaws from the Phosphorites, it is impossible to say whether they belong to *Anoplotherium* or *Diplobune.* Dr. Henri Filhol informed me that he was of the same opinion, in regard to the validity of the genus *Diplobune.*

In Dacrytherium the hind foot has at least four well developed toes and the internal digit is not placed at an angle with the others as in *Anoplotherium.* This structure of the pes is just what one would expect to find in a genus standing in ancestral relationship to the more specialized members of the *Anoplotheriidæ.* Granting that *Dacrytherium* fulfills in most of its characters, what we require of a form, supposed to be ancestral to *Anoplotherium,* there is still the presence in *Dacrytherium* of a preorbital fossa, which is absent in the skull of *Anoplotherium,* and also another objection, is, that *Dacrytherium* has claw-like ungual phalanges, much as in *Agriochærus.* I believe, however, the extremely compressed ungual phalanges of *Dacrytherium* is of little weight against this genus being ancestral to *Anoplotherium,* for in the latter these phalanges are rather compressed, more so than in the normal Artiodactyles, and they could be easily derived from those of *Dacrytherium.* The structure of the skull is not known in all the species of *Anoplotherium,* and one of them may have had a skull with a preorbital fossa, which is so characteristic of *Dacrytherium.*

As is well known, the original specimens of the manus and pes of *Anoplotherium commune,* which are in the Muséum d' Histoire Naturelle, Paris, show only two well developed digits as restored by Cuvier. This restoration of the feet of *Anoplotherium* is shown by Schlosser and Zittell to have been an error on the part of Cuvier, and I quite agree with these authors on this point. Prof. Zittell in his "*Traité de Palæontologie*" in speaking of the structure of the feet in *Anoplotherium* remarks "La plupart des représentation de la patte d' *Anoplotherium* faites jusqus à present omettent par erreur à la patte antérieur l' index et le rudiment de pouce, à la patte postérieur le second doigt." I have examined a fine cast of the hind foot of *Anoplotherium commune* and I find that the restoration of the internal portion as completed by Cuvier is quite erroneous. The two small bones placed by him on the tibular side of the pes do not at all fit the facets on which they are placed. The broad and obliquely placed facet on Mt. 111 in *A. commune* is for the large and wide spreading second digit, this same structure of the metatarsal occurs in *A.* (*Eurytherium*) *latipes* of the upper Eocene of Débruge.

Summing up the principal changes which have occured in the evolution of *Anoplotherium* from *Dacrytherium*, I emphasize the following: 1. Increase in height of the crowns of the upper molars, and the reduplication of the metaconid of the lower molars, this division of the metaconid is found in an incipient condition in young jaws of *Dacrytherium*. Complete separation of the metaconid into two distinct cusps only occurs in some forms of *Anoplotherium*. 2. The hind foot of *Dacrytherium* is normal in structure, and has at least four toes, this is the primitive type of pes, from which the specialized foot of *Anoplotherium* has been derived.

Note.—In my "Notes on the Fossil Mammalia of Europe," part III, AMERICAN NATURALIST, April, 1896, I find two mistakes, which should be corrected. On page 309, third and fifth lines from top, read *Adriotherium*, instead of *Adiotherium* as printed, and also page 310, eighth line from the bottom, read *Anoplotheriide*, in place of Suillines.—CHARLES EARLE.

BOTANY.[1]

De Toni's Sylloge Algarum.—Dr. De Toni[2] has recently issued the third volume of his *Sylloge Algarum*. It deals entirely with the Brown Algæ or *Phæophyceæ*—the FUCOIDEÆ as he calls them. A thousand species are described under one hundred and eighty genera, which are grouped into twenty-nine families. He divides the group into three orders, *Cyclosporinæ*, (Fucaceæ) *Tetrasporinæ* (Dictyoteæ) *Phæozoosporinæ* (Phæozoosporeæ).

Splanchnidium rugosum the interesting plant which after careful study was placed by M. O. Mitchell and F. G. Whiting[3] in the *Phæosporinæ*, is retained in the *Durvilleaceæ*, the fruit being described as a polysporous oogone. The general appearance of the plant and the structure of the conceptacles suggest a close relationship with the fucoids, but if the above investigations are to be accepted the plant

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] *Sylloge Algarum Omnium Hucusque cognitarum* by J. Bapt. De Toni, Vol. III, *Fucoideæ*.

[3] On Splanchnidium rugosum Grev. the type of a new order of Algæ, Phycological Memoirs, Pt. I. I., 1892.

bears zoospores in the conceptacles and not oogones, hence it must be placed in the *Zöosporinæ*.

The treatment of the Zoosporinæ is practically that of Kjellman in Engler and Prantl's, *Pflanzenfamilien*, except that the genera *Lithoderma* and *Arthrocladia* are placed in families by themselves, instead of in the *Ralfsiaceæ* and *Desmarestiaceæ* respectively, and that De Toni has included five small, mostly, monogeneric families, the *Phæothamniaceæ*, *Phæocapsaceæ*, *Hydruraceæ*, *Chromonodaceæ* and *Chromophytonaceæ* not mentioned by Kjellman. In all the Zöosporinæ except the above families the zöospores as far as known are laterally biciliated and are borne in some form of zöosporangia. In these families there are no zoosporangia and in at least a part of them the zoospores are not laterally biciliated and in general their relationship seems to be with the *Chlorophyceæ*. It seems more natural to place them, as Wille has with some of them, in the *Chlorophyceæ* next to their closely related genera.

The book is well arranged; priority in class, ordinal and family. nomenclature is strictly observed. It will be indispensible to the specialist in this line and a great help to the general student.—De Alton Saunders.

The Flora of the Black Hills of South Dakota.—In a recent number of the Contributions from the U. S. National Herbarium (Vol. III, No. 8; issued June 13, 1896), P. A. Rydberg gives the results of his explorations (in 1892) of the Black Hills of South Dakota. The report, which includes about eighty pages, includes the following, viz.: Itinerary, Geography, Geology, Altitudes, Precipitation and Temperature, Floral Districts, General Remarks, and the Catalogue of Species. The plates are a Map of the Black Hills, *Aquilegia brevistyla*, *Aquilegia saximontana* and *Poa pseudopratensis*. The floral districts recognized by the author are five, viz.: (1), the foothills and surrounding plains, (2), the Minnekata Plains, (3), the Harney Mountain Range, (4), the Limestone District, (5), the Northern Hills.

In summing up his discussion of the vegetation of these districts the author says, " From the foregoing can be seen what a varied flora the Black Hills have. There are found plants from the East, from the Saskatchewan region, from the prairies and table-lands west of the Missouri River, from the Rocky Mountains, and even from the region west thereof. In the foothills and the lower parts of the Hills proper the flora is essentially the same as that of the surrounding plains, with an addition of eastern plants that have ascended the streams. In the higher parts the flora is more of a Northern origin. Most of the plants

composing it are of a more or less transcontinental distribution, but often characteristic of a higher latitude. Some can be said to belong to the Rocky Mountain Region. The only trees of western origin are *Pinus ponderosa scopulorum*, and *Betula occidentalis*; the others are eastern, or transcontinental. The flora resembles, therefore, more that of the region around the Great Lakes than that of the Rockies."

It merely remains to say that the nomenclature and capitalization (*all* specific names decapitalized) of this interesting and valuable report are of the most advanced type.—CHARLES E. BESSEY.

Trelease's Hickories and Walnuts of the United States.—Dr. Trelease has rendered a good service to the botanists of the country by publishing (in the Seventh Annual Report of the Missouri Botanical Garden) the results of his studies of the Juglandaceæ of the United States, especially with reference to their winter characters. The species recognized are:

Hicoria pecan (Marshall) Britton.—Iowa to Southern Indiana, Kentucky, Louisiana and Texas, extending into Mexico.

H. myristicæformis (Michx. f.) Britton.—Arkansas to Alabama, Texas and Mexico, and in South Carolina.

H. aquatica (Michx. f.) Britton.—Virginia to Florida, around the Gulf to Texas, thence north to Arkansas and southern Illinois.

H. minima (Marshall) Britton.—Canada and Maine to Minnesota and Nebraska, south to Texas and Florida.

H. glabra (Miller) Britton.—Atlantic region from Massachusetts and Pennsylvania to Florida.—var. *odorata* (Marshall) Sargent.—Mississippi valley eastward, and from Canada to the Gulf.—var. *villosa* Sargent.—Missouri, on flinty hills.—var. *microcarpa* (Nuttall) Sargent.—Same range as var. *odorata*.

H. alba (L.) Britton.—Canada to the Great Lakes and Kansas, south to Texas and Florida.

H. mexicana (Engelm.) Britton.—Mexico, in mountains of Alvarez.

H. laciniosa (Michx.) Sargent.—New York and Pennsylvania to Iowa, Kansas and the Indian Territory.

H. ovata (Miller) Britton.—Canada to Minnesota, south to Florida, Kansas and Texas.

Juglans cinerea L.—New Brunswick to Dakota, Kansas, and the Mountains of Georgia and Alabama.

J. rupestris Engelmann.—Texas, New Mexico and Arizona, extending into Mexico.

J. californica Watson.—Coast range of southern California.

J. nigra L.—Massachusetts to Ontario and Minnesota, south to the Gulf.

The paper is accompanied by twenty five plates of trees, bark, buds, leaves and fruits.—CHARLES E. BESSEY.

Diseases of Citrous Fruits.—This recently issued bulletin (8) of the Division of Vegetable Pathology, of the U. S. Department of Agriculture, prepared by W. T. Swingle and H. J. Webber is a valuable contribution to science as well as horticulture. The diseases discussed are Blight, Die-back, Scab, Sooty-mold, Foot-rot, and Melanose. Eight good plates (three colored) accompany the paper.

Mulford's Agaves of the United States.—In the seventh volume of the annual report of the Missouri Botanical Garden, Miss A. Isabel Mulford publishes a monograph of the genus *Agave* so far as the species native to or growing spontaneously in the United States, are concerned. Sixteen species and four varieties are recognized, distributed as follows:

A. virginica L.—Maryland to Florida, Indiana, Missouri and Texas.

A. virginica var. *tigrina* Englem.—South Carolina.

A. variegata Jacobi.—Lower Rio Grande Valley, Texas.

A. maculata Regel.?—southern Texas.

A. schottii Engelm.—southern Arizona.

A. schottii var. *serrulata* n. var.—Rincon Mts., Arizona.

A. parviflora Torrey.—Mts. of Arizona.

A. lechuguilla Torrey.—west Texas and east New Mexico.

A. utahensis Engelm.—Utah, northern Arizona, southern California and Nevada.

A. deserti Engelm.—southern California.

A. applanata Lemaire.—western Texas.

A. applanata var. *parryi* (Engelm.)—southern New Mexico to central Arizona.

A. applanata var. *huachucensis* (Baker).—Huahuca Mts., Arizona.

A. shawii Engelm.—southwestern California.

A. palmeri Engelm.—southeastern Arizona and southwestern New Mexico.

A. asperrima Jacobi.—Spontaneous near San Antonio, Texas.

A. americana L.—Spontaneous in southern Texas.

A. rigida sisalana Engelm.—Naturalized in Florida.

A. decipiens Baker.—southeastern Florida.

A. sp.—Florida.

A. sp.—Texas.

It is with great pleasure that we observe the great reluctance of the author to establish new species; on the contrary she has refrained from giving names where most monographers would certainly have done so. Thus on page 96, after a description which might have been considered adequate, (at least by those who are fond of seeing their names cited in connection with specific names) the author says: "To avoid further confusion in nomenclature I refrain from giving a name to this plant until it is possible to obtain further data." We would commend this sentence to the careful consideration of a certain class of botanists who are apparently more anxious for their own "credit" than for the progress of the science.

Thirty eight plates, many of them half-tone reproductions of photographs, accompany this useful paper. If space permitted we should be glad to quote from the author's introductory discussion, which is full of interesting facts and suggestions; thus a case is cited in which the flower-stalk grew for twenty days at the average rate of two and three-fourths inches per day!—CHARLES E. BESSEY.

ZOOLOGY.

Sense of Sight in Spiders.—A detailed account of the experiments conducted by G. W. and E. G. Peckham for testing (1) the range of vision and (2) the color sense of spiders is published in a late volume of the Trans. Wisconsin Academy. The evidence offered by the authors is based upon a study of twenty species of Attidæ. This study has extended over eight successive summers, during which notes were made of many hundreds of observations. The movements and attitudes of the spiders of the group chosen are wonderfully vivid and expressive. The males, in the mating season, throw themselves into one position when they catch sight of a female, and into quite another at the appearance of another male. This power of expression through different attitudes and movements is of great assistance in determining not only its range of sight, but also its power of distinct vision.

The spiders were confined in boxes, the sides of which were marked off into inches. The bottom was of cotton cloth, the top of glass. Notes were taken of the distances at which prey was noticed, followed and captured. During their mating season the evidence was conclusive that these spiders not only see, but see clearly at considerable distance. The

following description of one of the many experiments described in the article serves to show the method of investigation:

A male of *Saitis pulex* was put into a box containing a female of the same species. "The female was standing perfectly motionless, twelve inches away, and three and a half inches higher than the male. He perceived her at once, lifting his head with an elert and excited expression, and went bounding toward her. This he would not have done if he had not recognized her as a spider of his own species. When four and one-half inches from her he began the regular display of this species, which consists of a pecular dance. This he would not have done had he not recognized her sex."

At another time a male of *Hasarius hoyi* was dropped into a box with another male which was standing seven inches away. "He at once threw up his first legs, this being a challenge to battle. The other male responded by throwing up his first legs. The two advanced upon each other slowly, and when only two inches apart began to circle about each other, waving their legs. The same male when put into a box with a female saw her as she stood quite eleven inches away, and at once lifted his first legs, not straight up, as in the case with the other male, but obliquely, and began to move with a gliding gait from side to side, this being the characteristic display before the females in this species."

That the spiders recognize each other by sight and not by any other sense is evidently shown by the fact that they remain unconscious of each other's presence when back to back, no matter how excitable they are when they come within range of each other's vision. As a further evidence of recognition by sight a male of *Dendryphantes elegans* was removed from the box in the midst of his courtship of a female, his eyes gently blinded with paraffine, and then restored to the box. He remained entirely indifferent to the presence of the charmer that had so much excited him a few moments before.

To sum up the result of these experiments:

"The Attidæ see their prey (which consists of small insects) when it is motionless, at the distance of five inches; they see insects in motion at much greater distances; they see each other distinctly up to at least twelve inches. The observations on blinded spiders, and the numerous instances in which spiders were close together, and yet out of sight of each other, showing that they were unconscious of each other's presence, render any other explanation of their action unsatisfactory. Sight guides them, not smell."

As to a color-sense in spiders, the authors are of the opinion that their experiments, while not conclusive, yet all taken together, strongly indicate that spiders have the power of distinguishing colors. (Trans. Wisconsin Acad. Sciences, Vol. X, 1895.)

Classification and Geographical Distribution of the Naiades.—In his study of the fresh water pearly muscles, Mr. Simpson finds that the division of these mollusks into two families, Unionidæ and Mutelidæ, founded on the completeness or incompleteness of the development of the siphons, cannot stand. He accordingly diagnoses the two families on the basis of the shell characters, and finds that his distinctions fully agree with what is known of the facts of geographical distribution of the paleontology of the Naiades, and the classification of v. Ihering, based on the characters of the embryos. The Unionidæ, as defined by the author, include the genera Unio Retzius, Anodonta Lamark, Prisodon Schumacher, Tetraplodon Spix, Castalina v. Ihering, Burtonia Bourguignat, Arconaia Conrad, Cristaria Schumacher, Lepidodesma Simpson, Pseudodon Gould, Leguminaia Conrad and Solenaia Conrad. In the Mutelidæ he places the following genera:—Mutela Scopoli, Chelidonopsis Ansey, Spatha Lea, Pliodon Conrad, Brazzæa Bourguignat, Glabaris Gray, Iheringella Pilsbry, Monocondylæa d'Orbigny, Fossula Lea, Mycetopoda d'Orbigny.

The author considers the relationship between these two great groups as not a very close one. The Unionidæ are characterized by schizodont teeth and a *glochidium* embryo. The Mutelidæ have taxodont teeth, and, so far as is known, the embryo is a *lasidium*.

Mr. Simpson finds that the Naiades are capable of being grouped into assemblages of related forms which have a more or less immediate common ancestry; and on the basis of this grouping they are distributed into eight provinces, as is shown in the following table:

Palearctic, ...	Europe. Northern and Western Asia. North Pacific to the Desert. Pacific drainage of North America.
Ethiopian, ...	Africa south of the Sahara.
Oriental, ...	Asia south of the Himalayas. East Indies to the Solomon Islands.
Australian, ...	Australia. Tasmania. New Zealand.
Neotropical, ...	South America.

Central American, . .	Central America. Mexico east of the Cordillera. Cuba.
Mississippian, ...	Entire Missisippi Valley and the Gulf drainage from West Florida to the Rio Grande. Mackenzie River system. Red River of the North. Great Lakes.
Atlantic, ...	Lower St. Lawrence and rivers of eastern Canada. Atlantic drainage of the United States.

The Unios date back in America to the Trias, where they were first discoved by Prof. E. D. Cope. The relations of the existing Naiad fauna with the fossil forms is given by the author as follows:

"The post-Cretaceous Unios of the northwestern States is evidently closely related to the fauna of the Mississippi Valley, and this seems to be related to that of Tropical Africa, as well as to the tertiary forms of eastern Europe and Siberia. The Unios of Australia and South America are apparently closely related to those of the Australian region. There seems to be, too, a general relationship between the Mutelidæ of Africa and South America. These Mutelids and the Unios which bear the embryos in the inner gills have perhaps formerly occupied extensive areas in the northern hemisphere, and may have been supplanted by more modern forms." (Proceeds. U.S. Natl. Mus., Vol. XVIII, 1896.)

Arkansas Fishes.—As the result of less than three weeks' collecting in western Arkansas, eastern Indian Territory and the St. Francis River in northeastern Arkansas, Prof. Meek obtained 83 species of fishes. A new Notropis was found in the Potean River, and a new species of Fundulus is described from the St. Francis. Mollusks are abundant in old river, the old channel of the St. Francis. Six species of Unionidæ were found at a locality farther north than hitherto reported. (Bull. U. S. Fish Commission for 1895, Wash., 1896.)

Batrachia and Reptilia of Madagascar.—The two collections of reptiles from Madagascar, now in the Natural History Museum of Paris, have been examined by M. Mocquard, who reports upon them as follows: The Grandidier collection comprises 68 species in all, Ophidians 13, Bathrachians 20, of which 3 are new species belonging to the genera Mantidactylus, Rhacophorus and Calophrynus. Lacertilians 35, including 2 new species, referred to the genera Lygodactylus

and Phyllodactylus. The Allnand and Belly collection comprise 33 Reptiles and 16 Batrachians. Among the latter are 2 new species of Mantidactylus and 1 of Stumpffia. There are but 11 Sphidia, but these include types of two new genera, Compsophis and Alluondina and a new species of Pseudoxyrhopus. The Lacertilia, 22 in number, yield 4 new species referred to the following genera: Chameleon, Brookesia, Uroplates and Paracontias. The diagnosis of the new Reptiles of this collection have been previously given in the *Comptes rendus de la Soc. Philom.* for 1894.

A comparison of these two collections, with the forms described by Prof. Boettger from Madagascar, shows that certain species considered by him as peculiar to Nossi-Bé are found distributed all through the northern part of the island. This is true not only of the Reptiles but of the Batrachians also. (Bull. Soc. Philom., Paris, 1895.)

The Molting of Birds.—In a paper published recently in the Proceeds. Phila. Acad., Mr. Witmer Stone gives a detailed account of his observations on the molting of birds, with especial reference to the plumages of the smaller land birds of eastern North America. Attention is directed to the following points: order, number and times of molt; change of color by abrasion; seasonal plumages; direct change of color in feathers. As a result of his studies Mr. Stone makes the following generalizations:

I. The annual moult at the close of the breeding season is a physiological necessity, and is common to all birds.

II. The spring molt and striking changes of plumage effected by abrasion are not physiological necessities, and their extent is dependent upon the height of development of coloration in the adult plumage, and does not necessarily have any relation to the systematic relationships of the species.

It naturally follows that closely related species may differ materially in the number and extent of their molts, and that males and females of the same species differ greatly in this respect when the nuptual plumage of the adult male is highly developed as compared with that of the female or with its own winter plumage.

III. The amount of change effected in the plumage at any particular molt varies considerably in different individuals of the same species and sex.

IV. Some species which have a well marked spring molt in their first and second years may discontinue it afterwards, when the adult plumage has once been acquired. And, on the other hand, some indi-

viduals may continue to molt in the spring, while others of the same species cease to do so.

V. The remiges are molted less frequently than any other part of the plumage. As a rule, they are only renewed at the annual molt (exception Dolichonyx).

VI. Variability in the order of molt in the remiges and presence or absence of molt in the flight feathers at the end of the first summer are generally family characters, i. e., Ceryle differs from any other species treated of in this paper in the order of molt in the primaries. All Picidæ and all Icteridæ, except Icterus (and Dolichonyx ?), molt the flight feathers with the rest of the first plumage. None of the Oscines except Icteridæ (as above), some (all ?) Hirundinidæ, Olocoris and Cardinalis molt the flight feathers at this time.

Mr. Stone's conclusions as to "color-change without moulting" are the same as those reached by Chapman, in his article on "The Changes of Plumage in the Dunlin and Sanderling," namely: that color-change without molt or abrasion is incapable of taking place from the very nature of the structure of a feather, and that all the cases so reported can be otherwise acconnted for. (Proceeds. Acad. Nat. Sciences, Phila., 1896.)

The Florida Deer.—The fact that the Florida deer is but little more than half the size of the deer of northeastern United States, together with certain cranial and dental peculiarities, is sufficient, according to Mr. Outram Bangs, to give it full specific rank. He therefore describes it under the name *Cariacus osceola*. The most striking differences between the Florida animal and its northern relatives are (1) the shape and size of the nasal and maxillary bones, and (2) the very large molar and premolar teeth. (Proceeds. Biol. Soc. Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

Professor Forbes' Eighth Report.—The nineteenth report from the office of the State Entomologist of Illinois, covering the years 1893–4, has recently been issued. It is the eight report of the present incumbent, Professor S. A. Forbes, and adheres closely to the lines of thorough and accurate record, which have made its seven predecessors notable in the literature of economic entomology. The bulk

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

of the volume (189 pages) is devoted to the Chinch Bug—the arch-enemy of Illinois agriculture, a voluminous record being made of the experiments with contagious diseases carried on by the entomologist and his assistants. There is also an article on the White Ant in Illinois, and in an appendix of 65 pages Mr. W. G. Johnson, assistant entomologist, gives an excellent discussion of the Mediterranean Flour Moth.

Flies Riding on Beetle's Back.—Rev. A. E. Eaton, the well-known British entomologist, writing from Bône, Algeria, sends this interesting note to the *Entomologist's Monthly Magazine*: "Across the mouth of the Seybouse, on sandy pasture land bordering the seashore, big coprophagous beetles are common, sheltering in large holes in the soil when at rest, and running about on business. A small species of *Borborinæ* may often be seen riding on their backs, chiefly on the pronotum, and about the bases of the elytra—sometimes half a dozen females on one beetle. The beetles occasionally throw themselves on their backs to try and get rid of them by rolling; but the flies elude all their efforts to dislodge them, dodging out of harm's way into the joinings of the thorax and out again, and darting from back to breast and back again, in a way that drives the beetle nearly mad. In vain she scrapes over them with her legs; in vain does she roll over or delve down amongst the roots of the herbage; the flies are as active as monkeys, and there is no shaking them off. It is difficult to get them off into the killing bottle; nothing persuades them to fly; and they would very much rather stick to the beetle than be driven off it down into the tube."

Proteid Digesting Saliva in Insect Larvæ.—Dr. Wilibald Nagel describes[1] the method of feeding in larvæ of *Dytiscus*. In these larvæ the mouth is very much reduced in size, and the ingestion of food is performed by means of suction through the much modified mandibles, the process being facilitated by the powerful digestive action of the saliva. Under natural conditions the larvæ eat only living animals, but in captivity they will also take pieces of meat. The saliva has a marked poisonous action, killing other insects, and even tadpoles of twice the size of attacking larvæ, very rapidly. The larvæ not only suck the blood of their victims, but absorb the proteid substances. Drops of salivary juice seem to paralize the victim and to ferment the proteids. The secretion is neutral, the digestion tryptic. Similar extra-oral digestion seems to occur in larvæ of ant-lions, etc., and

[1] Biol. Centralbl., XVI, 1896, 51–57, 103–112.

spiders, and according to Krause, in Cephalopods.—*Journ. Royal Micros. Society.*

Weismann on Dimorphism in Butterflies.—For some time *The Entomologist* has been publishing a series of interesting articles by Dr. August Weismann on the Seasonal Dimorphism of Lepidoptera. The June number contains a recapitulation from which we take this extract: "Although I am far from considering the few experiments, which I could here put forward, as sufficient for reaching a decisive settlement of our opinions on seasonal dimorphism, yet I cannot forbear arranging them, provisionally at least, in reference to our general conceptions of the subject. When, in the year 1875, I first set about investigating the ways of this striking and yet so long neglected phenomenon, I assumed that it was to a certain extent obvious, that this kind of dimorphism was everywhere a direct result of the various direct influences of climate, principally of the temperature, as it effects in regular alternation the spring and the summer brood of many-brooded species. I had also well considered the other possibility, that dimorphism connected with the time of the year might also depend upon the *indirect* influence of the changing environment, *i. e.*, that it might depend upon the *adaptation* to the varying environment of the butterfly according to the time of year."

I then said: "It is not inconceivable in itself, that phenomena occur among the Lepidoptera analogous to the winter and summer clothing of Alpine and Arctic mammalia and birds, only with the difference, that the change in coloring does not arise in one and the same generation, but alternately in different ones." But, at that time the fact that the *upper side* of butterflies, which is usually not adaptive, can be very variable just in summer and spring, sometimes more so than the adaptive under side, appeared to me to contradict this adaptation of seasonal dimorphism. Yet, it was the fact, that the one or the other seasonal form could be produced artificially by the operation of a higher or lower temperature, *i. e.* the stamp of the winter form might be impressed on the summer brood, and *vice versa.* I therefore concluded that it was the measure of heat which was acting during the pupal period which directly formed the species in one way or the other; and I felt the more justified in so doing, as the climatic varieties form a parallel to the seasonal forms, and as the former must, without doubt, be referred to the direct influence of climate, especially of temperature.

Thus, for example, *Chrysophanus phlœas* is seasonably dimorphic in Sardinia and at Naples; the summer form, which develops during the

summer heat, is very dark, almost black, but the spring form corresponds with our German red-golden *phlœas.*

Although to-day I still look upon this view as correct, and a directly altering effect of temperature as proved, yet I have gradually been convinced, that this is not the sole origin of seasonally dimorphic variability, but that there is also *adaptive seasonal dimorphism.* We must, I believe, distinguish *direct* and *adaptive* seasonal dimorphism ; and, I see in this distinction an important advance, which, before all, places us in position to explain the results of the various experiments undertaken by myself and others in a much more satisfactory manner.

I have already pronounced this view in a lecture delivered at Oxford in the beginning of 1894, and I have sought to show that adaptive seasonal dimorphism, which I had previously only put forward as possible, does *actually* occur. The example there given for perfect insects was, indeed, only a hypothetical one, *viz.*, the case of *Vanessa prorsalevana;* but for larvæ, at least, I can select an example from Edward's excellent work on the North American butterflies with tolerable certainty, *viz.*, that of *Lycæna pseudargiolus*, which will be more accurately discussed later on. I did not then know what I learnt shortly afterwards from an interesting little pamphlet of Dr. G. Brandes, that cases of seasonal dimorphism had been known for a long time among *tropical* butterflies, and that among these, at least, one of the seasonal forms depend upon the assumption of a special protective coloring. Brandes maintains, with justice, that the view hitherto widely held among us is erroneous, according to which seasonal dimorphism was not to be expected in trophical countries, since the alternation of seasons is absent there. Periods of rain and drought, at least for many tropical countries, form such an alternation very sharply. At any rate, Doherty, and, somewhat later, de Nicéville, have pointed out, for Indian butterflies, a series of seasonally dimorphic species, not merely by the observation of the alternation of the two forms in nature, but by rearing the one form from the eggs of the other; thus among Satyridæe of the genera *Yphthima, Mycalesis,* and *Melanitis,* and for the species of *Junonia,* it is accepted as proved; and in all these cases the difference between the two forms principally consists in the fact that the one form seems like a dry leaf on the under side, while the other possesses another marking, and at the same time a number of ocelli.

Without engaging in the controversy as to the biological value of these ocelli, I do not for a moment doubt but that the coloring with ocelli is also an adaptive form, possibly protective or intimidating coloring. If one of the two forms had no biological significance, it could

no longer exist; the single adaptive one would have replaced it. But it is obvious that the appearance of complicated details of marking and color, such as ocelli are, cannot be simply the direct effect of heat or cold, drought or humidity. *These influences are not the actual causes of such formations, but only the stimulus, which sets their primary constituents free, i. e.*, induces their development, as I tried to demonstrate in the lecture above noted. As the sufficient cause of the sleep of the marmots does not lie in the cold, but in the organization of the animal which is adapted to the cold, and as the cold only brings the existing predisposition to winter sleep into play, so among these butterflies with adaptive seasonal dimorphism the display of the one or the other marking is apparently connected, partially, at least, with one of the above named outward influences, although in reference to these trophical butterflies we do not yet know to which of them.

We recognize *temperature as the stimulus to development* with the cases of seasonal dimorphism of our indigenous butterflies, as in all cases of seasonal dimorphism, which have hitherto proved experimentally, it is always high and low temperature which gives the outward impulse to the appearance of the one or the other form where this impulse did not come exclusively from *within.*

There are, therefore, two different sources of the appearance of seasonal dimorphism: on the one hand, the *direct action* of alternating external influences, *viz.*: temperature, can bring about this change in the outward appearance; and on the other hand, the processes of selection. It is therefore necessary to consider these two kinds of seasonal dimorphism separately. It will certainly not always be easy to decide between them when a particular case has to be dealt with, as at present it is not always possible to say whether a coloring or marking has a definite biological value or not. Both causes also may co-operate in in one species.

Note on the Classification of Diplopoda.—The admitted impossibility of formulating a generally satisfactory definition of the term species exists partly because systematists have used it in the greatest variety of applications, and partly because natural groups are so diverse in structure and developmental history that a scheme calculated to elucidate one may increase confusion in another. It is hence desirable in proposing or making use of a classification to recognize as clearly as possible the conceptions under which the arrangement into the various categories of natural groups has been made.

The structure and distribution of the Diplopoda make it advantageous and usually easy to arrange them into species, which are groups

of very similar individuals not connected by intermediate individuals with other groups different in details of structure, form or color. An apparent and probably sufficient cause for this is the close similarity of all Diplopoda in life-histories, habits and food. All are scavengers, able to subsist upon a variety of decaying vegetable, or even animal matter, and there has been scarcely any response to calls for special adaptations to life as parasites, commensals, or under other changed conditions. The species of Diplopoda are not only extremely local in distribution, but are generally confined to almost identical habitats, removed from which they do not long survive.

Supposing the Diplopoda to be a natural group descended from a common ancestor, we are compelled to believe that such differences as appear among them are the result of accumulated variation not greatly influenced by external selective causes. Hence, existing differences indicate in general much more remote developmental divergence than in groups which have entered more thoroughly into the struggle for existence by responding to the demands of varied conditions. In this respect the Diplopoda offer a most striking contrast to the Hexapoda, and the results are in accordance; there are more millions of species of Hexapoda than there are thousands of Diplopoda.

Having accepted a criterion of species, the classification into higher groups is perhaps largely a matter of convenience; but convenience, scientific accuracy, and the recognition of affinities, alike demand constant attention to the fact, that the value of any character depends primarily upon its constancy, not upon the apparent *degree* of divergence. This is merely the reiteration of the chief axiom of systematic science, but the abundance of systems which completely ignore this fundamental idea are evidence that much reiteration is still desirable.

While in some natural groups it seems necessary to recognize subdivisions not definable by any constant character or complex of characters in the Diplopoda, we may conveniently proceed upon somewhat better ground, and require that the genera and larger divisions shall be limited by definite structural characters.

A dichotomous classification is theoretically the only exact one, for the reason that three or more natural groups could never be expected to be separated by exactly equivalent structural differences. Practically, however, a dichotomous system is inconvenient by reason of the great number of categories necessary in properly recognizing affinities. Hence, it is not a valid objection to the usual or multifid form of classification that the natural divisions arranged under the same category are not of the same rank, that is, not remote from

each other by equal structural distances. All that can be reasonably demanded of a classification is that its groups of all ranks shall be natural ones, and that the higher the groups, the more constant, and hence fundamental, shall be the characters by which they are separated. Furthermore, it must never be supposed that the variability of a character in one group need affect its importance if found to be constant in another.

As a general policy it is evidently desirable that scientific names of all grades shall mean as much as possible. The objection to the recognition of distinct and definable genera and higher groups on account of the consequent multiplicity of names is usually to be taken as an unscientific willingness to ignore structural differences and natural affinities, in the hope of escaping additional labor. In reality the difficulty of defining groups containing unrelated members, and of becoming acquainted with such through descriptions, much exceeds the temporary inconvenience resulting from change of names.

In attempting to embody in the classification of the Diplopoda a recognition of certain structural differences found to be invariable, several natural and distinct groups of families have been recognized as orders. It is here proposed to render this classification more definite and consistent by the division of two of these orders, in the belief that the resulting groups, in addition to numerous structural differences, have long been divergent in developmental history. The orders thus to be divided are the Diplocheta and the Merocheta. From the Diplocheta it is proposed to separate the true Iulidæ and their allies, under the name ZYGOCHETA, leaving under the Diplocheta Spirostreptoidea and Cambaloidea. The Zygocheta are distinct in many characters of the gnathochilarium, in the transformation of the first pair of legs of males as clasping organs, the adnate external seminal ducts, the absence of legs from the third segment, the presence of legs on the fourth segment, and the structure of the copulatory organs of both sexes. The Diplocheta have the first pair of legs nearly or quite unmodified, the external ducts distinct, the third segment with a pair of legs, and the fourth segment footless. Notwithstanding these and other important and invariable differences, it remains probable that these two orders are more related to each other than to any third group of Diplopoda.

The other case is similar; the Merocheta will, in the restricted sense, contain numerous families allied to the Polydesmidæ, with twenty closed segmental rings; the new order CŒLOCHETA will accommodate the

Lysiopetaloidea and Craspedosmatoidea,[3] and is characterized by the greater number of segments, the free pedigerous laminæ, the seven-jointed legs, the distinct mentum, and the normal presence of eyes. In the Merocheta the apertures of the external seminal ducts are small openings in the chitinous wall of the coxæ of the second legs, connecting with internal tube of nearly uniform diameter. In the Cœlocheta the coxæ contain a large cavity, while the aperture is large, the margin pilose and not chitinous.—O. F. COOK.

EMBRYOLOGY.[1]

The Tentacular Apparatus of Amphiuma.—In the Journal of Comparative Neurology, Vol. VI, March, 1896, Professor J. S. Kingsley has written an article entitled "On Three Points in the Nervous Anatomy of Amphibians" in which he has endeavored to show that the tentacular apparatus of Amphiuma, briefly described by me (Journal of Morphology, Vol. XI, No. 2), has been mistaken for a nerve and blood vessel. I consider the discovery of this degenerate organ of too much phylogenetic importance to be consigned at once to oblivion, and, therefore, offer in this article the results of a more careful study of it.

Since histological detail is important in this investigation, I state briefly the technique. The specimen, seventy-eight millimeters in length and seven millimeters in body diameter, was hardened in Kleinènberg's picro-sulphuric and, passed through the alcohol series from seventy to one hundred per cent and returned to seventy per cent, when the head was severed and placed three days in borax-carmine, then in acid alcohol twenty-four hours, after which it was imbedded in paraffine by the usual method and cut into serial sections one twenty-fifth of a millimeter in thickness.

Figure I is magnified twenty diameters. The outlines of all the features were drawn with a *Zeiss camera lucida*. Every feature appears in

[3] From the true Craspedosomatidæ there may be distinguished the Trachygonidæ, Conotylidæ, and Cleidogonidæ, in addition to the Chordeumatidæ established by C. L. Koch in 1847. The separation of other equivalent groups will probably be necessary when a fuller knowledge of European and Asiatic forms is gained.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

Figure I. Right-hand portion of section through head of Amphiuma 78 millimeters long, f, frontal; P, parietal; OSP, orbitosphenoid; E, eye; m, maxillary bone; mx*, branches of maxillary nerve; Tt, tentacular apparatus; rt, retractor muscle; mx, maxillary nerve.

the section just as distinctly as it is shown in the figure, b is the blood vessel and the adjacent mx* the nerve which Kingsley thought I had mistaken for the tentacular apparatus, Tt. Notice that three branches of the ramus maxillaris course along the external sheath.

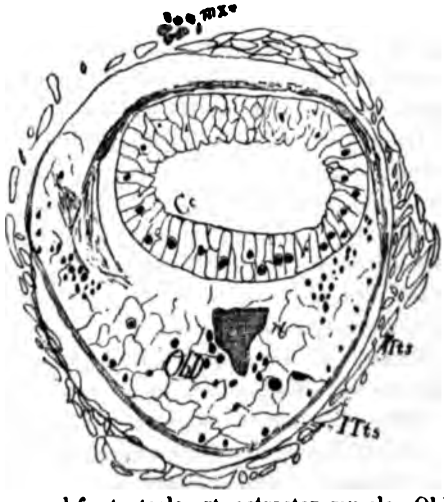

Figure II. Cc, canal for tentacle; rt, retractor muscle; ObD, orbital gland; ITts, inner sheath; ATts, outer sheath.

The histological details of the apparatus Tt. are shown in figure II as they appear viewed with a $\frac{1}{12}$ inch oil immersion lens giving about 1000 diameters. While the columnar epithelial cells lining the tentacular canal Cc are not so regular as one sees in a functional organ yet they are so well defined, especially in the lower portion that the observer cannot be misled as to their identity. The nucleus is visible in about one half the cells and the nucleolus is apparent in many cases. In the upper portion the cells have lost their nuclei and are in a degenerate condition. rt is a cross-section of a muscular element which I believe is the atrophied remains of the muscular retractor of the tentacle. In my preparation, only the bony and muscular tissues have taken on the very light shade of red which characterizes rt. Since the latter is certainly not a bone, I infer it must be a muscle, and if a muscle what other function could it have had than to retract the tentacle. This muscle is visible in ten consecutive sections while the canal Cc appears in greater or less completeness in thirteen sections. The black dots of various sizes seen irregularly distributed throughout the glandular tissue ObD may possibly be nuclei as they are stained a deep red or they may be scattered nerve fibres whose connection with the ramus maxillaris on its branches I have not been able to demonstrate because the degenerate glandular tissue was so loose as to be displaced in several sections. The irregular wavy lines, I think represent cell boundaries. These are visible with an enlargement of two hundred diameters in the lower portion but can scarcely be seen with an oil immersion immediately beneath the canal. ITts is the inner tentacle sheath composed of connective tissue fibres. It is clearly seen in eighteen consecutive sections. ATts represents the outer tentacle sheath which with a low power can be seen in twenty-five consecutive sections. Thus it is observed that this tentacular apparatus is about one millimeter long lying below and external to the eye.

The tentacular canal is complete in only four sections. Figure III represents the fourth section posterior to figure II. The columar epithelium has disappeared on the dorsal side where the inner sheath enters and on one side lies close to the wall, while on the other it mingles with a loose tissue T which may be the remains of a tentacle. This tentacle is prominent in six sections, in three of which the canal is complete so that the inner sheath does not enter it. The lumen of the canal varies but slightly in size. The musculus retractor rt dwindles as we pass anterior or posterior of the section shown in figure II. The glandular tissue decreases both anterior and posterior to the median section. The portion on the ventral side persists the longest, being present in thirteen

sections. The outer tentacle sheath retains the same circumference in about thirteen sections. As soon as the canal and glandular tissue have disappeared the circumference of the outer sheath lessens in both the six posterior sections and the six sections anterior to the thirteen

Figure III. T, tentacle; mx,* branch of ramus maxillarios; other letters same as in figure II.

median sections until it is only one fourth of the full size and the cells of the sheath become scattered, thus finally filling up the central area and creating a solid cord in the last two sections. It is worthy of notice that this tentacular apparatus was observed on the right hand side only in the specimen examined. In three other specimens of the same hatching, though they were several millimeters longer, no trace of the above described organ could be discerned. Kingsley has shown that no such organ exists in his specimens which were from the same lot as mine. An explanation of the occurrence of this organ in only one specimen may be found in the fact that it is an exceedingly transitory formation like the pronephros of the chick, which is present for only one day.

The second objection Kingsley makes to my observations, is that all the eye muscles are present in Amphiuma and the Sarasins say the retractor muscle of the tentacle is probably developed from the retractor bulbi. To this I answer that the Sarasins have not been able to demonstrate positively that the retractor muscle is developed from the retractor bulbi, and if it were true that the retractor muscle is developed from the retractor bulbi, I see no objection to the posterior part of the

retractor muscle functioning as a retractor bulbi after the anterior portion has undergone degeneration.

Kingsley further states that the described apparatus is not in the proper location to be compared to the tentacular organ of the Gymnophiona. In elucidating this point it is of service to compare figure I with figure IV taken from Die Anatomie der Gymnophionen von Wiedersheim.

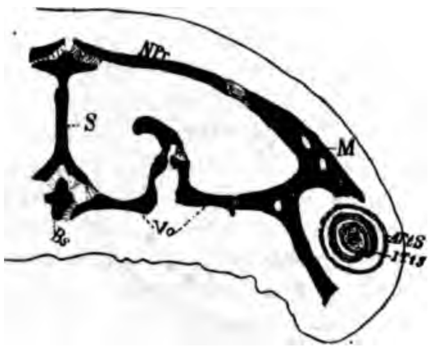

Figure IV. Cross section of *Siphonops annulatus*. NPr, naso premaxillary; Vo. vomer; M, maxillary; Atts, outer tentacle sheath; ITts, inner tentacle sheath. After Weidersheim.

It is seen that the columar-lined canal, inner tentacle sheath and outer tentacle sheath in Siphonops, have the same relation as in Amphiuma. It is further seen that the inner sheath of Siphonops is involuted ventrally to surround the tentacle while in Amphiuma a similar involution is seen on the dorsal side in Fig. III. In both genera the organ is covered merely by the skin and its subjacent tissue. The glandular tissue is not shown in Fig. IV as the section is anterior to the orbital gland. It is true the maxillary bone overhangs the apparatus in Siphonops whereas such is not the case in Amphiuma. In behalf of this contrast I quote from Cope (Bulletin of the United States National Museum, No. 34, p. 214): "There is also a very large foramen or canal passing through the o. maxillare from near its middle to the orbit, foreshadowing the canalis tentaculiferus of the cœcilia." Fig. I. is a section posterior to where the canal would enter the maxillary bone. Among the Gymnophiona there is considerable variation as to the relation of the apparatus to the maxillary bone as the following from Wiedersheim, p. 47 shows: "Sprengt man nun zum Behuf klarerer Einsicht die Deckknochen auf der betreffenden Schädelhafte volkommen ab, so wird man ein weissliches, walzenförmiges Organ gewahr, wel-

ches, wei bei Cœcilia, ganz vom Maxillarbein oder wei bei Epicrium und Siphonops an seiner äusseren circumferenz nur von der äusseren Haut bedeckt ist." Thus it is seen that the location of the organ in Amphiuma is very similar to its location in Gymnophiona.

A further corroboration of my views is noticed in the relation of the branches of the ramus maxillaris to the external sheath of the tentacle. According to Wiedersheim, in the Gymnophiona three branches of the maxillary nerve attend the tentacular apparatus in its course in the sub-orbital region. In Amphiuma I have found these three branches occupying the same relative position as is indicated by mx* in Fig. I. This striking similarity is seen at a glance by comparing fig. 54 in Wiedersheim's Anatomie der Gymnophionen with Fig. I. Before one can be convinced that the so-called tentacular apparatus in Amphiuma is really such I am aware my investigations must be verified by the discovery of this atrophied organ in other specimens. The importance of the discovery of such a feature is emphasized by Kingsley: "Were it true that Amphiuma possesses, either in the young or the adult, rudiments of a tentacular apparatus, the fact would prove of great value to those who would recognize in the Gymnophiona only degenerate Amphiumæ." Cope and the Sarasins have deduced considerable evidence favoring the close relationship of Amphiumidæ and Cœciliidæ, which fact renders it the more credible that a rudimentary tentacular apparatus has really been found in Amphiuma.—ALVIN DAVISON, PH. D.

PSYCHOLOGY.

Synæsthesia and Synopsia.—Until quite recently synæsthesia was regarded by psychologists generally as a purely artificial and fanciful association, or at best as a sign of degeneracy; it has lately received considerable attention, however, and the weight of evidence goes to show that it is both natural and normal—it may even be said, a phenomenon of common occurrence.

In an exhaustive monograph on the subject, published in 1893,[1] Prof. Flournoy of Geneva for the first time introduced a terminology which aimed to distinguish scientifically between the different forms of synæsthesia. The most important phase is the association of visual images, or *synopsia*. Attention was first called to this by Fechner, in 1876.

[1] Les phénomènes de la synopsie (audition colorée); by Th. Flournoy; Paris, 1893; pp. 259.

Flournoy distinguishes here between photisms, diagrams and personification. The first of these is the *audition colorée* of earlier writers; it consists in the natural association of a color with each particular sound, so that a spoken word appears to the hearer to be tinged with one or more hues, corresponding to its constituent vowel sounds. A diagram is a visual scheme in which some natural series of ideas (such as the months, days of the week, numbers, etc), is arranged. When a member of the series is recalled, the appropriate part of the diagram is visualized. Personification is simply the attributing of some personal characteristic, such as sex, to a number, etc.; or the association with it of a feeling of like or dislike. Flournoy reports some 350 persons as possessing synopsia in one or other of its forms, out of 2600 to whom questions were addressed, (13 per cent.); but as a large portion of his question-sheets were never returned, the real percentage may be regarded as somewhat greater.

In a recent paper,[2] Miss Calkins gives the results of a personal canvass of Wellesley students in 1893 and 1894. For the former year the affirmative answers numbered 33 per cent., for the latter 60 per cent. It may be doubted whether all the latter are true cases of synopsia. Yet when due allowance is made for possible temporary associations, it must still be admitted that synopsia is by no means a rare phenomenon.

Richard Hennig[3] gives an interesting study of the diagram-forms occurring in himself and his immediate family. He is able in a number of cases to trace their origin to certain associations of early childhood, and favors the 'natural,' or experiential view of the origin of all such schemes. He strongly opposes the notion of inherited forms or photisms: Only two pair in the list given by Galton, he thinks, show any real resemblance, and these may well be accounted for by similarity of early environment. " Only the tendency to synopsia can be inherited; but here the influence of heredity is unmistakable and undoubted." The writer points out a similarity between the number-form of himself and one brother brought up under the same surroundings, while in the case of another brother, whose early life was spent in another environment, the diagram was radically different. Herr Hennig urges the usefulness of the number-form as a mnemonic aid, and cites the case of a friend, who easily memorized dates by association with the appropriate places in his number-diagram.

J. Philippe has lately investigated the synopsia of blind persons, and finds a remarkable number of cases among them, though none occurred among those who were blind from birth.

[2] Synæsthesia, by Mary W. Calkins; Amer. Journalof Psychology, VII, 90–107.
[3] Ztschr. f. Psychol., X, 183–222.

With the reduction of synæsthesia to a scientific basis, which Flournoy has brought about, and the demonstration of its wide-spread occurrence, comes the demand for a more thorough examination of its bearing upon other departments of psychology. The physiological interpretation of synopsia is still unsettled, and is commended to physiological psychologists as a fruitful theme for investigation.—H. C. WARREN.

ANTHROPOLOGY.[1]

Exploration by the University of Pennsylvania in West Florida.—Little more than a year ago my friend Lieutenant Colonel C. D. Durnford formerly of the English Army, returning northward from a journey in the West Indies and Florida brought with him the specimens of aboriginal rope and netting found in a mud bed near Marco, Florida described by him in the AMERICAN NATURALIST for November, 1895.

That he realized the importance of the digging done in the mud in April, 1895 by himself and Mr. Charles Wilkins of Rochester, New York, was shown by the fact that on reaching Philadelphia he made the effort at once to present the details of the discovery to archæologists. As an original observer, a gatherer of inspiration from nature, coming generously to present us with unprecedented specimens and archæological data of much value, discribing to myself and others the details of the discovery and stating his belief that the lagoon fringing islands near Marco were net-worked with artificial canals, and would disclose other and similar relic preserving mud deposits, to him belongs the honor of opening a new door for archæology in the southeast.

The prompt recognition of the originality and value of this intelligence by Dr. William Pepper and his energetic action in cooperation with Mr. Stewart Culin, Director of the Department of Archæology have resulted in the recent expedition of the University of Pennsylvania sent by Dr. Pepper to Florida in the late months, under the direction of Mr. Frank Hamilton Cushing, whose fortunate presence in Philadelphia at the time of Colonel Durnford's visit ended in his employment by Dr. Pepper as Conductor of the Exploration. This led to the association of the Bureau of Ethnology of Washington of which

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

Mr. Cushing is a member, with the work whose results have delighted the friends of the University.

Summarized by Mr. Cushing in two newspapers (Philadelphia Times and New York Journal Sunday, June 21st, 1896) these results are represented by the array of specimens now in the Pepper Labora tory at Philadelphia. They witness the good fortune of Dr. Pepper and the University and the successful excavation of Mr. Cushing. The muck-filled artificial shell basin at or near where Coloned Durnford had worked, dammed, baled and cleaned out, and a large mound excavated 200 miles to the northward procured a superabundance of beautiful and unique remains.

The work shows that a storehouse of aboriginal manufactures escaping the notice of a good deal of reconnaissance, had lain unobserved within easy reach of scientific institutions in the east, testifying further to the fact that mud or permanent damp has here done for the Archæologist what permanent dryness has done at the Cliff Dwellings of Arizona and in Egypt. As at the Swiss Lake Dwellings here again, a whole category of remains that have perished elsewhere in the eastern United States have survived hermetically sealed in the ooze.

A few of the salient features of the collection concern:

(1) Facts relating to burial; crania from the mound and muck with funeral paraphrenalia.

(2) The relation of pottery, found in great abundance, to burial, and the allegoric and religious significance of fictile designs.

(3) The use of totemic ornaments, of masks representing the human face in ceremonials, and the allegorical significance of carvings representing the heads of animals, and paintings on wood.

(4) The economic facts of daily life illustrated by means of well prepreserved utensils and vessels of wood and by the haftings of wood and shell implements.

(6) Interesting data refeiring to the arrangement of canals, shell walls, basins, the height of shell mounds and what appear to be vestiges of pile-built houses sunken in mud and sufficiently indicated for study.

It will not be easy for the archæologist suddenly confronted by this display of aboriginal handiwork outshining the long toiled for gatherings of other searchers in the East, to hold fast to the caution that the occasion demands, to realize how much and how little such preservation of perishable remains signifies in a given case, to remember in the inferred estimate of cultural status that multitudes of similar objects, betokening the life history of other tribes in the eastern United States have perished, in short to weigh considerations that must temper the use of

colored words signifying degree of ethnic importance, advanced methods of construction, superiority in the arts, and kinship to other peoples.

Meanwhile the excavation and production of the strange carvings in wood, the human masks, the unique paintings, the hafts of wood and tools of shell, the relics of rope and fabric, remain in evidence to speak in manifold praise of the enthusiastic searcher who while telling his glowing story has shown that he has known where to dig and dug with effect.

Symbols inscribed upon the drawings of birds, totemic buttons arrangements for burial with reference to the "four quarters of the world," the paraphrenalia of priests buried together in the mud here seek explanation at the hands of an interpreter, whose experience should have qualified him for the task. Luckily the elucidation of the allegorical meaning of the serpent and the raccoon, the gopher and the bat, the badger and the cormorant, tokens of gods of the dead and the living of the morn and the dusk, has fallen to the lot of one whose knowledge of the mystic inner life of the Indian, gathered upon a painful path of Zuni initiation might best recognize in the manifold characters of these remains a symbolism hidden to other eyes.—HENRY C. MERCER.

SCIENTIFIC NEWS.

The International Geological Congress will hold its seventh session, in 1897, at St. Petersburg, Russia. The presidential chair on that occasion will be occupied by M. A. Karpinsky. A number of interesting excursions have been planned to take place both before and after the meeting. It is proposed to visit Finland and the Ural country, to examine the basins of the Don, the Volga and the Dneiper. While the grand tour at the close of the Session covers the ground from St. Petersburg to the Caucasus, giving opportunity for special examinatlon of many interesting localities.

The circular of announcement gives the following information in the closing paragraph.

"The Committee on Organization takes pleasure in making known to you that His Majesty the Emperor, upon the report of his Excellency the Minister of Ways of Communication (Transportation) has deigned to grant to all the geologists (who give notice in time of their

intention to take part in the work of the Congress) tickets allowing them free first class transportation on all Russian railways before and after the Meeting of the Congress, including the excursions."

Lord Lilford, the President of the British Ornithologists' Union, died June 17, 1896. At the time of his death he was engaged in a work on the Birds of the British Islands which was nearly completed. He was a contributor to Ibis, The Zoologist and the Proceedings of the London Zoological Society. His interest in natural history led to his keeping an extensive collection of living animals at his country seat in Northhamptonshire.

Mr. T. D. A. Cockerell, Las Cruces, New Mexico, will be glad to furnish information concerning the biological station he proposes to establish in New Mexico. If a sufficient number of students are enrolled, a beginning will be made this summer. For the study of nsect life New Mexico presents an unusual combination of advantages.

The prizes awarded by the London Geological Society have been distributed as follows: The Wallaston Medal to Dr. Edward Suess, Ph. D. Prof. of Geology in the University of Vienna; Wollaston Donation Fund to Alfred Harker, M. A. of the Geological Survey of Scotland; The Murchison Medal to T. Mellard Reade, Esq.; Murchison Geological Fund to Philip Lake, Esq.; The Lyell Medal to Arthur Smith Woodward, Esq.; Lyell Geological Fund to Dr. Wm. Fraser Hume, Demonstrator of Geology in the Royal College of Science and Charles W. Andrews, Esq.; The Barlow-Jameson Fund to Joseph Wright, Esq. and Mr. John Storrie of Cardiff. (Quart. Journ. Geol. Soc. London, 1896.

Messrs. Hatcher and Peterson have gone to Patagonia to collect fossil vertebrata in the Cenozoic beds of Patagonia for Princeton University.

Macmillann & Co. have made arrangements for the issue in New York and London of a "Dictionary of Philosophy and Psychology" under the editorial supervision of Professor Baldwin of Princeton University.

The following assignments of topics with the names of the authorities who will contribute original matter may be already announced:

General Philosophy and Metaphysics.—Prof. Andrew Seth, Edinburgh University; Prof. John Dewey, Chicago University. *History of Philosophy.*—Prof. Josiah Royce, Harvard University. *Logic.*—Prof. R. Adamson, Glasgow University. *Ethics.*—Prof. W. R. Sorley, Aber-

deen University. *Psychology.*—Prof. J. Mck. Cattell, Columbia University; G. F. Stout, W. E. Johnson, Cambridge University; Prof. E. B. Titchener, Cornell University; The Editor, Princeton University *Mental Pathology and Anthropology.*—Prof. Joseph Jastrow, Wisconsin University. *Biology.*—Prof. Lloyd Morgan, University College, Bristol. *Bibliography.*—Dr. Benjamin Rann, Harvard University.

With the publication of No. II, Vol. II, of its bulletins, the Chicago Academy of Sciences enters upon a new era of activity. Its publications will be issued at regular intervals. The Academy property is now housed in a fire proof building of the best architectural construction, and no further fears of fire are entertained.

Dr. Joseph F. James begs to inform his friends and correspondents that he has removed from Washington, D. C., and that after May 10, 1896, his address will be Hingham, Mass.

I desire to secure good sets, cleaned or uncleaned, numbering fifteen or more specimens each, of your local representatives of *Campeloma* (*Melantho* of Authors), *Lioplax* aud *Vivipara.* Where extra large sets can be sent they will be of especial value since the present object is monographic. Exchanges are offered in southern *Unionidæ* and *Strepomatidæ.* The rarer forms of the last named groups are also desired.

Cincinnati, Very respectfully

1815 Fairfax Ave. R. Ellsworth Call.

THE

AMERICAN NATURALIST

Vol. XXX. September, 1896. 357

PROF. BALDWIN'S "NEW FACTOR IN EVOLUTION."[1]

By Herbert Nichols.

That the pendulum of opinion swung too violently against the conception that mind is an active factor in Evolution I count the major misfortune of the modern epoch of Science. That there is now a return of interest I esteem to be the most important outlook of our day. That this return of interest centres in Psychology is inevitable. If now this new movement should become abortive through any false lead of Psychology the result would be deplorable.

It is with anxiety, therefore, that I read the numerous writings of Prof. J. Mark Baldwin upon the rôle played by mind in Evolution (see above Reprint for complete list). The prolific earnestness of this author, together with his conspicuous position as professor at Princeton and Alternate Editor of The Psycological Review, give unusual prominence to his views. Yet these views, as I believe, are precisely of the kind which we have most to dread. It is in this belief that I am prompted to the analysis of them which I here propose. And as Prof. Baldwin has no more enthusiastic admirer of his sincerity and

[1] Reprinted from The American Naturalist, June and July, 1896.

zeal, so I beg him to permit me to point out the more freely the objections to his main assumption.

In Professor Baldwin's latest paper, above referred to, he has "gathered into one sketch" an outline of his theory. In this pamphlet, as in all else that he has written on this subject, we are presented with a vast pyramid standing on its apex. We are told how he conceives Evolution to work under his assumption, and gradually his story narrows toward an explicit statement of what this assumption is. Unfortunately, however, the vast superstructure closes in to a cloud of mist, and does so, alas, not only before he has made clear in exact detail what his assumption is, but even before making understood how the things he vaguely suggests could ever clearly be conceived to be possible.

The gist of Mr. Baldwin's notion is that Pleasure-Pain is a psychic "factor" that crucially determines Evolution. Pleasure results from beneficial stimulus. It causes, in turn, "excessive" neural discharge. Neural discharge causes "expansion." Expansion brings the creature into continued subservience to the beneficial stimulus. Excessive neural discharge makes the paths of actual discharge more pervious to the continued stimulus and to subsequent discharges from the same source. Thus a "Circular Reaction" becomes fixed which, because it is beneficial, conduces to the preservation at once of the peculiar habit and variation in the organisms so developed, and also of the creature in which it is developed. The antithesis of all this happens with pain.

Now for the difficulties; and to bring them out let us imagine an unorganized creature before us—say an amœba. Our problem is to find how it becomes organized. Let us imagine it attacked by any given stimulus at some point of its periphery. Mr. Baldwin tell us that if this stimulus is beneficial it will give pleasure, and the pleasure will cause "excess movements." Mr. Baldwin does not pretend that these are yet organized movements. To do so would be to beg his whole question. Yet he claims that this unorganized movement would complete his "Circular Reaction" with the beneficial stimulus and perpetuate the beneficient work. *But how can we conceive*

that unorganized movement, or movement in the abstract, should do such an organized act as to select beneficial stimuli and avoid those which are detrimental? Especially how shall this be done after Mr. Baldwin has carefully laid it down than there can be no such thing as benefit or detriment in a mere muscular movement in and of itself?[2] Of course Mr. Baldwin knows that various propositions have been suggested by different physiologists to explain why an undifferentiated creature like an amoeba, puts forth pseudopodia and makes definite prehensile movements in response to certain stimuli; and makes definite revulsions in response to others. But if so he is aware that all these propositions are based upon some purely physical relationship of the different stimuli to the protoplasmic substance, whereby some act in one way and others in a reverse manner. All such movements are definite and concrete and can be perfectly understood. But how *mere movement in the abstract* should be able to select that sort of nutriment which is beneficial and to avoid those forces which are harmful is surely above human power to conceive—unless, perhaps, Mr. Baldwin can explicitly describe to us how it is to be conceived. To assume outright that the movements resulting from pleasure would locomote intelligently toward proper nutriment, or do aught differently than the same movements caused in any other way, is simply to leap the whole problem by one absolutely unbounded bald assumption. Than this it is more respectable to say that Ormozd takes the kitten by the neck and chucks it bodily to the saucer.

But, perhaps, Mr. Baldwin merely means that the excess movement would work to continue the contact with the original stimulus already made. If so, then must we contend that absolute quiescence would most conduce to the preservation of a contact already made, and incoordinate wiggling would be the thing in the world most likely to break the contact, and to drive the creature away from the beneficial stimulus.

Mr. Baldwin's assumption that excess movements, however caused, would be any more likely, in the abstract, to secure circular reactions among beneficial stimuli than among detri-

[2] Mental Development, p. 189.

mental ones is, therefore, wholly false. All would depend on the prevalence of one or the other sort of conditions. If dangers most abounded the creature would be all the more quickly destroyed by his excess locomotion. If benefits abounded then the creature would prosper because of that fact, but not because of any power of muscular tissue to select these benefits, save that be by its physical properties—i. e., the same which are being studied by the physiologists as before mentioned.

Thus falls the king-bolt in Mr. Baldwin's "circular reactions." But falling back upon the second link it does seem at first sight that advantage should be secured to a creature by a "new factor," which should have the power of saying *when* the creature should act and when not; and that had the intelligence to decide that the creature should move only when in the presence of beneficial stimuli and not move in response to detrimental ones. But here again there is a snare and delusion, and just where it was least to be expected. For it is just as likely as not that *to move* would be the most beneficial thing in the world under attack of detrimental forces—for instance, to get away from them; or that to move under beneficial conditions would be the most detrimental thing in the world—for example, would wiggle the creature away from a newly secured morsel of food. In short, so long as it remains true, as shown in our last paragraph, that abstract movement is equally likely to do harm or good, so also must it remain true, that even a "new factor," with the power attributed to it by Mr. Baldwin, could not by any possibility favor the organism by the means described. How should it by the exercise of a power which in itself is alike blind to good or ill?

Thus falls the main swivel in Mr. Baldwin's chain of reactions, and falls at a simple touch. But lest it seem to fall too easily in proportion to the mighty and world-deciding destiny asserted of it, let us pursue it further and in more detail. Thoroughly to dispose of an error we must see how and why it was made. The doctrine of pleasure, of which Mr. Baldwin's "excess discharge" is the attempted physiological expression, dates back to Aristotle. Aristotle declared that pleasure ac-

companies perfect use of our faculties, and pain their impeded use. The philosophy which prevailed after Aristotle was dominated by the Oriental superstition that the forces of this world are divided between the Powers of Good and of Evil. How this superstition seized upon and biased the dogmas of our theologic ancestors until belief in a personal Devil was universal among even the learned in the middle ages, is a matter of undisputed history. Aristotle's doctrine fitted well with this superstition, and his unquestioned authority enforced its universal acceptance. Thus, as late as 1647, we have Descartes, the highest authority of his age, declaring that "All our pleasure is nothing more than the consciousness of some one or other of our perfections." When Science dawned, and began basing mental activities upon correspondent neural processes, nothing was easier or more inevitable than that the doctrine which always had been conceded to express general conditions of welfare and activity should be transferred to general conditions of the nervous system; and that, in general, "heighted neural discharge" should be declared to be the basis of pleasure, and the reverse to be the basis of pain. Thus, an early conjecture of Aristotle, fostered by one of the grossest theological superstitions, and transformed, as I shall show, by most uncritical and fallacious physiological assumptions, is the historic origin of what Prof. Baldwin calls "A New Factor in Evolution.[3]

The origin of the notion having been accounted for independently of any critical regard of the facts, we will now examine it in the light of the facts. We have no means of examining neural discharges directly, or independently of their stimuli, their sensory effects, and their motor results; we have no other means of measuring them, except through analogy with the strength of these. In general it is fundamentally observed that where the stimulus is intense the sensation is intense. Also, muscular reaction is proportional to the stimulus and to the sensation. Every known fact, outside of the phenomina of pain and pleasure in dispute, conforms to the in-

[3] Whether it is with reference to Spencer, Bain, Descartes or Aristotle, that this factor is "new," Prof. Baldwin does not state.

ference that the stimulus, the neural discharge, the physic counterpart, and the motor result, rise and fall together. Beginning now with the motor reactions of pain, it is to be observed that they are among the strongest and most violent of which we are capable; the violent struggles that every creature makes to free himself from pain, or that he displays, reflexly, in the convulsions of its torture, are among the most familiar facts known. Again, it is equally well known, that the stimuli which cause pain are the most violent that we encounter; usually it is for that reason that they are detrimental. Also, pain is the strongest and most violent of our sensations. When, therefore, all the evidences alike, from every common source of observation, agree that the neural discharge *ought* to be strong proportionally as the stimulus, the sensation, and the motor reactions are strong, it would seem that we ought to conclude that the neural discharges of pain are strong.

Surely we ought so to conclude, unless Prof. Baldwin has further evidence to offer. The evidence most likely for him to offer is that pain is characteristic of exhaustion, weakness, disorder and disease. This is the stronghold of the traditional school, and has been the secret of its fallacy from its beginning. Yet, there is not a single one of these phenomena that is not perfectly explained without accepting the tradition, and without any of the violations of fundamental analogies which its acceptance necessitates. This is done upon the basis of specific pain-nerves. Every analogy demands that there should be such nerves. If all other sensations have specific nerves so should pain. They have long been anticipated in physiology. And recently they have been demonstrated with surprisingly wide-founded and abundant evidence;[4] quite equal indeed to that for the nerves of touch.

Necessarily the universal distribution of these nerves brings them into close connection with the vaso-motor mechanism. Wherever there is unusual congestion of the blood there is

[4] See article in Brain, p. 1, 1893, and p. 339, 1894, by Dr. Henry Head of University College Hospital, London. Also those by Prof. von Frey in Berichte d. math. phys. classe d. Königl. Sachs. Gesellschaft der Wissenschaft zu Leipzig, 1894, pp. 185 and 283; 1895, p. 166.

likely to be pain. We are not certain what the appropriate form of stimulus is for the pain-nerves, but assuming it to be mechanical pressure, then any unusual stretching or tension, whether in the capillaries or the surrounding tissues, as caused by congestion, or from undue secretion of any of the glands, or from any other disorder, would perfectly explain the attendance of pain. That this should explain the characteristic pains of exhaustion, weakness, disease, and all other abnormalities, rather than the mere loss of general bodily strength, to which the common tradition more directly attributes them, no scientist should doubt. For, first, there is no evidence that mere weakness, independently of the physiological derangements which are the co-results of *its* cause, are at all painful. A man may bleed to death, and suffer no pain. Again, a frail invalid may fade away with weakness, and suffer no trace of pain; indeed, may depart with gladness. Or a sprinter may drop with exhaustion and, perhaps, suffer no pain at all; or if any, none save what is unmistakably due to the abnormal disturbances of circulation already referred to. Secondly, all causes of weakness are likely to produce disorders which, in turn, shall produce disturbances likely to excite the pain-nerves in the way above indicated. This is so evident that it need not be discussed. Third, when so excited, even during general bodily weakness, there is still every evidence that the pain discharges are characteristically strong above other nervous activities, and relatively so proportionally to the lowering of the general level of strength. It would seem, therefore, that every known phenomena of pain, on the one hand, receives perfect explanation on the basis of pain-nerves, that every analogy demands such nerves, and that finally they have been conclusively demonstrated. And, on the other hand, it is strikingly manifest that every evidence we possess flatly contradicts the assumption that pain discharges are feeble.

The corresponding assumption that the neural discharge of pleasure is "excessive" equally fails of corroboration when confronted with the facts. Here, again, we can measure the discharge only by its psychic accompaniment, its stimulus, and its motor effect. That pleasures, among psychic states, are charac-

teristically intense, is not true. Again, that intensity of stimulus is not a uniform determinant of pleasure is one of the best known truths of every form of art. And that the motor effects of pleasure are not conspicuous for violence is not less well known. Some of them are violent, no doubt, yet abundance of others are among the most soothing and quieting influences which we experience. The entire field of pleasure therefore —source, centre, and motor discharge—is one endless contradiction of the assumption that its neural discharge is predominately intense, and points even to a new *definition* of pleasure from that of which the traditional school is possessed. Again, it is the delusive general relationship of pleasure to health, strength and welfare which has ever been the source of error. With health and freshness all functions, undoubtedly, are more vigorous, and those which give pleasure are more active among the rest. Also, in health we are freer of unpleasant disorders. Yet it remains true that the feeblest invalid is often capable of the intensest pleasure, and that the trained athlete may suffer excruciating pain if the dentist but tickle the bare nerve of his tooth with a feather.

Against the "discharge" link, pleasurable or painful, in Mr. Baldwin's "Circular Reaction," it would seem unnecessary to push the sword further. In has absolutely no foundation in fact. Yet, as this is of a class of tradition that dies hard, I will bring yet multiplied objections against it. When a child first brings its finger into contact with a flame it instinctively draws its arm away: a complicated and delicately articulated mechanism has been evolved by nature, and inherited by the child for this purpose. The case is typical, and other examples are innumerable. *Now, under Mr. Baldwin's Plan of Evolution, it would have been impossible for such an organized response to pain to have developed.* His whole scheme is one wherein "the excess discharges" of pleasure conduce to the development of organized responses to pleasure, and the "restricted discharges" of pain *specially prevent the development of organized responses to pain.* It is true that Mr. Baldwin expressly declares his "New Factor" to be ontogenic. Still, if so, then pain restrictions must have yet worked from the moment of each creatures

birth to stamp out every provision of the type above cited. Over and above this, *every intelligent organization against detrimental forces would be impossible from the moment of birth.*

This is no small obstacle to the universal acceptance of Mr. Baldwin's "New Factor," yet the more intimately we approach it the more do the difficulties increase. This time for a bull's-eye example we will take a plunge straight at the "pain-pleasure discharge" itself. Mr. Baldwin tells us it is "central"—let us now ask to what is it proportional? What gauges its "heightening" or its "restriction?" The pain or the pleasure, of course, Mr. Baldwin answers, since his "New Factor" is a psychic factor. But to which is the pain or pleasure proportionate—*the incoming sensory nerve current,* or the "*benefit from the external stimulus?*" It is just here that a "tremendous" (to use a favorite word of this enthusiastic writer) stumbling-block arises. Mr. Baldwin tells us with emphasis that the pleasure comes in and by the stimulus. But how and in what manner does the external pleasure-stimulus connect with the centrally rising "heightened discharge"? Plainly it cannot be through the mere intensity of the ordinary incoming sensory nerve-current; for the pleasure is proportional to the benefits from the external stimulus; and these benefits are by no means proportional to the intensity of the stimulus. But, perhaps, Mr. Baldwin conceives—he does not tell us here in the least what he does conceive, though it is an absolutely essential point—of some *specific kind* or *mode* of neural activity to convey his pleasure-stimulus from the periphery to the centre, and one in no way parallel to the intensity of the external stimulus. If so, then a still greater difficulty now arises to conceive how the "benefit" or the "detriment" from the external event expresses itself through this new mode of communication. We are told that the pleasure is proportional to the *amount* of the benefit worked by the stimulus, not to its intensity. But just how and when does this "amount" get transformed into this new kind of ingoing pleasure current? Benefit is a "tremendously" abstract affair. Where does it end, and when does it act? The benefit does not happen instantly—when then is its pleasure experienced? How and

when does it sum itself up with reference to the heightened motor discharge? For this last, we had supposed, resulted immediately upon the arrival of the sensory impulse at the brain, and cannot be permitted a long delay if it is to join in "Circular Reaction" with the passing stimulus.

Surely here is a puzzle! Let us endeavor to follow a concrete example; and again it shall be Mr. Baldwin's own, wherein he explicitly describes the sort of betterment that gives pleasure and "heightened discharge." When the sun shines on a creature its warmth promotes nourishment and other vegetative functions. Let us say now that it heightens digestion from a usual period of two or three hours to one of twenty minutes. *When, then, does the "central discharge" begin to be "heightened" by this betterment in order to complete Mr. Baldwin's "Circular Reaction?" Also, just how does the benefit gather itself together from the bowels to express itself as the pleasure of the original sensation; i. e., the sensation of warmth that came at the beginning of the twenty minutes?* A diagram drawn to scale of these physiological activities, and with their space and time processes accurately portrayed, would facilitate the acceptance of Mr. Baldwin's "New Factor" among scientists generally.

But, of course, all this is doing the utmost of injustice to Mr. Baldwin's "New Factor." For, is it not a psychic factor? And is it not the essence of psychic factors to surmount all lawful relations of space and intensity? How absurb of me to attempt to trace the benefits and detriments of the sun's rays through the viscera to the "heightening" and "restricting" of central discharges! Pleasure and pain, of course, are super-spacial and super-temporal fiats that leap all physical difficulties and bounds. Only why, then, does Mr. Baldwin take the trouble to localize them as *central?* Or why declare them to have any mechanical relationship with motor discharges? It is just here that I must plead it to have been most natural for me to have been mislead to conceiving that the "central" processes of pleasure have some lawful articulation with the incoming sensory impulse, since they are explicitly declared to have both temporal and spatial articulation with the outgoing motor discharges. But, perhaps, Mr. Bald-

win's vagueness and confusion of statement and *longing* to be scientific may here have got the best of these outgoing articulations—perhaps, they do not and could not work according to any known axioms of science even here! Let us examine this.

Upon close consideration it becomes obvious that Mr. Baldwin's " New Factor " not only interrupts all normal relations of intensity between incoming stimuli and outgoing discharges —so that feeble stimuli, if beneficial, now produce " heightened discharges," and violent stimuli, if detrimental, are " tempered to the shorn lamb"—but also it wholly *transforms* their mechanical effects. Not only now do pleasures produce " expansion " and pains " restrictions," but violent pleasure-discharges produce violent expansions, and violent pain-discharges produce violent contractions. Or, at least, I suppose they do; though here is the very pesky plague of it, to know what Mr. Baldwin does conceive to happen. For, if now the " discharges " do thus cause literal bodily expansion or contraction, in due accord with their intensity, it is impossible to conceive what " heightening " or " restricting " has to do with the case. And, on the other hand, if " expansion" refers to " degree of activity," and " restriction " means " quiescence," I give up trying to understand the matter, and plead insanity and hallucination at once; for then the innumerable acts which seem to be performed before my eyes, both expressively and preventatively of pain must be " restricted " absolutely, and by no possible means actually can happen; and the cause of my derangement in conceiving that they do is surely sprung from my overwrought sympathy for all physiologists or psychologists who shall attempt to measure the amount of restriction necessary to be applied to each varying intensity of incoming detrimental stimulus in order to reduce it to a constantly maintained zero of quiescence, and not have the least little bit over to set the creature wiggling right up to its detrimental persecutor, and perpetuate " Circular Reaction " therewith, just as if it were beneficial.[5]

[5] The physiological cult of the traditional Pain-Pleasure School juggle much with this " heighted and restricted central discharge," as if it were ordinary neural activity. But when all obfuscation is nailed to the board it becomes plain that, since the muscular effects come after the motor nerve-currents are formed,

But, seriously, there are a few things that we must conclude regarding Mr. Baldwin's "New Factor," if we are to pay to it any logical regard whatever: (1) The work assumed of it is not one of simple heightening and restricting, but one of absolute interruption, transformation and reversal of natural consequences. (2) These interruptions, transformations and reversals proceed by no known axioms or measurements of science, and as little so in their articulations with the motor apparatus as with the disseminated benefits and detriments from the external forces. (3) There are no central neural processes correspondent to these alleged activities of pain and pleasure. There are no facts which suggest them; no physical activities could behave in such disregard of physical laws; and to assert them as acting by such laws would either duplicate the "New Factor" as an efficient cause, or else reduce pain and pleasure to ordinary non-interfering parallelism; which is a flat contradiction to Mr. Baldwin's entire proposition. (4) It is absurb to locate this New Factor as "central." For a factor that transends all physical laws of space, time and intensity cannot be located in this physical world. (5) And, finally, if such a "New Factor" existed, any exact determination in physiology and in psychology would be futile. Whereas the psychic factor of Prof. James is a wee and comparatively inoffensive affair, which only tips a molecule here or turns a current there, just a little, and when absolutely needed—and apparently from the remainder of his system is never needed—on the contrary, this Factor of Mr. Baldwin's is the dominant

therefore it is something beside *the mere intensity* of these currents which determines whether the result shall be expansion or restriction. And if it is something different from the intensity of these nerve-currents, then also must it be different from the ordinary intensity of the central neural activity which gives rise to these currents. In which case it is nonsense to talk of "heightening and restricting" precisely as if they were performed by ordinary central activities.

Unmistakably it is no "ordinary" activity that either destroys ordinary intensity regardlessly of all opposing parallelograms of forces, or that upsets the laws of conservation of energy. True, we do not yet understand Inhibition; yet, no scientist thinks of explaining it, except within "*ordinary*" scientific laws. And any force which transforms any incoming sensory nerve-current, however detrimental, into flat quiescence without expenditure of other physical energy in opposition to it, certainly does not act within ordinary scientific laws.

force in evolution above all other forces. It acts upon *every* external stimulus, interrupts and transforms its natural effect, molds and remolds the entire organism and all subsequent species in accord with the non-physical and miraculous power. If such a factor be admitted so dominantly throughout nature then exact science becomes absolutely impossible.

Our examination of this "New Factor" may, therefore, now be summarized as follows. It is obvious, historically, how its ancient traditions, rooting originally in superstition, have survived and come down to be an anomaly in our scientific times. It never had any closer foundation in facts than the superficial observation that pleasure more often comes with health and strength, and pain with weakness and disease. The central neural processes on which it is alleged to be based do not exist. The phenomena in question, upon examination, flatly contradict at every point the assumptions and assertions boldly made of them. The alleged "Factor," if carried out under these assumptions and specifications so made of it, quickly reduces the entire realm of biology and of psychology to endless confusion and ridicule.

On the other hand these phenomena have now been treated of substantially without violating the symmetries of nature, and in accord with the obvious demands and analogies of the remainder of ascertained knowledge. Pain-nerves have been conclusively demonstrated. Pleasure and displeasure, if they have not been so successfully disposed of as bodily pain, have been finally divorced from it and from the tradition that they are "quality activities" of any kind; they are rapidly being driven by new analysis and definitions to where they are seen to be forms or movements of thought quite independent of specific qualitative make-up; are being explained on the same footing and in the same categories with conceptions, volitions and similar mental processes, which apparently may be of any and every "quality," or, at least, in which the qualities of the content play no at present determinable part.

Nor have these things been done in a corner. Modern literature is full of them. These new opinions have been put for-

wardly neither timidly, obscurely, nor by inferior men. Long ago so great an authority as Prof. James declared of the ancient tradition that it was "*one of the most artificial and scholastic untruths which remain to disfigure modern science.*" There would seem, therefore, now to be as little excuse for an intelligent man to believe in this "New Factor" as to continue to believe in the other half of the tradition, *i. e.*, in a personal devil. For a scientist to continue to throw such 'disfiguring untruth' among the already vastly complicated problems of biology and psychology, of heredity, and of social and ethical development, while completely and blindly ignoring the objections which have been heaped, mountain high, against it, cannot henceforth be counted as less than pure Orientalism. To persist in the attempt, with whatever sincerity and enthusiasm of purpose, can only result, as my first words portrayed, in retarding the swing of the pendulum to a more sober consideration by Science of the problems of mind, and in bringing our New Psychology to speedy and undeserved contempt.

It seems hardly worth while to follow Prof. Baldwin into the doctrines of "Imitation" and "Organic Selection" built by him upon his above foundation, when these foundations show themselves to be the veriest myths.

BIRDS OF NEW GUINEA (MISCELLANEOUS).

By G. S. Mead.

(*Continued from page 290.*)

The family of *Certheidæ* (Creepers) have but scant representation in New Guinea, the genus *Climacteris* furnishing the only specimens. One species is perhaps peculiar to the island, viz.: *Climacteris placens.* Its plumage above is dusky, tinged reddish on the head with black marks interspersed. Below grayish, spotted brown and black. Sexes alike. Length, six inches. Salvadori says the female has reddish cheeks.

One Nuthatch also belongs to New Guinea—*Sittella* or *Sitta papuensis*—a very small species, less than five inches in

length. Above, the feathers are brown barred with black; below, the arrangement and shading are similar; head and throat white, as are also the upper tail-coverts. Below they are dark, sometimes obscurely spotted. The tail is black and short. Bill black. Legs yellow. The female differs in the coloring of head and under parts, not always essentially.

Of the Megaluri scarcely more than two or three species are to be found within the boundaries of New Guinea. One of these is *Megalurus macrurus*, from the southeastern portion of the main-land. The bird is eight inches long; dull brown above, or at times brighter and tawny and streaked with black; under parts coarse white and bluff; and long tail, more than half the entire length. From the same region comes another species much smaller in every way, but of more varied coloring. Considerable white marks the little bird—the under parts, cheeks and quills of the wing feathers taking this hue. Above the ground color is a rusty brown, with black streaks and markings on the shoulders and head. Black prevails on the wing-coverts.

Cisticola exilis, with an endless string of synonyms, which it has obtained by its wide distribution and change of plumage, is a very small thrush, varying in length from 3.5 to 4 inches. In Australian form the head seems to be of a more even reddish or rusty hue. Otherwise the general color is plain gray, picked out with black along the neck and upper back. Sometimes the gray is tinged, as along the wings and tail. Around the face is much white and yellow. The under parts are a discolored gray or buff. The females differ in having the head touched with lines of black or deep brown, and generally in a deeper tone. The plumage changes with the seasons.

Among the Bubbling Thrushes the genus *Sericornis* is represented in New Guinea by two or three species. *S. beccarii*, from the Aru Islands, is colored above dusky, ferruginous on the rump and tail-coverts, and black edging on the wings. Some feathers show white points. Much brown appears in certain lights. White in streaks on the face. Throat white, slightly touched with black. The under parts are a discolored white flanked with brown.

S. arfakiana is very similar in general appearance and colorings. An obscure wing-bar may be traced on the brown wings. The head is darker than the back. The throat is ferruginous, the remaining under surface olivaceous. Length, 4.5 inches.

The beautiful family of the *Nectariniidæ* (sun birds), with their slender forms, their curving bills and metallic plumage, is well represented in New Guinea, rather numerically by individuals than by variety of species. *Cinnyris aspasiæ*, known also under an appalling number of synonyms, is black, green or blue, according to shading and locality, besides differing considerably in size. The green variety gives out a green gloss from the burnished surface of the back, while beneath the feathers are velvety black. Other reflections are to be observed in different lights. From the throat escape the loveliest blue tints. The larger form (*C. auriceps*), with its lovely golden-capped head, is a dark blue, and is found in several of the adjacent islands. *C. proserpina*, both small and large, is a black-shouldered form, throwing out green, blue and purple, according to the position of the beholder. All of these are of miniature size, and variants of the same general type.

C. frenata, the Australian yellow-breasted sun bird, with brilliant blue tints on its throat, is abundant in southern New Guinea and elsewhere, as well as at Cape York, where Moseley saw it. This species is yellow below, yellowish-green above. The female lacks the blue throat, but has bright gold instead over the entire under parts, from tail to the bill. It breeds in November and December, constructing a little purse of a nest with the covered entrance near the top. Within are laid the tiny eggs, colored dull green, and mottled with dusky spots. These repose on a soft bed of feathers and silky materials. In defense of his home, as indeed at almost all other times, the male is as belligerent as a humming-bird, attacking and putting to rout any vagrants loitering near. The total length of the bird is 4.5 inches, of which the bill comprises nearly an inch, and the short, narrow tail about the same. The latter member is black, with much white in spots on the outer feathers. Bill and feet black. The great beauty of the male lies in the metallic blue throat.

Of the group of Nectarinædine birds ealled *Arachnathera*, all confined to the Indo-Malayan region, three, if not more species belong to New Guinea exclusively. These are *A. polioptera*, *A. novæ guiniæ* and *A. iliolophus*, all about the same small size, and, owing to this fact, resembling each other to all appearance. The first named, *A. polioptera*, has a steel-blue gray head running into olivaceous, becoming yellowish green along the neck and back. On the wings and tail slate-blue takes the place of green, relieved along the edges by traces of gray or white. The under surface is yellow, retaining, however, the olivaceous tint of the upper parts. On the throat there is the usual changing hues, common to this class of birds. This species lives in the Astrolobe Mountains along with the *A. iliolophus*, although the latter seems more widely spread, being found as well in the southern portions of the great island. A special difference may be pointed out between the two birds, the general color is lighter, that is in the loosened, fluffy plumage of the lower back and sides. In this characteristic *iliolophus* has a marked advantage, the feathers becoming very soft and considerably elongated over the short tail. *Arathnothera novæ guiniæ* is similarly adorned. Its breast, in fact, the under parts generally, is more brightly adorned than the foregoing, being of a brilliant yellow, dashed, however, with olive. In other respects the coloration is nearly the same—olive, olive-brown and brown predominating. There may be in the present case rather more yellow especially about the face.

The Javan Swallow—*Aviundo frontalis*—is only about five inches in total length, measuring from the tip of its tiny bill to either point of the deeply-forked tail. The general color above is dusky, scarcely the usual steely-blue of most swallows, but with a darker shade on the shoulder and crown. The under parts are much lighter, at least in the case of the New Guinea specimens, which are invariably pale; in fact, the abdomen is almost white. On the upper breast and throat a fine rufous tint is very prominent. The tail above is a uniform black; below there is a broad band of white following the triangular form of the fork, but melting at the apex into the wavy white of the coverts. The little bird is by no means con-

fined to Java, but is very widely distributed over the Papua, migrating to far distant tropic lands besides.

Hirundo nigricans or *Petrochelidon nigricans* presents few features of coloration to distinguish it from the various forms of swallows of different denominations so well known in all parts of the world. The present bird wanders widely over the Australian continent and Papua. It is small of size, not much over five inches in total length, although many specimens exceed this measurement, the individual differences being unusually great. Dun rumped, as Latham called this species, fairly characterizes it; but this appearance varies according to age, locality and season. In general color above, dark gleaming blue, faintly marked white lines prevails. Below, the body is buff or whitish, with a dark-hued breast. Wings above are dusky; below a ruddy tinge, tail feathers similar.

Many of the family *Dicæidæ* find a home in New Guinea or its islands. They are all small, usually prettily colored birds, allied at least in appearance to the sun birds, although hardly as elegant of form or richly plumaged as those delicate denizens of the tropics.

Dicœum rubrocoronatum abound in southern New Guinea, especially near Port Moresby. It is a showy little creature, a trifle over three inches in length only. Above the color is blue-violet, becoming less distinct on the neck, and merging into rusty on the wings. The tail continues this deep blue of the body; but over the rump and crown of the head a bright scarlet is thrown. This reappears in a broad segment on the upper breast. The female lacks this conspicuous ornament altogether, while the scarlet elsewhere becomes merely dull red. In other respects she is colored like her mate, having the under parts pale yellow, olive and white. She is, however, unmarked by a pinkish tinge on the under tail-coverts, which adorns the male bird. In both the middle abdomen and throat are a buffish white. Bill and feet dark.

Scarcely to be distinguished from the preceding is *D. pulchrius*, who differs chiefly in the coloration of the under tail-coverts, which, instead of pink, are a yellowish-brown. In lieu of this deficiency *pulchrius* has been granted a slightly

larger expanse of scarlet on the head and neck. The habitat is the southeast.

Another species from southeast New Guinea, along the Fly River, differs from the first named chiefly in having a glossy black upper surface instead of blue. It is known as *D. rubrigulare.*

Smaller and more plainly colored is *D. pectorale,* whose leading tints are olive-green above, yellow on head and rump, no scarlet whatever excepting on the upper breast, the remaining under parts and tail-coverts light yellow, exclusive of the whitish under wing-coverts.

From the Bay of Gielvink comes *D. gielvinkianum* or *mafoorense,* of an olive color above glossed with steel-blue. Here again a shade of red appears on the crown, rump, upper tail-coverts and breast. The under surface is a yellowish-white bordered along the sides with olivaceous. A more brightly tinted variety is named *D. jobiense.*

The genus *Oreocharis* of the Dicæidæ, represented by one species, is peculiar to New Guinea; this is *Oreocharis arfaki,* collected by Mr. Goldie in the Astralobe Mountains. This is a larger bird by two inches than those of an allied kind just considered. The color above is dissimilar, viz.: an olivaceous, somewhat glossed. The dusky wings, however, are touched with green and yellow on some of the feathers. So, too, the tail above. Crown of the head and sides glossy black, melting into bottle-green on the neck. About the eye are dashes of the brightest corn-yellow. This is the color also of all the under parts, excepting the black throat. The under wings are paler, with black touches. A reddish stripe may be seen in the gold ground of the under parts.

Urocharis longicauda is likewise the sole species of the genus *Urocharis,* and occupies the same region of the Arfak Mountains. Above the general color is a shining black, the only exceptions being the rump, which is gray, and the tail, where on the outer feathers a long spot is visible. The side face is olivaceous; this is the color on the under body mingled with pale yellow. The female is larger by more than half an inch, and is a smooth olive-green. Length, about five inches. The tail nearly half this figure.

The genus *Melanocharis* comprises four species, all from New Guinea and its islands. These are not very dissimilar in size or coloration. The best known, well named *M. nigra*, is a glossy black above of a bluish cast. Beneath the principal tint is olivaceous, passing into pale yellow on the abdomen. The under wing-coverts are white. Total length, nearly five inches.

Another genus of the same family, consisting of but a single species, is *Pristorhamphus veroteri*. A larger bird this by an inch, with rich, velvety black plumage above, emanating pale green. Underneath a bluish tint. Besides, some spots of white on the tail, apparent when the bird is flying, but concealed at other times; there are white plumes, very soft and delicate, waving on either flank. The female is equipped with these same adornments, but is of dimmer coloring, mainly olivaceous. Habitat, the Arfak Mountains.

Less by more than an inch is *Rhamphocharis crassirostris*, the sole member of its genus. An olive-green bird above with dusky brown wing- and tail-coverts, and blackish tail. Below the body is a pearl-gray with a yellow wash. The female is of larger size, olive-brown above, but differing from the male in being rather more varied in neutral colors, yellow and white spots or dots appearing on the dull surface of wings, tail and back. The under parts are of a soiled white, specked with yellow and brown. The bill is not noticeably larger than that of other species.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

BY ERWIN F. SMITH.

II.

I. THE BEET (BETA VULGARIS L.).

1. THE BACTERIOSIS OF FODDER BEETS (1891).

(I) THE DISEASE.

(1) *Author, Title of Paper, Place of Publication, etc.*—This disease was first described by Dr. Ernst Kramer, Privat Docent

in the technical high school in Graz. His paper entitled (**23**) Die Bacteriosis der Runkelrübe (*Beta vulgaris* L.), eine neue Krankheit derselben, was published in *Oesterreichisches Landwirtschaftliches Centralblatt*, Jahrg. I, Heft 2, pp. 30 to 36, and Heft 3, pp. 40 to 41. Graz, 1891.

(2) *Geographical Distribution.*—The disease prevailed extensively in 1890 in the Eltzischen earldom in Vukovar, Slavonia.

(3) *Symptoms.*—The beet roots were said to be shrivelled and to contain comparatively little sap. The whole of the affected roots began to change to dark brown soon after harvest. On cutting them open dark brown spots were visible. During the winter the disease spread in the beet cellars to apparently sound beets, in spite of the fact that all roots showing any signs of disease were thrown out and destroyed at the time of storage. Roots in which the disease was well advanced showed a gummy ooze which appeared to be infectious to sound beets. Cattle fed with slightly infected roots were attacked with severe bloating and obstinate constipation, and in one case death ensued. Such was the account forwarded to Dr. Kramer along with samples of the diseased beets. There is no record of the symptoms of this disease as it occurs in the field. The diseased beets received by Dr. Kramer were shrunken and in some places were soft under the epidermis. From these soft places there oozed a slimy brownish fluid, which stuck to the fingers, but was without characteristic smell or taste. Brown or dark brown spots more or less softened, and of various sizes, were visible on cross-sections of roots not too badly infected. The inside of those specimens which were badly attacked was, however, almost entirely brown, and in parts the parenchyma was wholly destroyed, giving place to a slimy, sticky, gum-like, brown-colored, strongly acid, odorless fluid. The destruction of the tissues proceeded so far in some parts of the root that, finally, only the vascular bundles remained. The beets attacked by this disease yielded no characteristic odor, and they only began to smell bad in the last stages of the disease after rotting had set in.

(4) *Pathological Histology.*—An examination of thin sections, made through a brown spot, showed that the cells of this

parenchyma contained tiny roundish or ellipsoidal shining bodies, which were of various sizes, and either scattered about in the cells or united into groups. The individual particles showed not rarely a tremulous motion. When such a thin section was fixed to a cover glass by passing it three times through the flame, and was then stained with gentian violet, these bodies became a beautiful blue, and their bacterium-like form could be made out more clearly. In the parts of the root which had already become slimy great numbers of bacteria were to be seen in the gum-like fluid, together with loosened cells, plasma, and fragments of cell membranes.

(5) *Direct Infection Experiments.*—When a little of the soft, slimy mass was lifted on a sterile platinum needle and spread on a sterile [steamed ?] section taken from an apparently sound beet, the surface of the latter was covered within forty-eight hours with a slimy, brown, gum-like, acid layer, which consisted of a mass of those bacteria previously found in the diseased beets. Sections cut out of diseased beets with sterile knives and placed on fresh, unsterilized sections from sound beets, and kept in a moist chamber at 24° C., caused the latter to become affected. The infected spots browned and softened, and in the tissues bacteria appeared, which were just like those occurring in the diseased beets. A slimy layer also formed on the sterile cut surface of carrots when a slight quantity of the slimy ooze from the beets was spread over it.

"This preliminary investigation indicated that most likely in this case we have to do with a disease caused by bacteria. Positive proof, however, is not thereby afforded. To accomplish this experimentally it is absolutely necessary to isolate the bacteria occurring in the diseased beets, to cultivate them pure, and then to inoculate the pure cultures into sound living beets. If then as a result of the infection the previously healthy beet should become diseased with the before-mentioned symptoms, and the originally inoculated bacteria should appear once more in the tissues, then there would be no doubt about this being a bacteriosis of the beet."

Clearly this man knew exactly what he had to do.

(II) THE ORGANISM.—This is described as a bacillus, but not named.

(1.) *Pathogenesis.*

(A) Yes.

(B) Yes. Rather easy.

(C) No. These inoculations led to no satisfactory result, and had to be abandoned, because no suitable beet material was at the experimenter's disposal.

(D) No.

Conclusion.—Pathogenic nature rendered probable. The reason for this judgment in opposition to the above statements will be found in the following paragraph.

While Dr. Kramer was not able to secure infections, owing probably to the unfavorable conditions under which he worked, he hit upon an ingenious method of indirect proof, viz., the development in pure cultures of the same gum which is formed naturally in the diseased beets. His method was as follows: The softened or liquefied parts of the diseased beets were cut out, crushed and heated on a water bath, with the addition of a small quantity of milk of lime. The fluid was then decanted, and the remaining mass of beet squeezed as dry as possible and the two fluids mixed, filtered, and carbon dioxid passed into the filtrate for the removal of the somewhat superfluous lime. The fluid was again filtered and concentrated on the water bath. The fluid was now rendered acid by the addition of some drops of acetic acid, and a white, tough, gum-like substance was precipitated out of it by the addition of 96 per cent. alcohol. To obtain it in a pure condition this substance was repeatedly dissolved in water and reprecipitated by alcohol. The same substance was obtained directly from the gummy ooze of the diseased beets by dissolving it in water, heating, filtering, concentrating on the water bath, and precipitating with alcohol. In this case also the precipitate was a white, tough, gum-like substance. Both of these precipitates were tested chemically with the following results. Mixed with soda-lime and heated in a test tube there was no formation of ammonia, a proof that the substance was free from nitrogen. Boiled with orcin and hydrochloric acid it gave the well-known gum reaction, men-

tioned by Reichl and Wiesner. Boiled with sulfuric acid it was converted into dextrose. A watery solution gave a bluish flocculent precipitate with Fehling's solution, and on boiling the latter was reduced. On the addition to a watery solution of ferric chlorid and calcium carbonate the well-known precipitate of gum solutions resulted. No red coloration appeared on treatment with iodin. The formation of oxalic acid could not be detected on long boiling with nitric acid. All these reactions indicated a gum. In the beet this could be derived only from carbohydrates, and most likely from dextrose. Working on this hypothesis, a fluid culture medium was prepared containing 3–4 per cent. of dextrose, a slight quantity of peptone, and the necessary mineral ingredients. In this solution pure cultures of the organism were grown 8 to 14 days at a temperature of 24° C., and from the resulting products of growth a gum-like substance was obtained which proved to be identical with that secured directly from the diseased beets. These cultures were protected from contamination by cotton plugs, and at the close of the experiment cultures therefrom showed them to have remained pure, consequently this bacillus must have converted the dextrose into gum.

2. *Morphology.*

(1) *Shape, size, etc.*—The organism as isolated and grown in pure cultures is a thick rod with rounded ends, or often narrowed at the ends (zugespitzt), of variable length, so that not rarely coccus or ellipsoidal forms appear. These rods are about 1.30–2.00 x 0.7–1.0 μ. In cultures they occur singly or in pairs, which latter are more or less biscuit-shaped. Chains are rarer.

(2) *Capsule.*—No mention of any capsule.

(3) *Flagella.*—No statement as to motility, except mention of the trembling motion inside the cells of the beet, which can scarcely be taken as a proof of motility.

(4) *Spores.*—"Apparently spores are formed." This matter is left in considerable doubt. Rods in the stage of spore formation are said to be 1.35 x 2.00 μ.

(5) *Zooglœa.*—No mention of zooglœa.

(6) *Involution forms.*—No mention of any distorted forms.

3. *Biology.*

(1) *Stains.*—These bacilli take all the ordinary anilin colors.

(2) *Gelatin.*—On plate cultures of nutrient gelatin containing dextrose the colonies are small, nearly circular, sharply contoured, white, shining, and at most not over 1 mm. in diameter. Under a weak magnification they appear sharp-edged, granular and brownish. Stab cultures in the same gelatin show a fine thread not spreading beyond the needle track. At the mouth of the stab there is a top-shaped enlargement not inclined to spread out much. Streak cultures on nutrient gelatin develop a line along the track of the needle which is very slightly inclined to widen. This is formed of dot-shaped hyaline colonies, which finally fuse. The bacillus does not liquefy gelatin.

(3) *Agar.*—Plate, stab and streak cultures on nutrient agar were not unlike those on the gelatin. Exact statements as to the composition of the nutrient gelatin and agar are not given.

(4) *Potato, etc.*—Pure cultures on sterilized slices of beet gave a brownish, slimy growth, having a strongly acid reaction. The same on carrot gave a whitish, slimy layer, having a strongly acid reaction. On potato the growth showed no specially characteristic mark, but was strongly acid.

(5) *Animal Fluids.*—No statement.

(6) *Vegetable Juices.*—No statement.

(7) *Salt Solutions and other Synthetic Media.*—The 3–4 per cent. dextrose-peptone solution (distilled water?) containing the necessary mineral ingredients (not named) became cloudy in forty-eight hours and less limpid in 8 to 14 days.

(8) *Relation to Free Oxygen.*—Aerobic.

(9) *Reducing and Oxidizing Power.*—No statement.

(10) *Fermentation Products, and other Results of Growth:*

(a) *Gas Production.*—No statement. If the cattle disease were really due to this organism, then we may suppose it to be an active gas producer in the presence of certain carbohydrates.

(b) *Formation of Acids.*—This bacillus is a strong acid producer. The sap of the diseased beets shows a strong acid reaction. Pure cultures strongly reddened blue litmus gelatin in

forty-eight hours, and developed an acid reaction in neutral nutrient solutions within a few days. The composition of these solutions is not stated, nor whether the growth of the organism is self-limited by the production of the acid. This production of acid serves to strengthen the belief that the bacillus is really the cause of the beet disease. The nature of the acid was not determined.

(c) *Production of Alkali.*—Not recorded.

(d) *Formation of Pigment.*—Brownish color on beets.

(e) *Development of Odors.*—No odor.

(f) *Enzymes.*—Not determined. Cell walls are dissolved.

(g) *Other Products.*—Not stated.

(11) *Effect of Dessication.*—No statement.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined. If the bloating of the cattle were due to this organism, it must be able to grow at blood heat, and the accurate determination of its thermal relations should not have been omitted.

(b) *Optimum for Growth.*—Not determined.

(c) *Minimum for Growth.*—Not determined.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—No statement.

(14) *Vitality on Various Media.*—No statement.

(15) *Effect on Growth of Reaction of Medium (acid, neutral, alkaline).*—No statement.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—No statement.

(18) *Effect upon Animals.*—No cattle could be had for experimental purposes; but the germ was carefully tested on rabbits and white mice, and was not pathogenic either when fed to them in carrots, rubbed into subcutaneous wounds, or injected into the blood by means of a Pravaz syringe.

(III) ECONOMIC ASPECTS:

(1) *Losses.*—Serious.

(2) *Natural Methods of Infection.*—Not known.

(3) *Conditions Favoring the Spread of the Disease.*—Not known.

(4) *Methods of Prevention.*—No experiments, and no observations. Disease not studied in the field.

Remark.—This disease was also seen in 1891 by Dr. Paul Sorauer, who described it as follows, in a short note appended to a (24) " Review" of some papers on the Sereh-disease of sugar cane (*Zeitschrift für Pflanzenkrankheiten*, Bd. I., Heft 6, 1891, p. 360). " We can now report similar phenomena in our *Beta.* A parcel of beets sent to us from Slavonia were suffering from a disease which may be designated *gummosis.* Investigations up to this time have shown that the bacteria induce the formation of a syrup-like gum. Here also the first indications of the disease are a red-brown, subsequently a black-brown, staining of the vascular bundles, and each drop of gum swarms with myriads of apparently specific bacteria. If this gum is dropped upon sliced (*praeparierte*) sound beets the bacterial gummosis is there easily produced. The preparation of the beet so as to be susceptible to the disease appears to lie in a lessening of the acidity of the tissues, etc." No strictly bacteriological work appears to have been done, and I have quoted all of the article that is pertinent.

2. THE ROT OF SUGAR BEETS (1891).

In 1891, in (25) *Fungous Diseases of the Sugar Beet*, Bull. No. 15, Iowa Agric. Experiment Station, Ames, Iowa, p. 243, Reprint p. 9, Prof. L. H. Pammel, of that station, described a beet disease from Iowa which he attributed to the fungus *Rhizoctonia betæ.*

Associated with this fungus were various bacteria to which he ascribed the subsequent wet rotting of the roots. The rotting beets gave off a strong odor not unlike that of rotting potatoes. Unquestionably " the ultimate rotting is caused by bacteria." Several bacteria were isolated, and among others *Bacillus subtilis.* Inoculations with a pure culture of one of these organism did not give any very decisive results. No bacteriological studies of any consequence seem to have been made.

3. A BACTERIAL DISEASE OF SUGAR BEETS (1892).

(I) THE DISEASE.

(1) *Author, Title of Paper, Place of Publication.*—This disease was described by Dr. J. C. Arthur and Katherine E. Golden, of

the Purdue University Agricl. Experiment Station, Lafayette, Indiana, in (26) *Diseases of the Sugar Beet*, issued April 13, 1892, and forming Bulletin No. 39, Vol. 3, of that station, pp. 54–58, and summary, pp. 61–62.

(2) *Geographical Distribution.*—This disease appeared in sugar beets grown for experimental purposes at the Indiana station, and seems to have first attracted the attention of the station chemist owing to the low percentage of sugar found in some of the roots. These were examined microscopically, and bacteria, or bacteria-like bodies found in the tissues. This was in 1890. "Owing to the lateness of the season, and the lack of a plant house, the observations on the disease soon came to an end, to await the next growing season. The following description of the disease, and of its distribution and cause, is therefore the result of studies made almost wholly during the summer of 1891 and the winter of 1891–2." The disease is prevalent in many places in Indiana. In 1892 it occurred in all of the eight varieties of sugar beets grown on the Purdue Station grounds, and was found to some extent in nineteen of the twenty-seven samples of beets sent in for analysis from as many different localities in that State. This is not, however, it is stated, an entirely fair indication of the prevalence of the malady, since it is customary to select the best beets for analysis, and the proportion of diseased ones in such lots is less than the actual average. Of a total of 434 beets received from different parts of Indiana, and examined for this disease, 12 per cent. were affected. No record was kept of the percentage of diseased beets appearing on the experiment station grounds, but this is stated to have been large.

(3) *Symptoms.*—"This disease does not usually cause the death of the plants, any spots upon its surface, or any alteration or discoloration of the tissues."

"The beet root shows externally no marks by which the presence of the bacterial parasite can be detected; the most diseased and the strictly healthy roots cannot be separated by any external characters. This statement, however, does not apply to the leaves. While the plants are small, the foliage of healthy and diseased plants remain normal; but as the plants

reach full size, and especially as they approach maturity, those which are most affected can be told at a glance by the altered appearance of the leaves. The healthy beet leaf has a decidedly flat, uniform surface, while the diseased leaf is puffed out between the veins in little blister-like areas, giving the general appearance of the surface of a Savoy cabbage leaf. Diseased plants are necessarily less vigorous than healthy ones, and the fact is made apparent to the eye as the season advances, by the leaves becoming paler and smaller, and the outer ones dying away faster than upon healthy plants. All these indications taken together, most reliance being placed upon the crinkled surface, will enable one to select much diseased plants as they are growing in the field, with considerable certainty. But some roots not showing the foliage characteristics will also be found to be affected.

"Upon cutting across a root the most constant indication of the malady is a greater prominence of the fibres which form the concentric rings. In well-marked cases each microscopic bundle shows a dark dot, the circles of dots growing more distinct on exposure to the air. In less pronounced cases the woody fibres are merely yellowish, or even quite colorless, but more prominent after being exposed to the air for awhile than normal tissues. Furthermore, the diseased root is rather soft and tough, and of a yellowish-white color, while a healthy root is firm, somewhat brittle, and in color a clean white. It has also been found that diseased roots are lighter in weight than healthy ones." They also contain less sugar. The reduction in all cases being considerable, and in some cases amounting to nearly 50 per cent. This is "presumably due to the presence of the bacteria."

In a foot-note it is stated that the circles of dark dots are found in all sugar beets. "And yet the greater prominence which these dark spots assume on account of the disease, make them one of the most effective indications of its presence."

(4) *Pathological Histology.* — A microscopic examination showed the bacteria "throughout all parts of the root." "So far as observed the disease rarely or never breaks down the tissues or kills the plant."

"A section from any part of a diseased root, under a magnification of four or five hundred diameters, shows the presence of great numbers of bacteria." "These bacterial parasites of the beet are not few, or difficult of detection; but occur in great numbers in every cell of the plant, and are conspicuous under the microscope without staining or other special treatment. The more pronounced the disease the greater the number of bacteria. They are most abundant in the large, loose-celled tissue, lying between the fibrous rings of the root and in the similar tissue of the veins and midrib of the leaf. This tissue consists of parenchyma, in which the protoplasm lines the walls of the cells and stretches across in strings from side to side. The bacteria are largely imbedded in or attached to the protoplasm, but also occur in the cell sap, sometimes in large numbers. While the bacteria are most abundant and conspicuous in the colorless parenchyma, they also occur in the cells of the fibro-vascular bundles, and in the green cells of the leaf; in fact, as has already been said, in all parts of the plant."

(5) *Direct Infection Experiments.*—No direct inoculation, or grafting of diseased roots upon healthy ones, appears to have been tried.

(II) THE PARASITE. Organism not named.

1. *Pathogenesis.*

(A) Yes.

(B) Yes. Easily isolated. Plate method. From the deeper tissues of the roots only one form of microbe is obtained.

(C) No, or yes, doubtfully. "Inoculation with pure cultures into the beet root has been attempted, and results appear to show that the disease was transmitted; but the trials were few, and we desire to repeat them before further discussing this part of the subject."

(D) No.

Conclusion.—Pathogenic nature not established.

2. *Morphology.*

(1) *Shape, size, etc.*—The bacteria are all of one shape and appearance. They are nearly twice as long as broad, small,

oblong, colorless, usually occurring as colorless cells, although occasionally found in pairs. No measurements are given.

(2) *Capsule.*—Nothing.

(3) *Flagella.*—The organism is said to be actively motile when grown in a rich nutrient fluid. No statement regarding flagella; or as to motility when taken directly from the plant.

(4) *Spores.*—Said to be arthrosporous; but no details are given or proof advanced in support of this statement, which probably rests on no other foundation than that endospores were not observed. So far as known to the writer nobody has demonstrated the existence of arthrospores in any species of bacterium or bacillus.

(5) *Zooglœa.*—No mention of zooglœa.

(6) *Involution forms.*—No mention of involution forms.

3. *Biology.*

(1) *Stains.*—No statement respecting behavior toward stains.

(2) *Gelatin.*—"Upon neutral gelatine the bacteria at first form a whitish growth, which becomes pale yellow with age, and the gelatine is eventually liquefied. Upon acid gelatine the liquefaction proceeds much more slowly. In all cases the gelatine finally becomes alkaline, whether acid or neutral to begin with."

(3) *Agar.*—"Upon agar-agar the growth is about the same as upon acid gelatine."

No statement as to what nutrient substances were added to the agar or to the gelatin or as to the reaction of the agar.

(4) *Potato, etc.*—Behavior not stated.

(5) *Animal Fluids.*—Not stated.

(6) *Vegetable Juices.*—"Develop well in sterilized juice expressed from the sugar beet; but their development cannot be readily watched, as contact with the air causes the juice to turn dark or even black."

It is not stated whether the expressed juice was sterilized by steam heat or at ordinary temperatures by filtration.

(7) *Salt Solutions and other Synthetic Media.*—"In a Pasteur sugar culture the bacteria grow well, causing the liquid to become slightly turbid in twenty-four hours. As growth

goes on, the turbidity becomes greater, and again decreases until at the end of nine or ten days, when growth practically ceases, the liquid becomes clear, and a grayish sediment falls to the bottom of the tube."

No statement as to whether the sugar in this solution was broken up with the formation of an acid.

(8) *Relation to Free Oxygen.*—No statement. Certainly not anaerobic, from the ease with which cultures were obtained.

(9) *Reducing and Oxidizing Power.*—No statement.

(10) *Fermentation Products and other Results of Growth*:

(a) *Gas Production.*—No statement.

(b) *Formation of Acids.*—No statement.

(c) *Production of Alkali.*—Neutral or acid gelatin becomes alkaline. In view of this statement it would be interesting to know whether the juice from diseased roots is alkaline, or less acid than that from healthy roots.

(d) *Formation of Pigment.*—Old cultures on gelatin are pale yellow.

(e) *Development of Odors.*—No statement.

(f) *Enzymes.*—Gelatin is finally liquefied.

(g) *Other Products.*—No mention of any.

(11) *Effect of Dessication.*—No statement.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined.

(b) *Optimum for Growth.*—Not determined.

(c) *Minimum for Growth.*—Not determined.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—No statement.

(14) *Vitality on Various Media.*—Not recorded.

(15) *Effect on Growth of Reaction of Medium (acid, neutral, alkaline).* Liquefaction of gelatin delayed by acidity.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—None recorded.

(18) *Effect Upon Animals.*—No statement.

(III) Economic aspects:

(1) *Losses.*—"A source of danger to the beet sugar industry of no inconsiderable moment." See also (I) (2).

(2) *Natural Methods of Infection.*—Not determined.

(3) *Conditions Favoring the Spread of the Disease.*—Not determined.

(4) *Methods of Prevention.*—No experiments recorded, and nothing known.

Remark.—From the statement quoted under *Pathogenesis* (*C*) one might infer this to be a preliminary paper, and it is possible that a subsequent one may clear up some of the many mooted points. At present the most that seems to be made out beyond doubt is that there is in Indiana a disease of sugar beets accompanied by decreased sugar content, and always or usually associated with minute bodies distributed pretty uniformly through the parenchymatic tissues, and believed to be bacteria. To the writer of this article the evidence that bacteria are really the cause of this disease does not appear to be very conclusive. Until more proof is advanced it is permissible to doubt (1) whether the organism isolated by plate cultures, and supposed to have been derived from the interior of the beets, was actually so derived; and (2) whether the bacteria-like bodies which "occur in great numbers in every cell of the plant," but which "never break down the tissues," or cause "any alteration or discoloration of the tissues," are really micro-organisms. To have every cell full of aerobic bacteria, and no lesions, is very remarkable, considering the nature of the plant cell, and certainly requires unusually strong evidence. Under the circumstances is it not possible that these bodies may be of a crystalline or crystalloid nature? This seems the more likely, from the fact that the juice of healthy table beets, the only sort the writer has been able to examine, is full of small particles endowed with active Brownian movement, and readily mistaken for bacteria when examined in hanging drops with medium magnifications. The uninjured parenchyma cells of the petioles were also found to contain these bodies in large numbers and in active motion. They stain slowly with alkaline methyl blue, and are not microörganisms.

4. THE DEEP SCAB OF BEETS (1891).

In (27) *Bulletin No. 4*, Agric. Experiment Station, North Dakota, Fargo, N. D., Dec., 1891 (pp. 15–17), Prof. H. L. Bolley,

of that station, described "a disease of beets indentical with deep scab of potatoes," which latter he attributes in another article in the same bulletin to "a bacterioid fungus-like affair having characteristics which would seem to ally it both to the fungi and to the bacteriaceæ." Inasmuch as this beet disease has frequently been cited on Prof. Bolley's authority as of bacterial origin, *e. g.*, in the last edition of Frank's (28) *Krankheiten der Pflanzen*, Bd. 2, p. 27, it is proper to mention it here, although the evidence for and against the bacterial nature of scab will be taken up seriatim only when we come to consider the bacterial diseases of the potato, to which the reader is referred.

5. THE ROOT-BURN OF BEETS (1894).

This disease should perhaps also be included. What little we know about its bacterial nature is derived from the brief account by Dr. L. Hiltner, in an address entitled, (29) Mittheilungen aus d. K. pflanzenphysiologischen Versuchsstation Tharand: Wie kann der Landwirt durch richtige Wahl, Pflege und Bestellung des Saatgutes des Krankheiten der Kulturpflanzen einigermassen verbeugen? *Sachsische Landw. Zeitschrift* 1894, No. 18, pp. 207–209.

Dr. Hiltner states that a disease called "Wurzelbrand" has caused great injury in recent years in almost all beet-growing lands. This disease appears in an early stage of growth as a more or less extensive constriction at the junction of stem and root. Subsequently there is a browning and decay of the root which proceeds from the constricted portion, and usually ends in the death of the plant. In spite of numerous investigations the cause of this disease has not been satisfactorily determined. For a long time its symptoms were confused with the gnawings of a beetle, *Atomaria linearis*, but Hellriegel, and afterwards Wimmer, showed that the disease could be preventod by soaking the beet balls for twenty hours in one-half per cent. solution of carbolic acid, and on this ground ascribes the disease to a fungus, which was believed to pass over from the beet balls to the roots of the young seedlings. Hollrung, on the contrary, found a fungus in the diseased parts of only four out of sixteen roots examined for that purpose, and ascribed the disease to

other causes, *i. e.*, to physical, chemical and mechanical peculiarities of the soil (Hiltner's account). Dr. Hiltner's own observations date from the discovery of a browning of the root hairs. He states that often in his germination experiments he had observed that a part of the root hairs on certain seedling beets would be colored brown and peculiarly shortened. When examined under a hand lens these hairs were seen to be mere brown points instead of long tubes. Seeds from a lot which germinated well, and produced seedlings that showed this browning of the root hairs to a marked degree, came up badly when planted in garden earth, and those which did grow afterwards developed the root-burn, the characteristic constriction occurring just where the sound root hairs were wanting. Hellriegel's treatment was repeated. After soaking the beet balls in a solution of carbolic acid the root hairs remained perfectly sound, and there was no subsequent root-burn. The parasite, however, is not a fungus but a bacterium. "In each epidermal cell of the root which bore a stunted hair there was to be found a specific bacterium, and to this is to be attributed the final destruction of the root." It is not stated whether the organism was isolated from the roots, or whether any infection experiments were undertaken. The context would lead one to think nothing of this sort was attempted.

RECENT LITERATURE.

Journey Through Mongolia and Thibet.[1]—This volume is 413 pages, is published in octavo form under the auspices of the Smithsonian Institution. It is an account of the travels of Mr. Rockhill in Mongolia and Thibet, based on a diary kept during the journey. The variety of subjects touched upon by the author in his descriptions of the country traversed, and the people with whom he was brought in contact, gives this volume a peculiar interest. Appendices to the diary

[1] Diary of a journey through Mongolia and Thibet in 1891 and 1892. By William Woodville Rockhill. Published by Smithsonian Institution, 1894.

contain information regarding the Solar and San-Ch'uan T'ujen vocabularies, the plants of Thibet, and the mean monthly temperature. A route map of the explorations made by the author, and a table of latitudes, altitudes, etc., accompany the book. The illustrations are numerous, comprising page plates and cuts in the text.

Publications of the United States Geological Survey for 1893–4. FOURTEENTH ANNUAL REPORT.[2]—This work is issued in two parts. Part I containing the Report of the Director on the operations of the Survey of 1892–92 administration, together with several reports. Part II comprises the accompanying papers embodying the results of the topographic and hydrographic work of the survey, and of the geographical investigations carried on through its aid. These researches were prosecuted chiefly in Virginia, West Virginia and Maryland, in Vermont and Massachusetts, in certain Rocky Mountain areas, and the Pacific coast region.

The illustrations are numerous, comprising 76 plates and 75 figures and diagrams.

A MANUAL OF TOPOGRAPHIC METHODS.[3]—This manual is one of the series of monographs published by the United States Geological Survey in quarto form. Its object, as stated by the author in his introduction, is to present a description of the topographic work, instruments and methods used by the United States Geological Survey primarily for the information of the men engaged upon this work. The manual is accompanied by a collection of constants and tables used in the reduction of astronomical observations for position, of triangulation, of light measurements, and other operations connected with the making of topographic maps, and is illustrated by eighteen plates showing types of topography.

MINERAL RESOURCES OF THE UNITED STATES.[4]—This report for the calendar year 1893 is the tenth in the series. The subject-matter includes: First, a statement of the mineral products; secondly, the industrial conditions affecting these products; and thirdly, recent additions

[2] Fourteenth Annual Report of the United States Geological Survey, 1892–93. Part I.—Report of the Director. Washington, 1893. Part II.—Accompanying Papers. Washington, 1894.

[3] Monographs of the United States Geological Survey, Vol. XXII. A Manual of Topographic Methods. By Henry Gannett. Washington, 1893.

[4] Mineral Resources of the United States for the Calendar Year 1893. By David T. Day. Washington, 1894.

to the knowledge of mineral deposits in this country. The statistics of production, and of imports and exports, were collected by experts, whose names are given at the heads of the several chapters, and are, therefore, unquestionable. The arrangement of the material is convenient for reference, and a good index completes the work.

An Introduction to the Study of Zoology.[5]—This book, as stated by the author, is a kind of guide-book to beginners in the study of the animal kingdom. Among other good points made by Mr. Lindsay is a recommendation of a course of study in his advice to students, and suggestions as to the best books to buy for those whose time or money is limited. This forms Part III. Parts I and II treat respectively of the general principles of the subject and systematic zoology. Part I is concise, but clear, and on the whole up to date. The systematic part, however, is weak, by reason of the lack of clear, precise definitions. This is particularly true of the Vertebrata; and, in general, no advantage has been taken of the discoveries of paleontology.

The illustrations are numerous and "taking." On the whole, the volume will be of interest and value to those whose wants it is intended to meet, *i. e.*, the adult student, who wishes a first-lesson book which is not milk for babes.

The Cranial Nerves of Batrachia.[6]—This paper is a reprint in book form of an article published in the *Journal of Morphology*, Vol. X, No. 1. The author confines himself to a discussion of the V, VII IX and X nerves, including other nerves in the description only as they come into connection with those specified. After an explanation of the technique employed, a detailed description of the nerves and their components is given, followed by a comparative morphology of components. The closing chapter deals with the relation of the cranial and spinal nerves, the relations of the pre- and post-auditory nerves, and the bearings which the results of the author's studies have upon the classification of the nerves and their segmental relations.

Especial light is thrown on this subject by this research, which includes as an especial feature the determination for the first time of the motor and sensory fibres in each case. The monograph is one of especial excellence.

[5] An Introduction to the Study of Zoology. By B. Lindsay. London, 1895, Swan, Sonnenschein & Co. New York, Macmillan & Co. $1.60.

[6] The Cranial Nerves of Amphibia. By Oliver S. Strong. Boston. 1895. Ginn & Co., Pub.

Structure and Life of Birds.[7]—In this volume the author undertakes to show the development of birds from reptilian ancestors from anatomical evidence. He then describes the structure in detail, dwelling on the work done by special organs. The chapter on flight comprising some hundred pages, contains the latest information on this vexed problem, and includes much original matter, the result of Mr. Headley's personal investigations and observations. Color and song, instinct and reason, migration, and the principles of classification are treated of in separate chapters. A brief recapitulation of the arguments for and against the theory that the ancestors of the ostrich family were birds of flight, and hints as to the best methods of studying ornithology, are given in concluding the subject. Each chapter is accompanied by a list of books bearing upon the topic under discussion. Seventy-eight illustrations are given in the text, some of which are reproductions from photographs of birds in motion.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

The Chemical Composition of Turquoises.—Carnot[2] notes the occurrence of turquoise in the Burrow Mts., Grant Co., N. M., in a sort of pinkish-gray pegmatite. The structure is micro-crystalline, the fracture irregular and somewhat conchoidal. The analysis given is: P_2O_5 28.29, Al_2O_3 34.32, CuO 7.41, FeO .91, MnO trace, CaO 7.93, MgO trace, H_2O 18.24, F trace, quartz or clay 2.73, total 99.83. An analysis of the well known Persian turquoise gave P_2O_5 29.43, Al_2O_3 42.17, CuO 5.10, FeO 4.50, H_2O 18.59, quartz or clay .21, total, 100.00.

These analyses and others already published show, it is true, a good deal of variation in the composition of turquoise, yet are thought by Carnot to agree fairly with the formula P_2O_5 (Al_2 Cu_3 Fe_3 Ca_3) O_3+ Al_2O_3+5 H_2O. Stress is laid on the determination of *all* the iron as ferrous. The above data were obtained from the true oriental turquoise, or that "of the old rock."

The occidental turquoise, or that "of the new rock" may better be called odontolite, coming from the teeth of fossil mammals. They are very variable in composition, and contain iron in the ferric condition, as well as 3.02 per cent, or, in another specimen, 3.45 per cent of fluorine, thus differing from the oriental turquoise.

The occidental turquoise may be distinguished from ordinary bones and fossils by lack of calcium carbonate, presence of ferric phosphate, and by the large quantity of aluminium phosphate, also by the blue color.

Alstonite and Barytocalcite.—A posthumous note by Mallard[3] presented to the French Society of Mineralogy by M. Termier, gives interesting comparisons between the properties of the minerals containing barium and calcium carbonates. While barytocalcite has been long considered to be a double salt, the usual view concerning alstonite has been that it is an isomorphous mixture of the two carbonates. A series of analyses made by Chatelier suggests that alstonite may be also a double salt with the same formula as barytocalcite. The prismatic

[1] Edited by A. C. Gill, Cornell University, Ithaca, N. Y.
[2] Bull. Soc. Fr. Min., XVIII, pp. 119-123, 1895.
[3] Bull. Soc. Fr. Min., XVIII, pp. 7-12, 1895.

angle of alstonite is determined as 119° 9', which is not in accord with the view that it is an isomorphous mixture of witherite and aragonite, since their corresponding angles are 117° 48' and 116° 16' respectively.

The indices of refraction of witherite, alstonite and barytocalcite for sodium light were measured and compared with those af aragonite and calcite. In the following table, column III gives the mean between the values for aragonite and for witherite:

	I Aragonite	II Witherite	III Mean	IV Alstonite	V Barytocalcite	VI Calcite
α	1.5301	1.529	1.5295	1.525	1.525	1.48625
β	1.6816	1.676	1.679	1.673 (?)	1.684	
γ	1.6859	1.677	1.681		1.686	1.6585
Sp. G.	2.94	4.28	3.61	3.71	3.65	2.73

Attention is called to the remarkable crystallographic similarity between barytocalcite and calcite, nothwithstanding the difference in crystal system. The cleavage of barytocalcite form a pseudorhombohedron, being basal and prismatic. The angle of the prism 106° 54', and the angle between the base and prism is 102° 54', while the cleavage rhombohedron of calcite has angle of 105° 5'. Moreover, the optical angle of barytocalcite is small, and the negative acute bisectrix make an angle of +64° 22' with the c axis (i. e., with the intersection of the prismatic cleavages); the optical angle of calcite is zero, and the negative optical axis makes an angle of +63° 44' with the intersection of two rhombohedral cleavages.

In conclusion Buchrucker's values for the indices of strontianite are corrected. Mallard's values for Na light are: $\alpha = 1.518$, $\beta = 1.664$, $\gamma = 1.665$.

Rutile, Cassiterite and Zircon.—According to Traube,[4] who discusses the question of the isomorphism of the above minerals, the etched figures produced by KF or KF HF are exactly similar for each of the three species, and indicate holohedral symmetry in the tetragonal system.

An attempt to make rutile containing SiO_2 was not successful, though Traube considers that it must have been so in case rutile and zircon were isomorphous. Synthetic experiments were also made for the purpose of throwing light on the mode of occurrence of Fe_2O_3 in these minerals. By heating chemically pure amorphous titanium dioxide in a platinum crucible with sodium tungstate and a metallic oxide, the following results were obtained. With Fe_2O_3 rutile was formed containing in one case as much as 5.4 per cent of that oxide; rutile

[4] Neues Jahrb. B.B. X, pp. 470–476, 1896.

with 3.01 per cent Mn_2O_3, and in another case with 1.91 per cent Cr_2O_3 was prepared, but similar experiments with the oxides of nickel and cobalt were not successful, the rutile crystals containing no appreciable trace of Ni or Co. Chromiferous crystals of cassiterite were also formed. It seems, therefore, that these substances have a peculiar affinity for the oxides of the type R_2O_3, but not for those of the form RO.

Colored specimens of all three minerals become permanently lighter in color on heating.

Marignac's process for fusing zircon (i.e., with KF or with KF HF) was tried with rutile and cassiterite. Like zircon they both fuse rather readily, forming $K_2 TiF_6$ and $K_2 SnF_6$ respectively.

Miscellaneous Notes.—Wülfing[5] describes a simple apparatus for obtaining monochromatic light from direct sunlight. The experiments on quartz seem to show that the apparatus works with a good degree of accuracy. Measurements of the index of refraction of diamond gave for A, n=2.4024; for D, n=2.4175; and for H, n=2.4652. These are three of several values determined. The specific gravity of these diamonds referred to water at 4° was found to be 2.522±.003. Hematite from Elba was also investigated, giving:

ω	ϵ	
2.904	2.690	for line A
2.988	2.755	for line B
3.042	2.797	for line C

The specific gravity at 4° is 5.285±.002. A description is also given of a spectrum apparatus for use with a microscope or an axial angle instrument. In a later note[6] Wülfing gives a table comparing the values of the indices of refraction of the diamond obtained by himself with those determined by Walter; the agreement is very close. He states that either apparatus above mentioned may be obtained of Eug. Albrecht in Tübingen.

Kretschmer[7] describes the occurrence of garnet, vesuvianite, wollastonite, epidote, augite, quartz and calcite at the contact of marble with granite near Friedeberg in Silesia. Minute details as to locality, association and crystal form are recorded.

Goguel[8] reports on the crystal form, and in some cases on the optical behavior of formopyrine, $C_{12} H_{14} N_2O_2$, and its addition salts with

[5] Tscherm. Mitth., XV, pp. 47–76, 1895.

[6] Ibid, p. 350.

[7] Tscherm. Mitth., XV, pp. 9–28, 1895.

[8] Bull. Soc. Fr. Min., XVIII, pp. 27–31, 1895.

hydrochloric, sulphuric, nitric, phosphoric and oxalic acids.—Duparc and Pearce[9] have measured the crystal angles and observed the optical properties of eight new chemical compounds. These are benzoyl-malic acid, sodium orthophenyl-benzoate, potassium orthophenyl-benzoate, ammonium phenyl-glycolate, dextrocinchonine phenyl-glycolate, benzylic ether of bromo-tolu-quinone oxime, potassium luteo-phosphomolybdate and a potassium luteo-phosphotungstate.

Of late numerous additions have been made to our knowledge of the crystallographic and optical constants of organic compounds. The following three papers in Volume XXV in the Zeitschrift für Krystallographie may be cited as important contributions to this line. 1. The Crystal form of Some New Halogen Derivatives of Camphor, by F. S. Kipping and W. J. Pope; 2. On the Crystal Form of Some Organic Compounds, by W. J. Pope; 3. Crystallographic and Optical Investigations on Some Organic Compounds, by E. A. Wülfing.

An artificial cassiterite investigated by Arzruni[10] shows distinct dichroism with the ray vibrating parallel to the vertical axis colorless, while the ray vibrating at right angles thereto is pink. The crystals reach a half centimeter in thickness and twice that in length. Twins, which are so common with natural cassiterite, were not observed. The angles measured coincide within 2′ with those given by Becke for the natural mineral. The mean values from two determinations of the indices of refraction are:

	Li	Na	Tl
ω	1.9846	1.9968	2.0093
ε	2.0817	2.0929	2.1053

These numbers agree as well as could be expected with those obtained by Grubenmann for cassiterite, showing that the natural and artificial products are practically identical.

Schmidt[11] gives at great length tables showing the recurrence of like interfacial angles in the regular system. As an extreme example, the angle 35° 15′ 52″ occurs between eleven pairs of faces, the cube, octahedron or dodecahedron constituting one face of each pair. The table at the end of the paper may be of use for rapidly identifying rare faces on regular crystals.

Sohncke[12] shows that in accordance with his views of crystal structure no circular polarization is to be expected in crystals of the pyramidal

[9] Bull. Soc. Fr. Min., XVIII, pp. 31–43, 1895.
[10] Zeitschr. f. Kryst., XXV, pp. 467–470, 1895.
[11] Zeitschr. f. Kryst., XXV, pp. 477–503, 1895.
[12] Zeitschr. f. Kryst., XXV, p. 529, 1895.

tetragonal class (hemimorphic hemihedral division of the tetragonal system). So far as known, circular polarization is not exhibited by crystals with this grade of symmetry.

PETROGRAPHY.[1]

Petrography of the Bearpaw Mountains, Montana.—The Bearpaw Mountains are the dissected remains of a group of Tertiary volcanoes. Their cores of the old volcanoes are granular rocks, their lavas and tuffs are represented by basic sheets and beds. The lavas are largely basalts, leucite-basalt and other similar basic types.[2]

The cores consist of mica-trachytes, quartz-syenite, porphyries, containing aegerite-augite and anothoclase-phenocrysts, in which are imbedded microlites of oligoclase, trachytes containing hornblende and diopside and shonkinite. A few miles from Bearpaw Peak a denuded core is exposed, which furnishes a good example of the differentiation of a syenite in place. The intrusion is laccolitic in character. Around its borders it has highly altered the sedimentary rocks with which it is in contact. The most acid portion of the laccolite is a light aplitic syenite containing quartz and diopside. The main mass is a more basic syenite resembling monzonite or yogoite. It contains diopside and much plagioclase. The most basic phase is a shonkinite. Analyses for the three principal types follow:

	SiO_2	Al_2O_3	Fe_2O_3	FeO	MgO	CaO	Na_2O	K_2O	H_2O_5	Other	Total
Quartz-syenite	68.34	15.32	1.90	.84	.54	.92	5.45	5.62	.45	.57	= 99.95
Monzonite	52.81	15.66	3.06	4.76	4.99	7.57	3.60	4.84	1.09	1.86	=100.24
Shonkinite	50.00	9.87	3.46	5.01	11.92	8.31	2.41	5.02	1.33	2.68	=100.01

The totals corrected for Fe and Ce are 99.94, 100.22 and 99.93 respectively.

Two French Rocks.—In the serpentine of St. Préjet-Armadon, Haute-Loire, France, Lacrou[3] finds nodules composed of asbestiform gedrite surrounding a kernel of serpentine or biotite. The nodules are separated from the serpentine by an envelope of biotite. They are sup-

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.
[2] Weed and Pirson: Amer. Journ. Sci., IV, Vol. 1, p. 283 and 351.
[3] Bull. Soc. Franc. d. Min., XIX, p. 687.

posed to be of secondary origin. Bronzite and asbestus both occur in the rock. In the norite area of Arvien, Auvergne, the same author describes a variety of this rock which is characterized by the presence of secondary reaction, rims of anthophyllite and actinolite between its hypersthene and plagioclase, the former appearing next to the pyroxene. The plagioclase of the rock is often altered to actinolite, garnet and albite, while the hypersthene is changed to an aggregate of anthophyllite.

The Granite of the Himalayas.—McMahon[4] describes the granite of the N. A. Himalayas. Although highly foliated in the borders of its masses, the rock is shown to be eruptive. The author thinks the foliation is due to pressure upon the rock before it finally solidified. He attempted to show that this schistosity could not possibly have been produced after the rock cooled. The granite is coarsely porphyritic with large orthoclase crystals in a medium to fine grained groundmass composed of the usual constituents of granite. This is cut by tiny veins of quartz which are supposed to represent the micrystallized residue left after the first partial consolidation of the rock, or to be the result of a partial fusion of the quartz grains originally occurring in it. This quartz, though it presents the usual aspects of secondary quartz, is thought to have been injected into the vein spaces while it was in a molten condition. Sinuous areas and viens of microcrystalline mica are likewise observed in the granite, and these are thought to have been produced by the rapid crystallization of mica that had been melted, and not by the crushing and shearing of the original micas nor by secondary processes of any other kind. The paper is well illustrated by photo-micrographs.

California Rocks.—Fairbanks[5] describes the rocks of Eastern California between Mono Lake and the Mojave desert as comprising both sedimentary and igneous forms. Among the latter are both granitic and volcanic varieties. The granites form the eastern slope of the Sierra Nevadas. In the northern portion of the area it is a coarsely porphyritic biotite hornblende variety. In the southern portion it is replaced by a more basic phase containing less hornblende. The volcanic rocks met with in the district are andesitic flows, dykes and tuffs, and basalt flows among the more recent rocks and liparites among the more ancient ones. The microscopical description of the type is deferred to a later paper.

[4] Proc. Geologists Association, Vol. XIV, p. 287.

[5] Amer. Geologist, Vol XVII, p. 63.

Turner[6] gives a classification of the igneous rocks studied by himself from various places in California. He divides them into families in accordance with their mineralogical composition, including in the same family all those rocks with the same composition irrespective of structure. He then takes up the syenites and discusses them in some detail. The family is made to include syenites (granular), syenite-porphyries (porphyritic) and trachytes (microlitic and glassy) and apo-trachytes. The syenites include soda-syenite or albitite, augite-syenite, hornblende-syenite and mica-syenite. The apo-trachytes include among other rocks Rosenbusch's orthophyres and keratophyres. Until very recently no rocks of the syenite family have been proven to occur within the borders of the State. All those rocks described as such are now known to be hornblende-andesites, granites or diorites. The author refers briefly to the known occurrence of the syenites in the State and describes more fully some new ones.

He reports dykes of white albitite-porphyries or soda-syenite porphyries in the rocks of the Mother lode quartz mines. In the bed of Moccasin Creek the rock consists of quartz, muscovite and albite, but in other places it consists almost exclusively of albite with a few grains of an olivine-green mineral thought to be aegerite. The rock resembles somewhat Brögger's sölosbergite and Palache's albite rock containing crossite. An analysis of one specimen gave:

SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	H_2O	P_2O_5	Total
67.53	.07	18.57	1.13	.08	.55	.24	.10	11.50	.46	.11	=100.34

Gabbro-Gneiss from Russell.—The gabbro of Russell, St. Lawrence Co., N. Y., is said by Smyth[7] to change its character rapidly in consequence of a variation in grain from moderately fine to very coarse, in structure from porphyritic to granular and in color from black to gray. Upon alteration the gabbro passes into a rock made up of red masses in a groundmass of gabbro. In other places it becomes schistose, when it takes on a granulitic texture. Sometimes hornblende is developed in it in long narrow plates that run approximately at right angles to the schistosity, causing the rock to resemble a metamorphosed sediment. Even in the most gabbroitic varieties of the rock the plagioclase is changed into an aggregate of secondary products, among which scapolite is the most common. In the change of the massive gabbro into the schistose variety the constituents are first

[6] Ib., Vol. XVII, p. 375.
[7] Amer. Jour. Sci., Vol. 1, p. 273.

granulitized and then drawn out into lenticular areas. The feldspars of the gneisses appear to have been recrystallized, since the feldspathic areas consist of single feldspar individuals and not fragments of grains. The pyroxene also differs from the gabbro pyroxene. It has lost its characteristic black inclusions and has assumed a deep green color. This mineral, as well as the hornblende, which is abundant in the gneisses, are both regarded as having recrystallized, the augite material coming from the original augite of the gabbro and the hornblende from the secondary amphibole so common in the gabbro. The gneisses are thus schistose gabbros in which recrystallization has taken place with attendant granulitization. The author points out the fact that in the first stages in the alteration of the gabbro scaly hornblende and scapolite are formed, while in the final stage they have completely disappeared, and in this latter stage there results a gneiss which bears no evidence of having been crushed.

Notes.—The serpentine near Bryn Mawr, Penna., has resulted by the alteration of a peridotite according to Miss Bascom.[8] The rock of the Conshohocken dyke is a typical diabase.

GEOLOGY AND PALEONTOLOGY.

Fossil Jelly Fishes.—Certain curious forms, locally known as "star cobbles," have long been found in the middle Cambrian shales and limestones of the Coosa Valley, Alabama. They occur at two horizons associated with silicious concretions. The "star cobbles" are recognized by Mr. Walcott as fossil medusæ, and among the 8,000 specimens now in the collections of the U. S. Geological Survey he has separated two types allied to the recent Discomedusæ. From the large number of specimens that have been found over a relatively small area, it is evident that they were gregarious and very much like the modern Rhizostome (*Polyclonia frondosa*) in their habits.

The author describes three species, and refers them to two new genera, Brooksella and Laotira, which he also defines. These forms, *Brooksella alternata*, *B. confusa* and *Laotira cambria*, together with *Dactyloidites asteroides*, the author groups in the family Brooksellidæ, and gives a diagnosis of the family.

[8] Proc. Amer. Acad. Science, 1890, p. 220.

A number of drawings of different views of the three specimens described accompany the paper. (Proceeds. U. S. Natl. Mus., Vol. XVIII, No. 1086, 1896.)

Is Palæospondylus a Mansipobranch?—In a collection of fossil fishes belonging to Columbia College, Mr. Bashford Dean has found a specimen of Palæospondylus presenting structural details which decidedly oppose the hitherto accepted view that Palæospondylus is a paleozoic lamprey. Mr. Dean figures the specimen in question, and presents the positive and negative evidence as to its marsipobranchian affinity. From this summarized statement it is seen that the cranium, vertebral column and paired fins do not bear out the theory; the caudal fin is essentially marsipobranchian, but its diphycercal, or, perhaps, heterocercal condition, is also common to many groups, shark, lung fish, and teleostome. The only characteristic which Palæospondylus retains allying it with the Cyclostomes is the presence of tentacles in the anterior head region. The author suggests that in the presence of so much negative evidence the head-tentacles can hardly be taken as a crucial test of kinship, since it is quite possible for these structures to have arisen independently within the group to which Palæospondylus belongs. (Trans. N. Y. Acad. Sci., Vol. XV.)

The Skeleton of Aepyornis.—A small collection of remains of the extinct birds of the genus Aepyornis, obtained from central Madagascar, has been sent to England by Dr. Forsyth-Major. The specimens include portions of two skulls, two imperfect mandibles, some coraco-scapulæ, a nearly perfect sternum, and some small bones supposed to be rudimentary humeri. A detailed description of these bones is given in a recent number of *The Ibis* by Mr. Charles W. Andrews. Of the skull he remarks that "in several respects Aepyornis approaches the Dinornithidæ in the structure of the skull. Among the points of resemblance are the pedunculate occipital condyle, the prominent basitemporal platform, the open Eustachian groove, the structure of the facet for the quadrate, and the presence of the frontal crest of large feathers (as in some of the Dinornithidæ)."

The sternum is "ratite," and in Apteryx is found the closest resemblance to Aepyornis. According to the author the fossil sternum appears to lack a metasternal region, and consists of the two primitive costosternal elements only. In this respect it corresponds to an embryonic stage in the development of the sternum in the recent Ratitæ.

The coracoscapula is typically Struthious in form. It similarity to that of Casuarius gives support to Milne-Edwards and Grandidier's

opinion that Casuarius is a near ally of Aepyornis. (The Ibis, July, 1896.)

Geological News. MESOZOIC.—Mr. C. W. Andrews has published a paper on the structure of the Plesiosaurian skull, in which he institutes a comparison of the palatal portion with that of other Reptilia. He shows that while a similarity of structure in that region does not necessarily imply close relationship, nevertheless the very great resemblances existing between the Plesiosaurian and Rhynchocephalian palates, reinforced by the numerous other points of likeness in other portions of their skeletons, lead to the conclusion that the Sauropterygia, notwithstanding their single temporal arcade and the rhizodont dentition, are descended from a primitive Rhynchocephalian reptile. This conclusion is in accord with the opinion already expressed by several writers. (Quart. Journ. Geol. Soc., May, 1896.)

CENOZOIC.—A restoration of *Hoplophoneus occidentalis* Leidy has recently been completed by Mr. E. S. Riggs, under the direction of Dr. Williston. The material upon which its restoration is based is composed of parts of two skeletons found almost together and in exactly the same horizon just below the *bullatus* layer of the Oreodon beds of South Dakota. This material now forms part of the paleontological collection of the University of Kansas. (Thesis for the Degree of A. M. in the Kansas Univ., 1896.)

—In a paper on recent and fossil Tapirs, Mr. J. B. Hatcher describes a new species of Protapirus from the Protoceras beds of the White River (Oligocene) of S. Dakota, presenting some new facts as to the osteology of the skull and forelimb of this genus. He also gives additional characters diagnostic of the various species of Protapirus and Colodon already described by Leidy, Marsh, Wortman, Earle and Osborn; points out the distinctive osteological and dental characters in the skulls of the five generally accepted species of recent Tapirs; and reviews the previous work of others on the Phylogeny of the Tapiridæ and Helaletidæ. (Amer. Journ. Sci., Vol. I, 1896.)

—A restoration of the skeleton of *Aptornis defossor* has been completed for the British Museum (Nat. Hist.). The bones from which the specimen is reconstructed were found in 1889 in a chasm in the limestone at Castle Rocks, Southland, New Zealand, the greater number of them no doubt belonging to a single bird. Mr. C. W. Andrews gives a brief description of this skeleton, calling atten-

tion to the great size of the anterior vertebræ in the cervical region, and the peculiar long, slender coracoids which are ankylosed with the much reduced sternum. The figure accompanying the text shows the probable position of the scapula in relation to the coracoid, the coraco-scapular angle being very obtuse, as in most flightless birds; the humerus is proportionally small, and its pectoral crest is reduced to a mere tubercle. (Geol. Mag., London, June, 1896.)

GENERAL.—According to C. D. Perrine, thirty-three distinct earthquakes were felt in California during the year 1894. This does not include a series of over one hundred shocks in Virginia, Nev., during the week of November 16–22, nor heavy earthquakes and volcanic disturbances which occurred in the New Hebrides group of islands during October and November. (Bull. U. S. Geol. Surv., No. 129, Washington, 1895.)

BOTANY.[1]

The Teaching of Elementary Botany.—That the teaching of elementary botany in this country is, to say the least, very poor, is a statement which needs no argument to prove its correctness. Much of the botany of the public schools is a wretched attempt at doing something which neither teachers nor pupils understand. In some places the pupil is made to con the pages of a text-book in which emphasis is laid upon minute and meaningless anatomical details of the structure of a few flowering plants. Elsewhere field-work, so-called, is required of the pupil; but here again the chaff is carefully separated from the grain, and *the pupil is given the chaff.* Thus he is made to fill out vacancies in blanks (called "schedules") in which the unimportant structural characters receive as much attention as those which are significant, the result being a description which neither describes nor separates the plant under consideration from dozens of others. The meaning of any structure is entirely overlooked, while the pupil is compelled to give much time and labor to unimportant details.

There are two reasons for this condition of things: first, the little knowledge of the science of Botany possessed by many teachers; and second, the absence of any definite idea on the part of teachers of the culture-value of Botany in the education of the pupil. To remedy the first the colleges and universities are opening summer schools for

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

teachers; and in these, for the most part, something of modern Botany is given. Many years of personal experience has shown the writer that in one Western State it has been possible to do much in the way of improving the teaching of Botany through the agency of the summer school. Let not the teachers of Botany in the colleges and universities grudge the time given to work in the summer school. The additional work is doubtless the most productive work of the year, for, if it be well done, its effects will be felt by hundreds of pupils in many schools. Let professors put their best efforts and their most mature thought into this work.

The remedy for the second obstacle may be looked for in the movement in the National Educational Association, which resulted in the organization of a Department of Natural Science Instruction, whose first meeting was held recently in Buffalo. It was notable that every paper presented at this meeting emphasized the culture-value of Science, and this was especially marked in those dealing with Botany in the school curriculum. We, who teach in the larger colleges and universities, have been remiss in not setting forth more prominently and forcibly the culture-value of Science, and botanists have sinned equally with the others. It is high time that we not only teach Botany for the culture which it gives the student, but we should by lectures, public addresses, and by popular articles, show how it may be presented so as to insure culture. Here we have a duty to perform, and if we have the interest of Science truly at heart, we will not shrink from the labor which this duty imposes. Let every professor of Botany realize that through the new department of the National Educational Association he may influence the teaching of his science so that it may have a culture-value.—CHARLES E. BESSEY.

The Conifers of the Pike's Peak Region.—It may help the visitor to Colorado Springs and Manitou to know that the following conifers are more or less common in the adjacent mountains. Perhaps, when he learns that through the carelessness of man enormous forests of these trees have been burned from the sides of Cheyenne Mountain, Cameron's Cone and Pike's Peak, and that where once grew dense forests of conifers, with their power of conserving the moisture of the snows and rains, there grows the worthless "popple" (*Populus balsamifera candicans*), he, too, will be ashamed of man, the vandal, who has destroyed forever, I fear, the conifer forests of this region, with the destruction of which forests there has been a decrease in the volume of water in the mountain streams, while at the same time the sudden and dangerous floods which rush down the mountain sides have greatly increased.

Juniperus communis alpina, the Mountain Juniper, is common everywhere from 7000 feet altitude to timber-line (11,500), as a low, spreading and almost trailing shrub.

Juniperus occidentalis monosperma, the Brown Cedar, or more commonly called here by the erroneous name of White Cedar, is common in the Garden of the Gods.

Juniperus virginiana, the Red Cedar, is to be found in the Garden of the Gods and generally at low altitudes. Some of the trees are entirely clothed with the short, blunt leaves, giving them a smoothness not generally seen in this species. Such trees are more glaucous, and are more round-topped than the ordinary kind in which many of the leaves are sharp-pointed.

Abies concolor, the White Fir, occurs abundantly from about 7000 feet to 8000 or 10,000 feet above sea level. Its beautiful layered foliage and erect cylindrical cones make it an object of interest to every traveller.

Pseudotsuga taxifolia (*P. douglasii* of Coulter's Manual), the Douglas Fir, is the most common of the single-leafed conifers, occurring everywhere from the foot of the mountains to timber-line. It is distinguished at once by its elliptical cone, with trifid bracts between the scales.

Picea engelmanni, Engelmann's Spruce, and *P. pungens*, the Sharp-leaved Spruce, are common from 7000 or 7500 feet up to 9000 or 10,000 feet altitude on the eastern slopes of Pike's Peak.

Pinus flexilis, the Rocky Mountain White Pine, occurs from Cheyenne Mountain to Pike's Peak, from 7000 feet to timber-line, where it is very common. It may readily be distinguished by its leaves, *which are in fives*.

Pinus balfouriana aristata. This tree resembles the preceding, and apparently is often confused with it under the name of "White Pine" or "Foxtail Pine." It grows at high altitudes (10,000 feet) up to timber-line, and in this region is a small, or at most, a moderate-sized tree. Its short leaves (about one inch) which are in fives, and prickly cones distinguish it from all other species.

Pinus edulis, the Nut Pine, is a low, spreading tree, often not more than ten or twelve feet in height. It may be distinguished by its short leaves and small cones, the latter containing a few large edible seeds. It is common in the Garden of the Gods and on the foot-hills a few hundred feet higher.

Pinus ponderosa scopulorum, the Rocky Mountain Yellow Pine, or more commonly called the Bull Pine, is the most abundant conifer of the region. It grows at all elevations, from the foot of the mountains and foot-hills to timber-line. Its leaves are long, and occur in twos and less commonly in threes.—CHARLES E. BESSEY.

Ferns Near Colorado Springs, Colorado.—So many thousands of travellers visit the beautiful city of Colorado Springs every year that the following list of the ferns to be found within easy walking distance from the end of the car lines may be of interest to botanical readers.

Notholæna fendleri Kunze.
Pteris aquilina L.
Cheilanthes tomentosa Link.
C. fendleri Hook.
C. gracilis (Fee.) Mett.
Pellæa atropurpurea (L.) Link.
Asplenium trichomanes L.
A. filix-foemina (L.) Bernh.
A. septentrionale (L.) Hoffm.
Phegopteris dryopteris (L.) Fee.
Dryopteris filix-mas (L.) Schott.
Cystopteris fragilis (L.) Bernh.
C. fragilis dentata Hook.
C. bulbifera (L.) Bernh.
Woodsia scopulina D. C. Eaton.
W. oregona D. C. Eaton.
W. obtusa (Spreng) Tore.
Botrychium virginianum (L.) Swz.
B. matricariaefolium A. Br.

A few notes on the above list may be of interest. There is a good deal of individuality about the Colorado climate, and the same is true of its ferns and their habits. The *Woodsia* and the *Pteris* are almost the only ferns found on the open hillsides, and these but sparingly; the others seek the protection of the mountain cañons. Most of them prefer cañons opening toward the north. During three summers spent in Colorado I do not remember finding a single fern in any canon opening toward the south.

In *Notholæna fendleri* we are told that the pinnules are oval in mature specimens. In most young fronds I have found them deltoid or spatulate, and in some beautiful specimens this form is retained. In such ferns the stipes are lighter in color and weight, the zigzag course of the rachis is less pronounced, the pinnæ are more distant, and the pinnules less numerous, giving the specimens a much lighter and more graceful appearance. The departure from the normal form is worth noting, but not sufficient to constitute a variety.

Cheilanthes tomentosa, according to the books, is from eight to fifteen inches in length at maturity. Most specimens live up to the rule, but

not a few ignore the books, and mature their fruit before they are eight inches tall; indeed, some very saucy specimens refuse to grow beyond a single inch, and scatter their spores to the winds in spite of their insignificant size.

The two ferns named above are to be found in most of the shady cañons near Colorado Springs, but *Asplenium trichomanes* I have found only in one place in South Cheyenne canon, while *A. septentrionale* has not been seen outside of the beautiful gulch in the Ute Pass, from which the city of Manitou obtains its water supply. All the lower canons of the Ute Pass would be rich fields for the botanist if the vandal tourist could be kept out of them ; as it is, there are still a few treasures left on the high rocks and in out of the way corners. Here *Phegopteris dryopteris* flourishes and *Cystopteris* runs riot.

The two Botrychiums were found in North Cheyenne Cañon, *B. virginianum* four, and *A. matricariaefolium* eight miles from the mouth of the cañon. Naturally, such fleshy ferns are seldom found in the dry atmosphere of Colorado, yet, in the one station where found, *B. matricariaefolium* was quite plentiful, and varied in form from a simplex-like plant of two inches to beautiful specimes nine inches high.

Of *Cystopteris fragilis* Eaton wisely wrote "very variable." The same might well be written of the whole genus so far as Colorado is concerned. It is the most abundant fern on Cheyenne Mountain, and there flourishes with little regard for the specific fences within which the books expect it to grow. I have not included *C. montana* (Lam.) Bernh. in the above list, because specimens found are hardly as broad as the typical form that species demands, while too broad to be classed as *C. fragilis*. The *C. bulbifera* found is without bulbs, but otherwise conforms to the books. The "winged" or "wingless rachis" of the books is not an unfailing test in differentiating the Colorado species of this genus, a microscopical examination of the indusium being necessary.

But if the Colorado *Cystopteris* is "very variable" what shall I say of Woodsia? I have entered *W. scopulina*, *W. obtusa* and *W. oregona* on the above list, because from the large amount of material on hand it is easy to select specimens which exactly conform to the species type in the books. I believe also that some specimens answering to *W. mexicana* might be selected, while a few would almost pass for *W. alpina*. But when this has been done what are we to call the still larger number of specimens, which are not exactly *W. scopulina*, nor *W. oregona*, nor *W. mexicana*, nor *W. obtusa?* Shall we say they are Woodsia, simply Woodsia, and nothing more? It seems to me that in this genus there is work waiting to be done of the same wise sort that Mr. George E. Davenport did some years ago in the genus *Botrychium*.—ALFORD A. BUTLER.

ZOOLOGY.

Lygosoma (Liolepisma) Laterale in New Jersey—As this species (the Oligosoma laterale of Girard and authors) is mainly characteristic of the South Atlantic and Gulf States—the Austroriparian Region in brief—its occurrence in New Jersey under such circumstances as to lead one to believe it a regular member of the fauna is of interest as an additional fact showing the strong southern stamp which the fauna and flora of that interesting region bear.

Yarrow's check-list, and most published lists since, record Salem, N. C., as the most northerly locality in the eastern United States, while southern Illinois and Indiana mark the northernmost limit of distribution in the Mississippi valley. Prof. Cope, however, informs me of a record for Maryland.

The New Jersey record is based upon a single specimen taken near Batsto, in Burlington County, on May 29th, of the present year. It was found on the ground concealed beneath a wood pile on a deserted farm, and glided away so quietly, clinging so closely to the ground and so skillfully seeking concealment beneath every small plant and chip, that it almost escaped unperceived.

It remained an interesting captive for about one month, but finally succumbed to its appetite in attempting to dispose of a large Polydesmus entire. During its captivity it partook of several small specimens of Julus, Polydesmus, and pill-bugs (Armadillo), besides small beetles and flies, a larval grasshopper and an earth-worm (Allurus)—food very different from that selected by *Sceloporus undulatus* under similar circumstances.

It was very fond of water, and sipped it up eagerly when poured on the bottom of its jar; the snout was buried beneath the surface, and the slender tongue travelled out and in rapidly until the water had sunk beneath the surface of the sand.

A similar movement of the tongue took place, as in the snakes, when food was detected, or under other excitement. The body movements were exceedingly graceful, but slow, as compared with Eumeces or Sceloporus.

During the night of June 14th two eggs were laid, and were found the next morning on the surface of the sand, without any covering whatever. Sceloporus has a similar habit in captivity, my experience being that the eggs are only partially or not at all covered. These eggs of

Lygosoma have the usual tough parchment-like shell, with calcareous surface deposits, as in many Reptilia. They have a structure essentially similar to that described for the egg-shells of Pityophis. They are more slender in form than those of Sceloporus, and measure 9.5 x 4.5 mm. The contained embryos were somewhat younger than those of Sceloporus usually are at the time of disposition—that is, the allantoic outgrowth was but just visible. The fertility of this specimen renders it probable that the species is a regular habitue of the neighborhood, but its small size, and retiring and nocturnal habits, render it very likely to escape notice.—J. PERCY MOORE.

On a New Glauconia from New Mexico.—Nasal entirely divided, rostral rounded behind, reaching the line of the eyes. Two labials anterior to the ocular, the posterior reaching the eye. Frontal and supraorbital scales smaller than those posterior to them. The eye is close to the nasal, and distant from the supraocular. Postocular reaching last labial, and bounded posteriorly by three sublingual scales. Inferior labials five, the second twice as large as any of the others; the fourth barely reaching the commissure of the mouth, and the fifth very small. Scales in fourteen rows. A large preanal plate. Tail flattened below, entering total length about fifteen times.

Color very light brown above, whitish below. Total length 235 mm.; tail 12 mm.

I found the specimen above described in a road at the silver mines at Lake Valley, southern New Mexico.

The appearance of this species is so similar to that of the *G. dulcis* that I originally identified it with the latter. It is, however, very different, especially in the number of labials, and the scales which adjoin the postocular posteriorly. There is no plate comparable to the so-called parietal of *G. dulcis*. I propose that it be called *G. dissecta*.—E. D. COPE.

On the Habits of Keen's Deer Mouse, Peromyscus keeni (Rhoads).—The following interesting notes were forwarded me by the Rev. J. H. Keen, a missionary on the Queen Charlotte Islands. The Deer Mouse referred to was originally described in the Proceedings of the Academy of Natural Sciences for 1894, from specimens procured by Mr. Keen, and forwarded to Philadelphia. The remarks on the use of the cheek pouches for the conveyance of food are of particular value. It has been known for many years that several species of this genus possessed cheek pouches; but I can remember no personal observation of their use by the living animal, having been published.—SAMUEL N. RHOADS.

Acad. Nat. Sci., Phila., July 16, 1896.

MASSETT, QUEEN CHARLOTTE ISLANDS,
BRITISH COLUMBIA, February 22, 1896.

Samuel N. Rhoads, Esq., Philadelphia, Dear Sir:—The following notes I have made lately on the character of *Sitomys keeni* may be of interest. Use them as you think best.

Sitomys keenii is the common house mouse here, and specimens are very numerous. I recently confined an adult female in a fairly spacious cage with glass front, I subsequently introduced three other nearly adult specimens. At first the old female resented the intrusion, but soon became reconciled. The younger ones may have been her offspring, having been taken in the same place. On two other occasions I introduced an adult male taken in another locality, whereupon the old female in each case attacked the intruder fiercely, chased him all over the cage till he was exhausted, and then flew at his thoat and bit him so severely that he died almost immediately. A shrew introduced later she treated in the same manner.

After a couple of days they became reconciled to confinement, and indifferent to being watched whilst feeding or at play. They ate bread moistened with milk and raw potatoes, but showed a marked preference for wheat. The wheat they never ate on the spot, but filled their pouches with it, and ascending a sloping board deposited it in their sleeping place. This they did with great rapidity, and a handful soon disappeared. The average number of corns taken at one mouthful was ten, but once or twice the old mouse took as many as sixteen. The first few corns they took up with their mouths, but used their feet to cram in the rest. When their pouches were full their heads were twice their normal size, and their expression extremely droll.

The storing propensity is evidently very strong. It is quite a common occurrence to find any empty article, which has been unused for a few days, half full of rice or corn when next taken up. Boots and shoes seem to be the favorite storing places; but a neighbor of mine, on visiting his outhouse, found the oven of a disused stove half full of rice, which had been obtained from sacks close by.

When in a trap, frightened, these mice sound an alarm by suddenly contracting the nails of the fore-foot so as to cause a sharp scratch on the floor. This they repeat several times, using sometimes one fore-foot, sometimes the other, but never the hind-feet.

Sitomys keenii is the only mouse here; *Mus musculus*, though on the opposite mainland, not having yet found its way thus far. In the summer of 1894 *S. keenii* was unusually numerous. An Indian crossing to

the mainland observed one in his canoe when in mid-ocean, and on reaching mainland saw it jump ashore and escape.

Yours faithfully, J. H. KEEN.

The Inheritance of an Acquired Character.—*Editor American Naturalist:* It has been my fortune recently to have brought to my notice an instance illustrating Darwin's theory of the origin of species, that seems to me noteworthy.

A certain Mr. J. B. Perry, a resident of Cleveland, is the owner of a very fine female fox-terrier, which has recently given birth to a litter of seven puppies, five of whom are remarkable from the fact that they were *born with short tails.*

These five were male puppies, while the two with tails of ordinary length are female.

Of the five short-tailed dogs one has almost no tail at all, it being but a little stump not over half an inch long.

When I examined them they were just two weeks old and barely had their eyes open. The tails of the females had been recently cut, and the scar on the stump was plainly perceptible, while the ends of the five short tails were entirely grown over with hair, and plainly were born in the condition I found them. Their length was about half the ordinary length, or about what is considered the "proper thing" by dog-fanciers, except in the case of the one already mentioned as having almost no tail at all.

When it is remembered that the custom of cutting off over half of the caudal appendage of the fox-terrior has prevailed for many generations back in the ancestry of a thoroughbred, the birth of short-tailed dogs is not to be wondered at. Yet this instance is so striking that it seems worthy of being brought to the notice of the readers of the AMERICAN NATURALIST.—NORMAN E. HILLS.

The Hartebeest (Alcelaphus).—This large genus, despite its number of species, is sharply defined, and though at first sight the caama and tsessebe, bontebok and Hunter's hartebeest, seem very indifferent, yet they possess horns of such a characteristic type, have features and habits so much in common, that it seems a useless multiplication of names to separate this genus into subgenera.

Although to us the caama is the best known species, at once arresting our attention by its ugliness, yet the earliest known kind was the bubaline hartebeest of the north, the bukyel wäsh, as the Arabs call it. For, on the Egyptian monuments there often appears the figure of an ox with unmistakable hartebeest horns, harnessed to the chariots of the

kings; and, since all the hartebeests can be readily domesticated when caught young, we conclude that in the days of the Pharoahs they actually broke in the hartebeests as beasts of draught. The Dutch name implies stag-ox, so that the old settlers may have done the same, unless the Zulus brought the Arabic name down with them, and it was then translated by the Boers into equivalent Dutch.

The Caama or true South African hartebeest is, as Cornwallis Harris remarks, made of triangles. The male stands five feet at the withers, and nine in extreme length. The crupper is drooping and the shoulder elevated; the head heavy, narrow, long. The horns are seated on the summit of a beetling ridge of bone on the forehead, almost touching at the base, thick, diverging and again approaching, turned forewards and then acutely backwards, with points directed horizontally to the rear. The surface of the horns is embossed with five or six prominent knots on the front only. The neck and throat are bare, with no mane. The coat is short and glossy; color, bright orange-sienna with a crimson cast. There is a black patch at the base of the horns above the forehead, continued behind, and terminating in front of the ear. A black streak down the nose, and a black stripe down the ridge of the neck. Chin black. A black line down the front of each leg, terminating in an angular band above the fetlock. Tail reaching to the hocks, with backwardly directed glossy black hair. Legs slender with taper hoofs. Ears whitish, long, pointed and flexible. A half-muzzle dividing the nostrils; nose flattened, moist. Eyes high in the head, wild, and of a fiery-red color. Female with more slender horns, and fainter in color. Two mammæ. Young born singly in April and September.

The hartebeest is found in small flocks, headed by three or four stout males, the weaker being expelled and forced to establish a community of their own. In fighting they drop down on their knees, and, placing their forehead parallel with the ground by putting their nose between their legs, butt each other with intense fury, their gnarled and angular horns interlocking, and inflicting gaping, jagged wounds. A common habit is to rake their horns against the trees until they are covered with bark.

In running the caama has long, *oily*, and beautiful paces, which are of the most approved racing style. Moving at a smooth and swinging canter, throwing its hind quarters well under the body, brandishing the glossy tail, and carrying its great beamy head in the best possible manner, it cuts a very majestic appearance, notwithstanding its angular build. So swift of flight is it that the hunter is again and again disappointed when trying to chase it on horseback; but in and around

Vryburg, and near to the farms in Bechuanaland, it has become so used to the sight of man who protects it, that it no longer regards him as an enemy. Sir C. Harris, however, saw him in a more unsophisticated state, for, he says that, when followed, the caama frequently stops, and turning proudly towards the foe with a most sapient look, sneezes with great violence, an act of overt folly, so much so, indeed, that it would appear to be playing a game of hide-and-seek with the hunter, ever peeping at him from behind the trees.

The flesh is dark and venison-like in appearance, but somewhat tasteless. The skin is in much request among the Bechuanas for karosses.

The caama is very liable to a terrible scourge that affects most of the big game of South Africa. It originates in a kind of bots, probably the larvæ of an Oestrus, which force their way into the nostrils, and the head becomes literally crammed with maggots, numbers of which are expelled in the process of sneezing.

The bontebok and blesbok bear to each other the closest resemblance, being equally robust, with the same hump on the back just behind the neck, the same broad nose, characteristic indeed of the whole Alcelaphine division, and finding its greatest expression in the wildebeests; and, as Harris says, both have the same fine, venerable, old-goatish cast of countenance. The lyrated horns are placed vertically on the summit of the cranium, those of the bontebok being jet black, whereas they are light brown in the blesbok. They have in common the snowy white blase on the nose; the belly is white; and the back hoary and glazed, as though they wore saddles. They are equally addicted to the use of salt, which occurs abundantly in the form of an efflorescence in the Kalahari, and both scour against the wind with their square noses close to the ground, as though they were running scent. The bontebok now survives only on certain farms near Cape Agulhas, but the blesbok has a more northerly range, and formerly existed in great numbers in the Free State and Transvaal.

In the country of the Tamboukies, immediately beyond the eastern frontier of Albany, there exist boundless billowy successions of surge-like undulations, known appropriately as the Bontebok flats; whether the "painted-goat" ever existed there is problematical, but the blesbok used to be shot there in considerable numbers. These two bucks stand out from the rest of the hartebeests by their violet and chocolate coloring; their height is about 3 feet 8 inches, and length 6 feet 4 inches, but animals of this stature are seldom found now.

The sassayby, or, as Livingstone called it, the tsessebe, is found north of Lake Ngami. It stands 4 feet 6 inches at the withers, and some 8

feet 3 inches in length. Horns robust, turning outwards, forming a complete crescent when looked at from before; some 12 or 15 annuli on the lower half, upper half smooth; the characteristic hartebeest zigzag is only faintly reproduced. Selous has noted a hybrid between the tsessebe and the caama. Herr Matchie's *Damalis jimeru* is not clearly separated from the tsessebe. The most characteristic feature of this species is the slate-colored markings on the sides of the shoulders and flanks, while the general color is brown, fulvous or tawny.

Of the North African forms we can only mention here Hunter's hartebeest, which has a much shorter face than the typical caama. It stands some 4 feet at the withers, and is of a uniform chestnut brown, with white tail and belly. A white chevron stretches between the eyes. The horns are inclined outwards at the base, and then run vertically upwards, the greater part being quite smooth; length round curve, 26¼ inches. (The Scientific African, February, 1896.)

Zoological News.—The material obtained by deep-sea dredging in the gulf off the coast of Cape Breton includes many animals hitherto considered as exclusively Mediterranean as to habitat. In view of the importance of this discovery, M. De Folin (de Biarritz) has prepared a catalogue of the species found in the collections, the first installment of which is published in the Revue des Sciences Nat. de l'ouest, April, 1896.

ENTOMOLOGY.[1]

Fossil Cockroaches.—Mr. S. H. Scudder's studies of the American Fossil Cockroaches have recently been published by the U. S. Geological Survey (Bulletin 124). Most of the forms figured and described are from the paleozoic fauna. While, in 1879, only seventeen species of cockroaches belonging to this fauna were known, there are 132 species now described.

Dr. Packard's Monograph of Bombycine Moths.—In the important memoir recently published by the National Academy of Sciences, Dr. A. S. Packard embodies the results of many years work upon the Bombyces. The volume contains about 300 quarto pages and 50 plates, many of the latter being beautifully colored. The scope

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

of the memoir is shown by the following list of contents: Introduction; Evolution of bristles, spines and tubercles of caterpilars; External anatomy of caterpillars; Incongruence between larval and adult characters of Notodontians; Inheritance of characters acquired during the life-time of Lepidopterous larvæ; Geographical distribution; Phylogeny of the Lepidoptera; Classification of the Lepidoptera; Nomenclature of wing veins; Systematic revision of the Notodontidæ.

In classification Dr. Packard adheres to the lines of the paper he recently published in the NATURALIST. The discussion of acquired characters is one of the most interesting parts of the book and is well worth reading by biologists generally. The volume is an extremely notable contribution to the literature of American entomology.

Grape Insects.—Mr. C. L. Marlatt contributes to the recent Year Book of the Department of Agriculture a valuable discussion of the Insect Enemies of the Grape. In the introduction he says: Upward of 200 different insects have already been listed as occurring on the vine of this country, and the records of the Department alone refer to over 100 different insects. Few of these, however, are very serious enemies, being either of rare occurrence or seldom numerous, and for practical purposes the few species considered below include those of real importance. They are the grape phylloxera, the grapevine fidia, both chiefly destructive to the roots; the caneborer, destructive particularly to the young shoots; the leaf-hopper, the flea-beetle, rose-chafer with its allies, and leaf-folder, together with hawk moths and cutworms, damaging foliage, and the grapeberry moth, the principal fruit pest.

The extent of the loss that frequently results from these insects may be understood by reference to a few instances. The phylloxera, when at its worst, has destroyed in France some 2,500,000 acres of vineyards, representing an annual loss in wine products of the value of $150,000,000, and the French Government had expended, up to 1895 in phylloxera work over $4,500,000, and remitted taxes to the amount of $3,000,000 more. The grapevine fidia, on the authority of an Ohio correspondent, in a single season in one vineyard, killed 400 out of 500 strong five-year-old vines. The prominent leaf-defoliators, as the rose-chafer and flea-beetle, frequently destroy or vastly injure the crop over large districts, and the little leaf-hopper, though rarely preventing a partial crop, is so uniformly present and widely distributed as to probably levy a heavier tribute on the grape in this country than any other insect.

Flower-Haunting Diptera.—Mr. G. T. Scott Elliott has made numerous observations which go to show that flower-haunting Diptera are of much importance in pollination. He thinks that his evidence clearly proves the color-sense of the Diptera observed, and also that they "are, on the whole, more intelligent than the lower class Hymenoptera." "It is to these Diptera," he says, "that we probably owe all of the neatly made, small and bright colored forms of flowers." The author gives tables showing the visits of about sixteen Diptera to various types of flowers, and compares these with the visits paid by Hymenoptera. He suggests that the Diptera map out the ground as vultures do, and keep flying up and down over a chosen area. At the beginning of his paper,[2] there is an interesting note on the part which insects play in *isolation*. Thus if flowers of the same species occur partly inside a sheltered wood, and partly outside, probably not more than five per cent of those outside will be fertilized by pollen from those inside the wood and *vice-versa*. This means for reproduction almost perfect isolation.—*Journal Royal Micr. Society.*

Larval Habits in Panorpa.—Dr. E. P. Feldt contributes to the tenth report of the State Entomologist of New York an important paper on Scorpion fleas from which we quote the following relating to *Panorpa rufescens*:

"Throughout their different stages, the larvæ usually harmonize with their surroundings so closely that it is difficult to detect them. Frequently a slight motion of the earth is the first indication of their presence. They burrow in the earth and remain underground much of the time. Many burrows ran less than one inch below the surface, although a few extended to a depth of three or four inches. The larvæ may be fed readily upon raw meat placed upon the surface of the ground. Some time after placing the meat in the cage, they may be found under it, frequently in a more or less cell-like depression. When in such a position they rarely try to escape, but trust to their protective resemblances, and remain motionless. Around the edge of the piece of meat and also under it, the mouths of burrows may be seen and in them the heads of larvæ; when in such positions they dodge back quickly at the least disturbance. Unless the meat is moved very cautiously the burrows will appear empty; but if quiet is maintained for a few moments, the heads will soon be seen. The burrows opening under the meat frequently come to the surface a little distance away, and it is quite easy to drive a larva out of its back door. Not infre-

[2] Trans. Ent. Soc. London, 1896, pp. 117–118.

quently they have been observed to emerge from a burrow for their feeding. This usually occurred in the afternoon. On one of these occasions a little fellow was watched through a simple lens. It was interesting to see him bite off a piece of meat and swallow it with every evidence of satisfaction. The antennæ were moved back and forth in the most appreciative way. As the larvæ increase in size, more burrows open upon the surface and they are seen lying at their mouths. One time two were seen out of adjacent burrows. The larger seized the smaller in the back and tried to drag it down into its burrow. The smaller was unable to escape, and when it was pulled away with forceps the body-wall was ruptured. At another time a smaller active larva was seen to attack a larger inactive one, which, unable to resist, was bitten so severely that the segment swelled considerably, but was not ruptured. In a day or two the larger died and was fed upon by its former persecutor.

EMBRYOLOGY.[1]

The Wrinkling of Frog's Eggs During Segmentation.—The occurrence of wrinkles in frog's eggs during the process of segmentation was first observed and very briefly described by Prevost and Dumas, who have the honor of being the first observers of the segmentation itself (Annales des Sciences Naturelles, I ser., 1824, Tom II, p. 110).

A somewhat better description was given later by Bär (Archiv. für Anatomie, etc., 1837) and Reichert (the same, 1841), who gave to the phenomenon the name "Faltenkranz," and made some attempt to explain its nature and origin.

By far the best description, and, indeed, the only really good one which has ever been published, is that by M. Schultze, which appeared in 1863 (Observationes nonnullæ de ovorum ranarum segmentatione, Bonnæ).

He gives an excellent account of wrinkles observed in the eggs of *Rana temporaria* and *R. esculenta*, and concludes with an "explanation of their origin." But he really devotes only a very few lines to the explanation, and gives up the remainder of this portion of his paper to

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

a controversy with Reichert, over the existence of a vitelline membrane on the surface of the egg.

Similar wrinkles have since been observed in the eggs of the common toad by Goette (Die Entwickelungsgeschichte der Unke), and of *Amblystoma punctatum* by Eycleshymer (Journal of Morphology, Vol X). But neither of these authors offer any explanation of their origin. So far as known this "Faltenkranz" has never been described for any American species of frog, nor has any attempt been made to study it by means of serial sections.

The following paper is offered as a contribution of some new and interesting details in the occurrence of the phenomenon, together with the results of a microscopic study of sections, in the hope of arriving at a rational conclusion as to its origin.

The author desires to acknowledge his great indebtedness to Dr. E. A. Andrews, for the suggestion which led to the study, and for much subsequent assistance. Thanks are also due to Prof. T. H. Morgan, for the kindly loan of a copy of Schultze's paper.

FORMATION OF THE WRINKLES.

The eggs of a small wood-frog, in all probability *Chorophilus triseriatus*, were obtained for class work on March 27, 1896.

They were unsegmented when found, and were immediately placed in ice water to check any further development.

After remaining thus for five hours they were used in the laboratory, being removed to watch-glasses containing tepid water. Some were allowed to remain in the ice water for eight hours before being examined, and it was noticed that these segmented much more rapidly than the ones which had been kept only five hours. Actual segmentation had been prevented during the stay in the ice water, but there seemed to have been a storing-up of energy, a sort of preparation for segmentation, so that when removed to a favorable environment the process began very quickly (5–10 mins.) and was carried on much faster than it would have been normally.

These eggs were obtained from pools covered with ice quarter of an inch thick, and most of the bunches were quite near the surface.

This must occur frequently where the eggs are laid so early in the spring; and according to Morgan (AMERICAN NATURALIST, August, 1891), Chorophilus always lays its eggs very early.

The storage of energy noticed above may suggest a natural method of compensation whereby the warm sunshine of mid-day may offset the freezing cold of the nights, and in this way the eggs will really lose very little time in their development.

O. Hertwig and Schultze have recently experimented on the influence of a very low temperature upon the development of the eggs of *Rana fusca* with very different results.

Hertwig (Sitzungsberichte der König, Preuss. Akad. d. Wiss, 5 April, 1894, p. 313) found that freshly fertilized eggs were injured by an exposure to a temperature of 0° C. for 24 hours. On being raised to the ordinary temperature a portion were developed very much more slowly than normal eggs, while in the remainder a part of the yolk was found incapable of division.

Schultze (Anatomischer Anzeiger, X Band, No. 9) subjected eggs of the same species to a temperature of 0° C. for 14 days, and then obtained perfectly normal embryos from them. These eggs had reached later stages of development before being cooled, and he does not state whether their subsequent development was more or less rapid than ordinary.

Subjection to a temperature of 0° C. for so long a period would probably have a very different effect from that of only a few hours duration.

Loeb and Norman (Archiv. f. Entwick, III Band, No. 1), experimenting on the eggs of the sea-urchin, Arbacia, found that when put in sea water to which had been added 2–3 per cent. NaCl or $MgCl_2$ segmentation of the protoplasm was wholly prevented, but that the nucleus went on dividing. On being put into normal sea water after a few hours exposure to the concentrated solution the eggs divided at once into several cells, the protoplasm merely rearranging itself around the new nuclear centers.

Possibly the same may be true in these frog's eggs when subjected for a short time to a freezing temperature, and the subsequent hastening of segmentation may be due to the fact that the nucleus has already divided.

In watching the segmentation of these eggs the great (comparative) size and depth of the furrows was specially noticed, together with the distinctness of the wrinkles formed along either side of them.

Accordingly it was determined to study their segmentation more in detail, and a fresh lot was obtained the next morning. These remained in ice water only one hour, just long enough to get them home. They were then transferred to watch-glasses and examined in strong sunlight. This fact must be taken into account in connection with the time periods given.

The first furrow appears at the superior pole without any previous flattening as in Amblystoma (Eycleshymer, Jour. of Morph., X, p. 348). At first it is a shallow groove just at the pole itself, but it soon spreads

over the pigmented hemisphere. The ends may progress at a uniform rate, or one may exceed the other in rapidity as in Petromyzon (Eycleshymer, loc. cit.).

At the first appearance of the furrow it is very shallow and perfectly smooth, and extends about 0.2 mm. on either side of the pole. It begins to deepen in two or three minutes, and at the same time minute wrinkles appear on either side (Plate I, figs. 1, 5, 7). If the eggs be placed upon a black surface in the sunlight these wrinkles are seen very distinctly as fine lines radiating from the pole (Plate II, fig. 31). The number and arrangement of these lines is not constant or definite either at this, or any subsequent period. The suggestion at once presents itself, that these wrinkles may be the foreshadowing of subsequent segmentations, and one egg was obtained in which the wrinkles seemed especially significant in their arrangement (Plate II, fig. 32). The entire lack of regularity in the size and arrangement of the wrinkles would, however, preclude any such idea, since the subsequent segmentations are very regular.

As the furrow gradually deepens and extends toward the yolk two changes are noticed :

1. New wrinkles appear along its sides ; these do not all radiate from the pole, but are inclined toward it at greater or smaller angles (Figs. 6, 8, 11).

2. The radial wrinkles first formed at the pole change considerably. Very fine and delicate at first they coalesce gradually into a few larger and deeper ones, which are sharply defined.

These fused ones may or may not occupy the position of one of the antecedent ones (Figs. 2, 3, 33, 34, 35). As the furrow progresses the number and position of the wrinkles changes constantly, new ones being formed and old ones disappearing. This is especially true of the finer ones ; some of the larger fused ones near the pole remain quite constant (Figs. 10–15).

This continual changing is best seen by making sketches of the wrinkles with a camera lucida at short intervals, as in the movements of the pseudopodia of Amœba (Figs. 1–4 and 10–15).

The whole appearance thus far is exactly as if the egg were covered by a very thin, but firm membrane, which was gradually pulled in toward the center at the groove. The remainder of the sphere being perfectly even, and with no chance for "give" at any point, in consequence of the uniform tension, the edges of the groove would necessarily be wrinkled, the wrinkles becoming more and more prominent as the groove deepened. The whole process takes place so slowly that the

most careful scrutiny fails to detect the actual motion, or to see evidences of any movement of the protoplasm within the egg, which might cause the wrinkling.

The furrow at the pole has become quite deep by the time its ends have reached the equator, in five or six minutes. The ends seem to stop here for a time, just at the border of the yolk area, while further changes take place in the pigmented portion. First the two edges of the groove approach each other at the pole, and seem to fuse slowly, the wrinkles entirely disappearing during the process. This fusion then extends in either direction along the groove for some distance, often half way to the equator, obliterating the wrinkles as it goes. By this means the groove may entirely disappear at the center while remaining near the periphery (Fig. 38).

At this point the furrow begins to enter the yolk area on either side, and at the same time the groove reappears along the center of the pigmented hemisphere. It now becomes very broad and deep. Indeed, it seems to reach clear through to the yolk, and its walls are considerably rounded on either side. But they are now smooth, so that the wrinkles remain in all fifteen or twenty minutes on the first furrow.

This first furrow divides the egg into two nearly equal parts (Figs. 3, 12, 28, 35). When its ends first reach the yolk area, where they stop for a time, as already noted, the two blastomeres thus formed are very much rounded at their ends, and diverge strongly from the groove. This is readily seen in the series given in figs. 1 to 4, but it becomes much more prominent after the reappearance of the groove—(Figs. 28 to 30). Under a higher magnifying power a wide space can now be seen at either end between the two segments. The floor of this space is triangular in shape and doubly curved, being concave from side to side, and convex antero-posteriorly. It is formed of light colored yolk, into which the pigment shades gradually around the borders. The two segments are thus rounded in a manner very similar to that of the first two blastomeres of a meroblastic egg.

As the furrow proceeds toward the inferior pole the space between the two pigmented segments diminishes, the borders of the furrow approach each other, and the surface of the egg becomes smooth once more, with the groove indicated merely by a narrow, faint line.

It remains in this condition some eight or ten minutes before the second cleavage begins, and this may be called its resting stage. The rapid closing of the groove previous to the appearance of the second furrow occurs also in Amblystoma (Eycleshymer).

The Second Cleavage.—The second furrows begin about forty or forty-five minutes after the first. They start *from the equator* and move toward the dark pole, and not vice versa as in Rana, Amblystoma, Petromyzon, and most of the amphibians. This was without exception in all cases noted (some thirty-five or forty). They form a right angle with the first groove, but do not always meet it at the same point (Figs. 19, 20). They are attended with the same appearance, fusion, and gradual elimination of wrinkles as the first furrow, with the slight difference that the wrinkles start at the equator instead of the pole, and are never quite as large. This is shown in figs. 16 to 18, in which both the wrinkles and the furrows can be seen approaching the pole.

The second furrows seem to reach the first over the dark hemisphere before they start toward the inferior pole. They start at about, but not necessarily exactly, the same time.

Here, too, there is the same rounding of the pigmented segments, though to a much less degree. This may be due to the fact that in this instance the furrows start from the periphery, and consequently the segments are more sharply defined there.

The Third Cleavage.—Half an hour after the appearance of the second set of furrows, and sometimes before they have reached the inferior pole, there are indications of the first horizontal cleavage. As seen from figs. 21–24 this occurs very much nearer the superior pole than the inferior. The furrows, accompanied as before by wrinkles along their edges, as seen in the figures may start in each segment either from the first or second vertical, usually, however, from the second (Fig. 22).

They move much more rapidly than the preceding cleavages, and the entire process is completed in fifteen to eighteen minutes.

In all the eggs observed the grooves started usually at the second vertical in the two quadrants on the same side of the latter, and moved around toward each other at the first vertical.

They seldom met at the same point, but as a result of their formation the superior region was divided into four quite equal and nicely rounded cells, much smaller than the four inferior ones.

The only wrinkles formed upon the yolk area that could be detected by the most careful examination are the few which occur on the yolk side of this first horizontal groove. Schultze failed to detect any wrinkles whatever along this horizontal furrow, although he describes the other details with great exactness. This is all the more striking, because he both observed and figured them upon subsequent furrows up to the

32-cell stage. Other authors, with possibly a single exception, make no mention of them, except along the first furrow.

This confining of the wrinkles almost exclusively to the pigmented area is manifestly connected with the different organization of the two halves of the egg. The pigmented half is richer in protoplasm, and is to a higher degree under the influence of the cell nucleus; while the yolk has its protoplasm scattered about amongst the yolk granules, and is also further removed from the nucleus which lies in the pigmented half in the undivided egg (Hertwig). This results in the more rapid segmentation of the pigmented cells, and the presence of the wrinkles seems intimately associated with this rapidity of segmentation.

The Fourth Cleavage.—This appears from fifteen to twenty minutes after the third.

In this cleavage also the furrows started in every instance *from the periphery* of the four superior quadrants and move toward the pole, accompanied by wrinkles. These latter are now much smaller than heretofore, and are not easily detected under a low power. They are also very transitory, and disappear almost immediately. There is somewhat of a tendency in these furrows to run nearly parallel to the first or second vertical, recalling the conditions in teleosts (Figs. 26, 27).

There is a subsequent fusion and elimination of the furrows after each cleavage, as has already been noted in the first segmentation.

This elimination of the grooves, due to the fourth cleavage, leaves the pigmented pole of the egg divided into four cells of a totally different shape and arrangement from that of the four blastomeres resulting from the third cleavage (Fig. 37). After remaining thus for several minutes the furrows reappear, and the cells resume the shape seen in fig. 36.

From this point segmentation proceeds very rapidly, and in a manner exactly similar to that of other frog's eggs. The wrinkles have now become so small as to be seen only with the greatest difficulty and under a high power, and they disappear so quickly as to easily escape detection. But they are present at least up to the 128-cell stage, and appear, fuse, and disappear, as in the first cleavage.

Gastrulation begins about twelve hours after the first cleavage, and the blastopore closes at about the fifteenth hour. The neural folds appear and gradually fuse to form the neural canal as in Rana. By the end of the first day the embryo has elongated considerably, and the head is well differentiated.

The tail then becomes defined, the gill folds appear, and the eyes are seen as two minute black spots. And by the end of the third day the embryo escapes from the egg envelopes and swims about freely.

The yolk sac seems unusually large, and the tail is comparatively long at this period, but otherwise these tadpoles are externally like those of Rana and Hyla.

NATURE AND ORIGIN OF THE WRINKLES.

If the eggs be preserved during the first segmentation while the wrinkles are still present, and then sectioned parallel to a plane tangential to the superior pole, considerable additional light is thrown upon this process of wrinkling.

As has been noted in the eggs of other frogs the pigment is gathered into a thin surface layer over the superior hemisphere.

Houssay states that the pigment does not characterize this pole, but only happens to be there on account of the coincidence of its density with that of the surrounding protoplasm (Etudes d'Embryologie sur les Vertébrés, Archiv. de Zoöl. Exper., 1890). However this may be, during segmentation pigment appears along the sides of the furrows, so that eventually the resultant cells come to have a more or less definite pigment layer around their periphery, Houssay accounts for this by saying that there is an intimate relation between the activity of the cell and the presence of pigment. When the resting cell becomes active its granules become smaller, and pigment appears in them as the result of chemical action.

The presence of pigment, therefore, is the result of an increased activity in the cell.

But Bambeke tells us that "the cortical layer, when it enters the interior of the protoplasm, is not entirely employed in limiting the spheres of new formation. In fact, I find masses of pigment whose presence can only be explained by considering them as debris from the cortical layer, which has penetrated into the protoplasm" (Fractionnement de l'Oeuf des Batraciens, Archiv. de Biologie, Vol. I, p. 346, footnote).

An examination of sections of these Chorophilus eggs shows a similar occurrence.

In addition to the pigment layer which borders the first segmentation furrow, and which is somewhat thicker near the centre of the section, there is also a lunate mass extending downward vertically from the superior pole on either side of the furrow (Fig. 39) and in immediate contact with it. This mass can be traced in the sections from the surface layer, in which it has very little area, down somewhat beyond the bottom of the furrow, where it spreads out laterally and is lost in the surrounding protoplasm. In this particular egg the mass does not

extend quite to the level of the nuclei, the position of the latter in the figure having been taken from subsequent sections in the series. This mass, however, is not to be regarded as "debris," but its presence is due to a definite cause to be explained later. The remainder of the section is occupied by homogeneous protoplasm filled with rather small yolk granules, and surrounded by the thin, transparent vitelline membrane.

Under a high power (Fig. 40) the wrinkles appear as deep sinuses extending obliquely into the protoplasm, and bordered by a thick layer of pigment. These sinuses are angular and irregular in outline, and often cantract at their inner ends into long, narrow slits, with rather distinct walls. We can now see what it was impossible to detect from a surface view, namely, that the wrinkles are compound.

The larger, principal ones have secondary, smaller ones extending outward from their sides, approximately at right angles. Schultze observed and figured these compound wrinkles in his surface views of R. temporaria, and adds another detail which I have been unable to find in the Chorophilus eggs, viz., the breaking-up of a single wrinkle at its peripheral end into several smaller ones arranged radially from a common point.

The pigment usually fills the projecting protoplasm between adjacent sinuses. It is also much thicker in the region of the wrinkles than elsewhere along the furrow, as can be seen in fig. 40.

In view of these different facts, therefore, it seems evident that there is an intimate relation between the wrinkles and the pigment—and that both may be results of the same cause. It remains to ascertain what this cause is, if possible.

According to Schultze the egg is a single cell, and just as cellular division is brought about by the contractility of protoplasm, so is the segmentation of the egg due to the same cause. These contractions originate at the point where the furrow begins, and are at first confined to a very small area. Since the cortical portion of the egg protoplasm possesses a glutinous consistency, it is not to be wondered at that folds or wrinkles appear at the same time with the furrow, in its immediate vicinity, and at right angles to it. These subsequently disappear in consequence of the difference in contractility between the outer and inner protoplasm, due to their different consistency.

It is exceedingly difficult to understand how compound wrinkles, of such a nature as we have just described, could be produced by the simple contraction of a viscous cortical layer of protoplasm, especially if that contraction *starts from a fixed* point in the layer. Indeed, how

could it produce any wrinkles at all in the layer itself? Would it not rather tend to flatten the superior pole and stretch the viscous layer tightly over the underlying protoplasm in a manner similar to the action of the muscles of the diaphragm during respiration?

As to the difference in contractility between the outer and inner protoplasm, it is evident that if this is to smooth out the wrinkles, the difference must be in favor of the outside layer, and also the contraction must be *at right angles to the length of the wrinkles.* That means in the present instance that it must be parallel to the first furrow. Neither of these conditions seem possible, and we thus find both explanations inadequate when confronted by the facts in the case. They both fail to account for the mass of pigment under the superior pole also.

Bambeke states that "the entrance of cortical pigmented masses into the interior of the egg . . . can only be explained by admitting the existence of contractions in the ovular protoplasm during segmentation."

According to the interpretation of the present day, cell division is brought about by means of some force or forces acting along the length of the segmentation spindle. In the present instance this spindle was formed between the two nuclei represented in fig. 39, which lie some distance below the surface of the egg.

If we interpret Bambeke's "contraction of the ovular protoplasm" to be identical with this working force of the segmentation spindle, it will explain the presence of the lunate mass of pigment directly over the spindle, and will help us to understand the presence of a pigmented layer on either side of the segmentation furrows. But it does not explain in any way the formation of the wrinkles.

Reichert's wonderfully inconsistent explanation of the origin of the wrinkles is quoted, and sufficiently commented upon by Schultze, in the paper already referred to.

In view of the fact, therefore, that we have no explanation which can stand the test of our present knowledge of cell division, we venture to offer the following:

We agree with Schultze that the external pigmented layer is necessarily somewhat denser that the internal protoplasm.

Modern research indicates that this layer is drawn inward in some way, by the forces working along the segmentation spindle, to form the furrow which lies over the equator of the spindle. The bottom of the furrow, thus formed by an infolding of the surface layer, describes an arc which becomes shorter and shorter as the furrow deepens.

This shortening of the arc must result in one of two things. The bottom of the furrow may remain of the same length as at first, and

make up for the shortening of the arc by protruding a little from the surface of the egg at either end of the furrow. Such a condition is admirably shown when one creases the top of a soft felt hat, and would necessarily be even more manifest if the hat were filled with a viscous fluid.

An examination of any of the surface views will show that this is not the case with these Chorophilus eggs, but that the first indication of the groove at either extremity is a slight hollowing in of the surface, and not a bulging outward. This view is confirmed by a study of the sections.

The other alternative is that the shortening of the arc must result in longitudinal condensation along the bottom of the furrow, starting at the center and increasing as the furrow progresses.

Such a shortening or contraction along the bottom of the furrow would very naturally throw its sides into folds or wrinkles at right angles to its length. The pigment layer in contact with these walls would also be thickened in the region of the wrinkles, and toward the center of the groove.

Since the condensation starts at the center and advances in both directions with the progress of the furrow, the wrinkles would be arranged somewhat radially about the superior pole. This progressive contraction also accounts for the successive appearance and disappearance of wrinkles, and for the confluence of smaller into larger ones.

As the sides of the furrow begin to fuse into the permanent segmentation plane or cell-wall the wrinkles disappear through the gradual readjustment of former relations.

Indeed, the whole phenomenon seems very largely dependent on the rapidity of segmentation and the consequent sudden disturbance of normal relations before the different portions can adjust themselves to their new conditions.

This fact will serve to explain why the wrinkles show so prominently in this particular species, which has a very rapid development, and also why the conditions under which they were examined—the transference from ice to tepid water and the placing of the eggs in strong sunlight—were especially favorable.

It may also suggest a reason why one observer has failed to detect wrinkles in the eggs of a given species, while another, working under more favorable conditions, has seen and described them. And it will in a measure account for the absence of wrinkles on the yolk hemisphere, since segmentation is very much slower there.

If our explanation is a correct one there ought to be no wrinkles at all along the bottom of the groove, while they should be present and have their greatest depth about half way between the bottom and the surface.

The sections show that this actually occurs. Fig. 39 is a section cut just at the level of the bottom of the groove, and shows no trace of any wrinkles, nor are there any in the two or three preceding sections.

They then appear and gradually increase in size up to the level of fig. 40, which is a magnified portion of the same groove about half way to the surface.

The problem of the compound nature of the wrinkles finds its solution in the fact that there must be a condensation along the bottom of the larger wrinkles, in all respects similar to that in the groove, and due to the same cause, though, of course, on a very much smaller scale.

But in this instance the condensation would proceed in only one direction, and hence we find the secondary wrinkles all inclined in the same direction to the principal ones, just as we have already observed, and as Schultze has so finely figured.

Summary.—1. Subjection to a temperature of 0° C. for a period of eight hours completely arrests all development for the time being, but results, on the subsequent restoration of ordinary conditions, in a cleavage more rapid than that of normal eggs.

2. Segmentation, at least up to the 128-cell stage, is accompanied by the formation, fusion and subsequent elimination of well defined wrinkles along the sides of the furrows in the pigmented area. There are no wrinkles on the yolk, except along the inferior border of the third cleavage furrow.

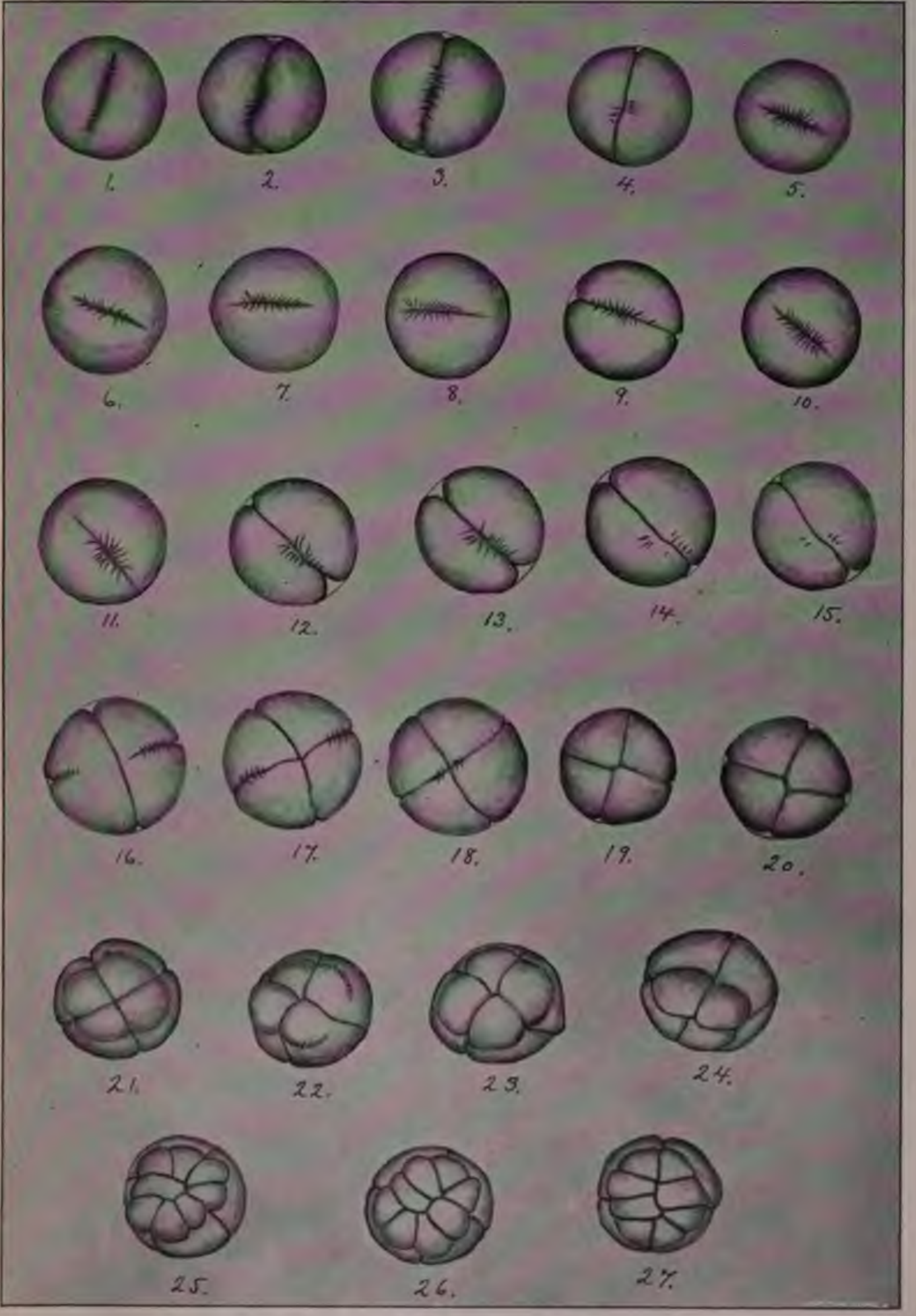
3. As seen in an examination of cross-sections these wrinkles are compound in nature, the larger, principal ones having smaller secondary ones along their sides.

4. The wrinkles on the first furrow are arranged somewhat radially about the superior pole. On subsequent furrows they are inclined at an angle toward the point where the furrow starts.

5. The pigment which borders the segmentation furrows forms a thicker layer in the region of the wrinkles than elsewhere along the groove, thus showing an intimate relation between the two.

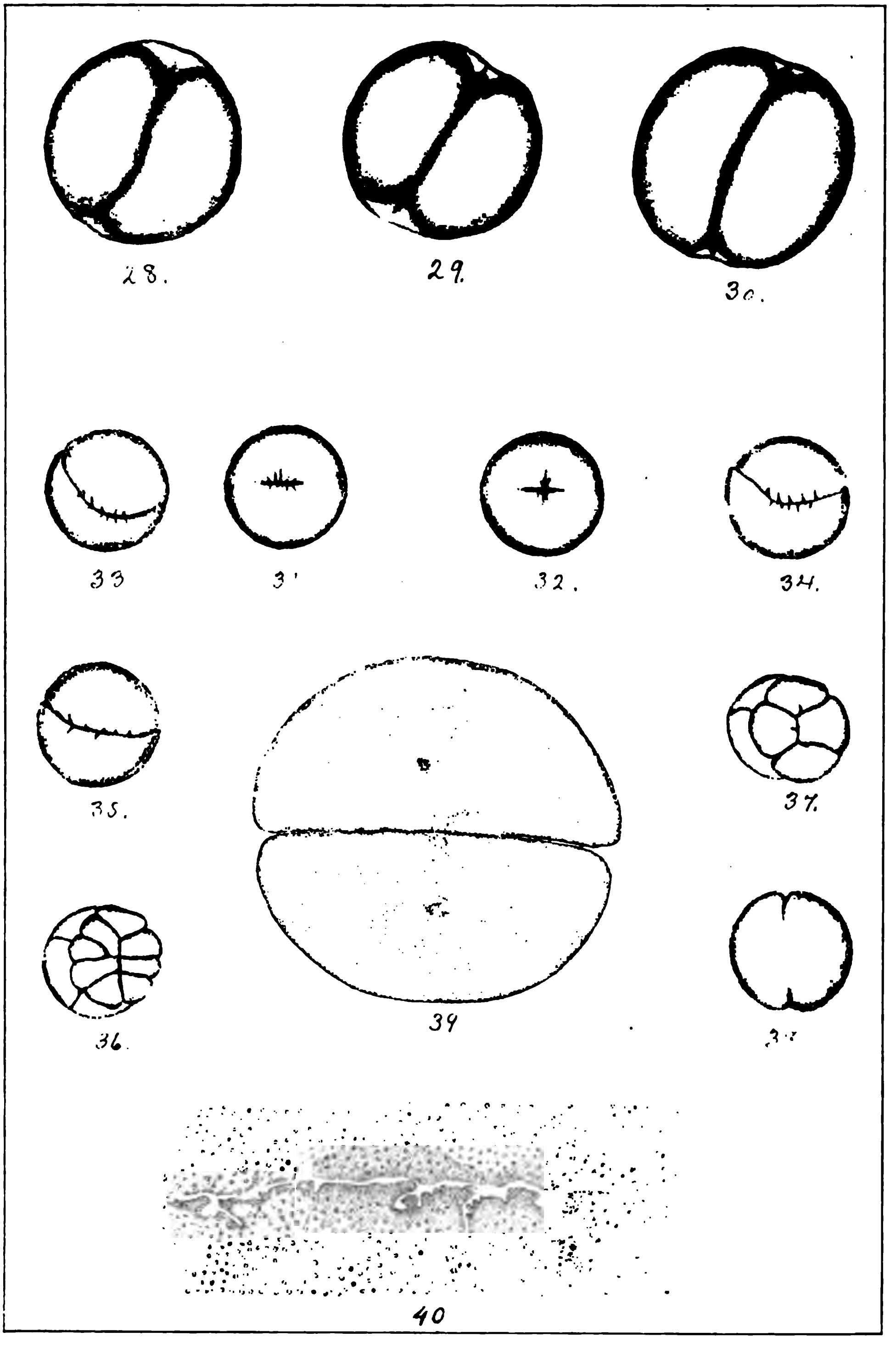
6. The probable cause of the wrinkling is to be found in the condensation along the bottom of the groove, which results from the shortening of the arc, and is a necessary consequence of the infolding of the surface layer to form the groove.

PLATE XIII.



Segmentation of Rana.

PLATE XIV.



Segmentation of Rana.

7. The second and fourth grooves *start from the periphery* and move toward the pole.

8. The blastomeres are more rounded and the segmentation furrows are deeper than those in most frog's eggs.

9. The development is very rapid; gastrulation begins within twelve hours, and the tadpole escapes from the egg during the second or third day.—CHARLES B. WILSON.

EXPLANATION OF PLATE I.

All figures drawn with Zeiss Camera x 16 diam.

Figs. 1–4. Successive stages in cleavage of living egg at intervals of 3 mins., 3 mins., 2 mins.

Figs. 5, 6. Stages in cleavage of second living egg.

Figs. 7–9. Stages in cleavage of third living egg—intervals, 2 mins., 4 mins.

Figs. 10–15. Stages in cleavage of fourth living egg—intervals, 2, 6, 3, 4, and 5 mins.

Figs. 16–18. Stages in second cleavage of third egg.

Figs. 19, 20. Variations in second cleavage.

Fig. 21. Eight-cell stage of first egg.

Figs. 22–24. Stages in third cleavage of second egg.

Figs. 25–27. Variations in sixteen-cell stage.

EXPLANATION OF PLATE II.

All figures drawn with Zeiss Camera.

Figs. 28–30. First cleavage under higher magnification to show rounded blastomeres.

Figs. 31, 32. Beginning of first cleavage, showing radiating wrinkles at pigmented pole.

Figs. 33–35. Variations in wrinkles on the first furrow.

Fig. 36. View of an egg during the fourth cleavage.

Fig. 37. The same egg four minutes later.

Fig. 38. Fusion and partial disappearance of the first groove.

Fig. 39. Horizontal section of an egg during the first segmentation, taken at the level of the bottom of the furrow. Nuclei added from the fourth section below this.

Fig. 40. A portion of the same furrow about half way between its bottom and the surface of the egg, more highly magnified.

PSYCHOLOGY.[1]

Fear Among Children.—The term *fear* is applied with some ambiguity to two distinct phenomena. The sudden and unexpected advent of danger arouses the whole organism and causes an uncontrollable excitement which manifests itself in violent agitation, momentary paralysis, or other well-known signs. While the strength and duration of the emotion depends largely upon the temperament and nervous condition of the individual, it is primarily a function of the immediate stimulus, and its basis is physical rather than mental. Chronic fear, on the other hand, is determined more by the constitution of the individual than by external stimuli, and remains present after the circumstances which called it forth are removed. It occurs in every degree, from the purely normal to the extreme pathological, as exhibited in certain forms of insanity. The normal phases of this emotion are best observed in children, where repressive self-control is less liable to interfere with its open manifestation.

Prof. Alfred Binet has recently carried out such an investigation.[2] He issued a questionary, addressed principally to school-teachers, but circulated also among parents who seemed qualified to give discriminating answers. From the nature of the case, the replies dealt generally with the more permanent form of fear (*peur* as distinguished from *crainte*). On examination of the reports, M. Binet classes the causes of fear as follows: 1. Night, darkness, solitude—the sense of mystery in things—in short, what might be termed in English the quality of *phantomhood*. 2. Loud noises, such as thunder or the report of a gun. 3. Objects which excite repugnance or disgust: small creatures, such as rats or spiders; the sight of blood or a corpse. 4. A danger, real but not hitherto experienced by the child, whose likelihood is greatly exaggerated and which preys upon his mind; thus a child may be afraid of meeting a beggar or a drunken man, of being bitten by a dog, etc. Such a feeling is generally traceable to some story, true or false, which the child has heard. 5. The memory of a severe accident or narrow escape leads to a chronic fear of its recurrence.

We may carry the analysis a step further than Prof. Binet. In 2 and 5, the distinctive element is a sudden nervous shock, with its after-

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

[2] La peur chez les enfants, *Année psychologique*, II, 223.

effects; in 3 and 4, the influence of the imagination predominates. Although the imagination is an important factor in 5, this class presents a distinct problem of its own, deeper rooted, which may lead the inquirer into the sphere of comparative psychology.

A point in the investigation most difficult to ascertain, was the proportion of children susceptible to fear. The answers returned varied widely, probably because the distinction between the two kinds of fear was not usually taken into account by the observers. The most reliable data seemed to indicate a general average of about 10 per cent; but the proportion appeared to be at least three times greater among girls than among boys. The question of temperament was investigated, but here too the answers varied considerably, and most diverse traits were included in the different lists; the only generalization that seemed warranted was the preponderance of fear among the gentle and timid,—which is, after all, not a point of startling novelty. On the intellectual side, where teachers are in a position to give trustworthy judgments, the figures indicate a slight excess of fear among the brighter, and a lower proportion among the more stupid than among the mediocre. Prof. Binet argues, however, that this is not due to a direct connection, but that the tendency to fear is increased by a vivid imagination, which is generally associated with greater intellectual capacity. On the other hand, there is a close connection between fear and the state of the health; and a nervous condition, whether due to a shock or otherwise, is fruitful soil for fear in children as in adults. But a further element must be reckoned with here, in the case of the child: for, as he grows conscious of his failing, he loses confidence in himself, and thereby becomes still more liable to fear.

Aside from the concrete causes of fear already noticed, a number of factors are concerned in its development. Heredity plays a prominent part here as elsewhere. Ill-treatment is a most effective agent in fostering it, and this heading may be extended to include the many instances of misdirected efforts to train the child which are far from wilful. The pedagogical value of the study, which M. Binet brings out in a closing section, is nowhere more marked than here. Closely allied to this factor is the influence of tragic stories and mysterious tales on the child's imagination, a principle which even judicious parents are apt to forget. Finally, the force of example—the *contagion of fear*—is shown unmistakably by Prof. Binet's study. The younger is influenced by the older, the stronger by the weaker, the child by the teacher; if the latter show signs of fear in any crisis, the former is

almost sure to give way. This is, of course, no new discovery, but it is a fact which cannot too often be emphasized.

Fear begins to be manifested between the second and third years of age, and, until about the ninth year, the child's powers of self-control are insufficient to inhibit it. Under normal conditions, it decreases rapidly from the ninth until the twelfth year, when, apart from the influence of special conditions or circumstances, it comes well under control.—H. C. WARREN.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Association for the Advancement of Science.—This body met at Buffalo, New York, from August 24th to 29th inclusive. The council met on the 22d, and the 29th was devoted to an excursion to Niagara. The attendance was not as large as sometimes, the number of members present being 333. The quality of the papers was said to have been in general excellent. Only three of the sections continued in session on Friday afternoon (the 28th), viz.: the Geological, the Anthropological, and that of Social and Economic Science. The affiliated societies coöperated to a considerable extent, the Geological and Chemical Societies reading papers in the appropriate sections, and the Entomological Club suspending its meeting. Eighty-two fellows were elected. Prof. Wolcott Gibbs was elected an honorary fellow, and Mr. Horatio Hale a fellow for life. On Wednesday afternoon a symposium was held in the Geological Section in honor of the sixtieth anniversary of the appointment of Professor James Hall to the position of Director of the Geological Survey of New York.

At the opening session the association was welcomed by addresses from Mayor Jewett, and from Dr. Park, President of the Society of Natural Sciences. President E. D. Cope replied in the following language:

"*Mr. Mayor, Ladies and Gentlemen of the Local Committee and Citizens of Buffalo:* I utter the sentiments of the American Association for the Advancement of Science in expressing our pleasure at being once again in your beautiful city. We feel at home here, and we know that we are among friends who understand our motives and our objects. But, inasmuch as we represent the entire nation, I will give a brief outline of the objects of the Association, and the aims which it has in view. Our principal occupation is that of original scientific research, although

many of us are of necessity also teachers of scientific knowledge. The primary object of the Association is, however, not teaching, but the advancement of science by the increase of knowledge. We seek to penetrate the unknown and to build up a system which will express with certainty the mutual relations of the various parts of the universe, including ourselves. Although many facts are known, and some laws have been formulated, very much still remains unknown, and many of the highest principles of nature remain undiscovered. Original research furnishes the material for teaching and the matter which is contained in books. Much money is devoted to the building of libraries and of schools, but not much is given for the purpose of supplying the knowledge which is to be taught in the schools and from which books are made.

"The motives of the original investigator vary with his years, but the taste for research is generally developed early in life. In some it is a love of the beautiful, whether it be the beauty of a perfect mechanism or the beauty of form that attracts him. In some, it is the desire to know, and in others it is a high interest in the problem of human origin and destiny. In many it is the same feeling which prompts the adventurous explorer to enter new regions, not knowing what he will find, but believing that whatever is, is right.

"The services rendered by science are twofold. They have a value either material in their character or utilitarian, or they have a mental value, inasmuch as knowledge serves to clear the mind of fears and doubts, and so to promote human happiness. The true man of science is not influenced by utilitarian considerations, but pursues the truth wherever it may lead, knowing by experience that its benefits are many and sometimes unexpected. Another benefit which the cultivation of science promotes is the formation of correct habits of thought. The rational faculty of the mind is of very ancient origin, and developed early in the history of man. But its use in the early stages of human development has been largely *a priori;* that is, in the advance of knowledge, rather than as a digestor of knowledge after its acquisition. In other words, the scientific method consists not in the use of abstract reason, but in a rational use of the results of observation and experiment. This is the lesson which the history of science teaches mankind, that if we wish to know the actual state of affairs, our course is first to observe the facts and to draw our inferences from them, and not to attempt to describe the universe from our inner consciousness as we think it ought to be. All the results attained by science have been due to adherence to this method. Nevertheless it is not forbidden to entertain hypotheses before discovery, if such hypotheses are not valued for more than they

are worth. Another service which we imagine science renders to the community is the example which it offers of the reward of labor. The scientific man loves to work not only for the sake of acquisition, but also because of the pleasure there is in work as an activity of the human organism. By it we learn that by work only can great results be accomplished, and the law of conservation and correlation or energy teaches that something cannot be made out of nothing.

"In our educational function we hope by example to show that the mental life is as worth living and affords as much pleasure as the physical life. This is a lesson on which it is necessary to continually insist, since mankind is constantly prone to imagine that mental activity and thought are uninteresting and dull. On the contrary they afford a high class of pleasures which are conservative of the entire organism.

"We also emphasize the desirability of free thought on all subjects whatsoever, and the necessary corollary that the thought shall be careful and judicial. Thought so applied to our practical affairs must be in the highest degree beneficial in every direction both personal and national. We expressly repudiate two common types of thought. One of these attempts to prove by reasoning, if not by reason, a contention in which a person has an especial interest. It is to be feared that this habit of mind is too common, and it implies a lack of honesty of purpose which is entirely foreign to the scientific spirit. The other type of thinking to which we object is the acceptance of allegations concerning matters of fact or theory upon insufficient evidence, or upon authority only. Both of these methods lead to inaccurate results, and from both the scientific method protects us. I do not hesitate to say, that the future of science will be greater than its past, and that it affords a career to those who are adapted for it which promises a high degree of happiness and benefit. I believe that in this country with our facilities in various directions, the pursuit of science will become a more conspicuous part of our national life than it is now, and I am sure that nothing is more desirable for our national life than that this should be the case. In the cultivation of science we see the cultivation of honesty, of industry and of truth, all qualities which are essential to the prosperity of a people.

"Fellow citizens of Buffalo we thank you for the very material aid which you are rendering us in the attempt to develop this enterprise."

The Nominating Committee recommended the names of Prof. Wolcott Gibbs, of Newport, for President, for 1897–8, and of Dr. Asaph Hall, Jr., of Washington, D. C., for General Secretary; who were elected. The committee also recommended that a formal meeting for organiza-

tion only be held in 1897, at Toronto, in view of a cordial invitation from that place, and that it adjourn to assist the citizens in entertaining the British Association for the Advancement of Science, which is to meet there at that time. Other invitations were received from Nashville, Tenn.; Columbus, Ohio; Indianapolis, Ind.; Detroit, Mich.; Minneapolis, Minn.; Seattle, Wash., and San Francisco, Cal. The recommendation of the committee was not agreed to by the Association, who ordered that a regular meeting should be held, and referred the time and place to the council. At a subsequent meeting of that body it was agreed to meet in Detroit, commencing August 9th, in order to give the members the opportunity of attending the British Association meetting at Toronto thereafter.

Messrs. Tarr, Mayberry, Packard, Bessey and Carhart were appointed a committee to coöperate with the national educational societies in arranging the methods of science teaching.

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AMERICAN NATURALIST

Vol. XXX. October, 1896. 358

FRESH RELICS OF GLACIAL MAN AT THE BUFFALO MEETING OF THE A. A. A. S.

By G. Frederick Wright.

1. The first paper upon this subject was presented by Prof. G. Frederick Wright, detailing briefly the results of a single day's exploration at Trenton, N. J., under the guidance of Mr. Ernest Volk, who is continuously carrying on similar explorations under Prof. F. W. Putnam for the Peabody Museum at Cambridge and the Central Park Museum, New York City. Professor Wright was requested to select his own ground upon the Lalor farm, where permission has been given for exploration, and the surface to a depth of three feet would be dug over in his presence. The point selected is on the bluff of the terrace of glacial gravel upon which the city of Trenton is built, a mile or more south of the center of the city. The bluff here facing the river is about fifty feet above it, and the terrace stretches back in a dead level for a mile and a half. The situation is such that there was no chance for surface wash to have remodified the deposit. In the near vicinity were boulders two or three feet in diameter resting upon the surface, or slightly below it, showing the ordinary conditions of deposition in connection with floating ice which characterized the whole delta terrace at Trenton, and which have been so often described by the geologists who have visited the region.

A trench three feet deep and three feet wide was dug from the face of this bluff backwards about thirty or forty feet. The upper twelve inches of this trench consisted of sand discolored with vegetable decomposition, which had evidently been disturbed. In this stratum there were found two flint arrowheads or spear-heads, one argillite chip, and one flint chip, together with a fractured pebble, four pieces of pottery, and a piece of charred bone.

The lower two feet of the excavation, except where interrupted by a pit, consisted of compact sand distinctly stratified, which had clearly been undisturbed. In this was found at varying depths one imperfect argillite implement, about three inches long and an inch and a half wide and a quarter of an inch thick, with five unrolled and angular fragments of argillite, two of which bore pretty clear evidence of having been chipped by human hands. These were the only fragments. There were no chippings or fragments of flint or jasper in the lower two feet of the excavation.

This brief paper of Professor Wright was but the prelude to bring out from Professor Putnam a fuller statement of the results of Mr. Volk's work on the Lalor farm. For two years Mr. Volk has been carrying on similar excavations over adjoining parts of the farm where the situation is similar to that described, and with corresponding results. Flint and jasper implements and flakes are abundant in the upper twelve inches of the soil, while no flint or jasper occurs in the lower two feet, of undisturbed sand and gravel. A large number of boxes of implements and fragments accumulated by this work of Mr. Volk have been sent up to the museums above mentioned; but, owing to the lack of time, Professor Putnam has not yet opened them and published the results. But in preparation for this meeting Professor Putnam had requested Mr. Volk to pursue further investigations and send the results to him at Buffalo. These were presented by Professor Putnam in a paper from Mr. Volk describing between thirty and forty argillite implements and fragments which had been found in his subsequent excavations in the undisturbed lower two feet of sand, as described in Professor Wright's excavation. As in that

case, so in this, flint and jasper were abundant in the upper twelve inches, but argillite was the only chipped and angular material found in the lower two feet. A large diagram accompanied Mr. Volk's description in which the position of each one of these argillite fragments was found. The box was then opened for the first time, and the fragments presented for examination. Of the artificial character of many of them there was not the least question on the part of any one present.

The importance of these discoveries as confirming the evidence of glacial man at Trenton heretofore presented can readily be perceived. It coincides with that presented by Professor Putnam and Dr. C. C. Abbott and Mr. Volk, going to show that there was a clearly marked succession in the human occupancy of the Delaware Valley indicated, first, by the sole use of argillite for implements, followed by a gradual and almost complete transition to the use of flint and jasper in later times. (See Putnam's report to the Peabody Museum in the Proceedings of the American Antiquarian Society, October, 1889, p. 11, and Observations upon the Use of Argillite by Prehistoric People in the Delaware Valley in Proceedings of the American Association for the Advancement of Science, by Ernest Volk, vol. xlii, p. 312). It also sweeps away at once the ingenious theories of Professor Chamberlin and others who would account for the occurrence of implements in the lower strata of sand and gravel through the agency of dry-weather cracks in the surface, the overturning of trees, the decay of tap roots, and the activity of burrowing animals; for none of these agencies would select the argillite, and leave the flint and jasper upon the surface. Therefore it would seem that there can be little doubt that these argillite fragments were scattered by the agency of man at the time that the deposition of the Trenton gravels was still in progress.

2. A second paper was by Prof. E. W. Claypole, detailing the particulars concerning the discovery of human relics from the drift at New London, Huron County, Ohio. These consisted of what would be called Neolithic axes, found by an intelligent workman in the process of well-digging in the blue till twenty feet below the surface. The circumstantial evidence

sustaining the testimony of the workman is of the most convincing character. The passage from the yellow till into the blue till and the occurrence of occasional strata of gravel are characteristic of the glacial deposits of northern Ohio. The implement had been subjected to oxydizing agencies characteristic of the deeply covered strata of that immediate vicinity. It is impossible briefly to detail this evidence. We must therefore wait for its full publication by Professor Claypole.

In a word, the geological situation at New London, Ohio, is this: The watershed between the Great Lakes and the Ohio is but a few miles to the south, and drains to the north through the main valley of Vermillion River. The land about New London is level for several miles, and is about two hundred feet below the summit of the watershed. There is no opportunity for any disturbances to have occurred subsequent to the glacial period; but in the retreat of the ice from the watershed a temporary glacial lake doubtless occupied the upper part of the valley of Vermillion River, emptying its waters into a tributary of the Mohican, and thence into the Muskingum and the Ohio. But this lake evidently did not exist for a great length of time.

Heretofore numerous flying reports of the discovery of implements in the glacial till have been made, but this is the first instance where the evidence has seemed in itself altogether convincing and satisfactory.

RELATIVE EFFICIENCY OF ANIMALS AS MACHINES.[1]

By Manly Miles, Lansing, Mich.

In my paper on Energy as a Factor in Rural Economy, read at the Washington meeting of the Association, approximate quantitative estimates were made of the energy expended

[1] Read in Section F. at the Buffalo meeting of the American Association of Science, Aug., 1896.

in the exhalation of water by plants, and evaporation from the soil; and at the Madison meeting similar estimates were given of the potential energy of an acre of corn, and of a fat ox, as representing the work done in the constructive processes of growth. The substance of the last mentioned paper, with some additional matter was published in the AMERICAN NATURALIST of July, 1894.

Some further illustrations of the same general principles are now presented in an inquiry as to the relative efficiency of different classes of animals, as machines, in utilizing the potential energy of their food in useful work.

From the imperfect data now available, there are many questions relating to this subject that cannot be definitely answered, but the approximate quantitative estimates we are able to make must be of interest in suggesting the lines of research required for a satisfactory solution of the problems involved in discussing the economy of foods and diets, and especially in the interpretation of the results of feeding experiments.

The chemical theories of nutrition have been so generally accepted in popular expositions of alimentary processes that it may be well to recapitulate the leading facts relating to energy as a factor in physiology in order to clear up the field of view and give due prominence to the principles we have to deal with.

The food consumed by animals serves two distinct purposes which should be clearly distinguished. The materials required in building tissues, and in the manufacture of animal products (meat, milk, wool, etc.), have alone been noticed in popular essays on the subject of nutrition, while the quite as important expenditures of the energy supplied in foods, as the motive power required in the constructive processes involved in converting the food constituents into animal tissues and products, have been misinterpreted or entirely ignored.

As pointed out in the papers above noticed, but a limited amount of the constituents of foods are stored up by animals in their processes of growth—in their increase when fattened—or in the animal products they manufacture. With refer-

ence to our present subject, the following table, showing the percentage of food constituents found in the increase of fattening animals at Rothamsted, will serve as a sufficient illustration.

TABLE 1.

Percentage of food constituents in the increase of fattening animals.

Constituents of Food.	Stored in Increase.		
	Oxen.	Sheep.	Pigs.
	Per cent.	Per cent.	Per cent.
Proteids	4.1	4.2	13.5
Carbo-hydrates and fat	7.2	9.4	18.5
Ash	1.9	3.1	7.3
Dry Substance	6.2	8.0	17.6

It will be seen that much the larger part of the food constituents were not utilized by the animals as materials for building tissues, but they have served a useful purpose in yielding up more or less of their stored energy, according to the degree of disintegration to which they were subjected, which was made available in the constructive processes of nutrition and the related incidental physiological activities of the system.

With this limited demand for the constituents of foods to serve as materials for tissue building, there must be an extensive disintegration of their organic substance to furnish the enormous supplies of energy required in the repair of tissues, in increase in growth, in the vaporization of water exhaled by the lungs and skin, and to supply the sensible residue that is lost by constant radiation from the body in the form of animal heat.

The obsolete theory of Liebig that certain food constituents are alone used to build tissues and that certain other constituents are burned in the system to produce heat, still continues to be the leading assumption in attempts to popularize chemical theories of nutrition in formulating diets and nutritive

ratios, and attention should be directed to the erroneous interpretations of organic processes that are made in the application of this false theory in connection with a fancied analogy of the animal machine to a steam engine.

We are told that "when coal is burned in the furnace a part of its potential energy is transformed into the mechanical power which the engine uses for its work. The rest is changed to heat which the engine does not utilize, and which, therefore, is wasted. The potential energy of the food is transformed in the body into *heat* and *mechanical power. The heat is used to keep the body warm. The mechanical power is employed for muscular work.*"

As an outcome of this false analogy the term "fuel value of foods" has been introduced to serve as an index of their capacity "to keep the body warm," and provide for muscular work. The absurdity of these crude and superficial views of energy as a factor in nutrition will be readily recognized by physiologists and we need only notice some obvious inaccuracies of statement.

In the first place, there are no processes of combustion in physiological activities, and fuel as such, can have no value in animal nutrition. The assumption that the potential energy of foods not expended in muscular work is "used to keep the body warm," is in direct conflict with familiar physiological activities. There are large expenditures of energy in transforming to the form of vapor the water exhaled by the lungs and thrown off as perspiration by the skin and it is a well known fact in physiology that the body is cooled by the evaporation of water from the surface that is constantly taking place.

The laboring man, perspiring freely in hot weather, expends a considerable part of the potential energy of his food in the cooling process of vaporizing the water discharged by the skin as a result of his exertions. The law of the conservation of energy is strictly observed and there is no demand for fuel to burn to "keep the body warm." The heat liberated in the destructive metabolism of the tissues, or what we speak of as the wear and tear of the sytem, is disposed of in various ways,

and the large expenditure in the cooling machinery for vaporizing water is an important factor in securing a proper adjustment in equilibrium of the numerous physiological activities of the system.

The fallacious and misleading theories of nutrition we have noticed have been so widely disseminated in recent official reports and bulletins that a detailed statement of the role of energy in animal nutrition, as now recognized by physiologists, seems to be required in relation to our present subject.

The energy required as the motive power in building tissues and the elaboration of animal products, as well as that expended in muscular work, is all derived primarily from the potential energy of the organic substances consumed as food, and as an incident of the various metabolic processes of the system animal heat is produced.

The stored, or potential, energy of organic substances is an essential element of their constitution, representing the work done in their processes of construction, and it can be liberated in the form of heat by any of the various methods of disintegration to which they may be subjected. For example, when organic substances are burned the heat produced is a measure of the energy stored up in their construction, but this method of liberating heat from food constituents has no place in physiological processes, and the value of foods as fuel is not a legitimate subject of discussion in domestic economy.

Microbes disintegrate the organic substances on which they feed and liberate their stored energy in the form of heat, as in the familiar processes of fermentation. The numerous microbes in the alimentary canal add their quota to the available energy in the form of heat derived from the constituents of foods which they tear apart in feeding upon them. The digestion of foods by animals is another means of liberating the stored energy in the form of heat.

Destructive metabolism, resulting from the various activities of the animal machine, immediately follows the constructive processes and the stored energy of the tissues originally derived from food constituents is liberated as heat which, so

far as needed, is used again as the motive power in rebuilding the disintegrated tissues and other physiological processes.

Animal heat is, therefore, the result of physiological activities that are carried on in accordance with the laws of the conservation of energy, without the slightest indication of anything analogous to processes of combustion.

Living substance, as pointed out by Foster, is matter constantly undergoing change, energy being used and stored up in constructive metabolism, and liberated again as heat in the correlated and quite as essential processes of destructive metabolism. Energy doing work, or stored as potential energy in the tissues, cannot be detected by the thermometer, and the heat liberated from foods in the various processes of disintegration they undergo, and from the tissues through destructive metabolism, is again made latent, as far as it is utilized in doing the work required in constructive metabolism, in vaporizing water exhaled by the lungs or thrown off by the skin as perspiration, and animal heat is the sensible residue not disposed of in these physiological processes.

Our domestic animals may then be looked upon as machines for doing work in the repairs and other vital activities of the animal machine itself, including muscular exercise, and the manufacture of animal products used as food by man. The importance of these animal machines as factors in domestic economy leads us to inquire as to their relative efficiency in utilizing the potential energy of foods in the special work they are fitted to perform.

Aside from the individual and class peculiarities that require attention, their efficiency in utilizing energy must vary with the quality and quantity of food consumed and a tentative solution of the problems presented in this line of inquiry can only be made. The quantitative estimates of the expenditures of energy in different ways we are able to make must, therefore, be interpreted as representing approximately the results with the particular animals under the conditions to which they were subjected.

The feeding experiments at Rothamsted, and the composition of different animals and their increase, as shown in the

extensive series of analyses made there, furnish the most convenient and only reliable data for our purpose in the case of fattening animals. The average results with oxen, sheep and pigs, as estimated by Sir John Lawes several years ago, and the feed consumed and increase made by the "analyzed fat pig" have been taken as the basis for calculating the expenditures of energy as given in the following tables.

In making a comparable estimate of the expenditures of energy in milk production, the record of a Guernsey cow at the New York experiment station in 1891, appeared to answer the required conditions, as detailed statements of the composition of the food consumed and milk produced were given, and the dry substance of the milk for each month differed but little from the dry substance of the 100 pounds of increase in the fattening animals under consideration, indicating in a general way that nearly the same amount of work had been required in its production.

The factors used in estimating the potential energy of organic substances (based on experiments of Berthelot and

TABLE 2.

Expenditures of energy in fattening oxen, sheep and pigs, to produce 100 pounds of increase in live weight; and a similar estimate of the expenditures of energy in milk production. In foot-tons of work.

		Potential energy of total feed consumed.	Expended and stored in 100 lbs. of increase.	Lost in excreta including urea.	In repairs of the system and other physiological processes.
Oxen		3,389,929	413,558	955,790	2,020,580
Sheep		2,809,787	440,584	655,967	1,713,240
Pigs		1,269,181	457,800	175,674	635,699
"Analysed fat pig"		1,478,393	435,160	272,335	770,900
Guernsey Cow	April	1,610,800	In 557 lbs. milk. 329,390	581,573	699,840
	May	1,626,678	In 581 lbs. milk. 349,440	564,020	713,230

Andre, 1890) represent in round numbers the calories per pound of the proximate principles of foods and tissues as follows: proteids, 2500 calories; fats, 4200 calories; and carbohydrates, 1900 calories.

For convenience of comparison, the energy in the table is represented in its equivalent of foot-tons of work; the energy expended in raising a weight of one ton one foot.

Without an examination of other details this table would be misleading in its indications of the relative efficiency of the different classes of animals as machines for doing work. There are decided differences in the quality and quantity of feed consumed which should be taken into consideration, but we have no data for a quantitative estimate of the modifying influence of these different conditions which can only be noticed in general terms. For convenience of comparison the most important facts relating to feed consumed may be summarized in tabular form.

TABLE 3.

Kind and quantity of feed consumed by fattening animals to produce 100 pounds of increase in live weight, and feed of the cow for equivalent results in milk production.

	Dry substance of feed.	Dry substance of increase.	Proteids in feed.	Proteids in increase.	Feed Consumed. Hay.	Grain.	Green feed.
	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
n	1109	68.6	218	9	600 (clover)	250 (oil cake)	3500 (swedes)
ːp	912	72.5	177	7.5	300 (clover)	250 (oil cake)	4000 (swedes)
	420	73.8	52	7.0		500 (Barley meal.)	
ıalyzed fat pig"	478	71.4	100	7.7		1. Bran. 2. Bean and lentil meal. 3. Barley meal.	
rnsey } April	469	78	82	19.2	300 } (clover and	210 } mixed	420 } corn
ıw } May	510	83	81	21.5	182 } timothy)	213 } grain	868 } ensilage

To produce 100 pounds increase the oxen consumed more dry substance of food than the sheep, and of coarse fodder there was twice as much hay requiring a larger expenditure of energy to dispose of it, and a smaller amount was utilized and stored in the increase which contained less dry substance. There was also a larger loss in the excreta and a greater expenditure in repairs of the system and other physiological processes, resulting from the increased metabolism.

A comparison of tables 2 and 3 will show that less work is required to dispose of the smaller amount of coarse feed in the rations of the sheep, and with less dry substance of feed they give a much better proportionate return in increase than the oxen; while the pigs fed on barley meal alone make the same increase from less than one-half the dry substance of feed consumed by the sheep, with a diminished waste in excreta, and the repairs of the system and other physiological processes are carried on with a comparatively small expenditure of energy.

The fourth column of table 3 is, however, of special interest in relation to the processes of nutrition, and the difference in the results we have been discussing. One of the most marked results of proteid substances in the food, now recognized by physiologists, is to increase the metabolism of the system. From the small amount of proteid substance stored up in the increase of fattening animals, as shown in tables 1 and 3, an active metabolism of the system must be carried on to dispose of any considerable excess supplied in the food, as all that is digested and not retained in the increase is discharged by the kidneys in the form urea. The supply of energy to carry on the increased metabolism of the system arising from an excess of proteid food is, to some extent, however, immediately provided for in the heat liberated in its conversion into urea.

The oxen, with 218 pounds of proteids in their feed, give the smallest return in increase, the largest amount of waste in excreta, and decidedly more work is required in repairs of the system and other physiological processes resulting from the increased metabolism. The pigs fed on barley meal, with only

one-fourth as much proteid matter in their feed, have a decided advantage in utilizing energy in their increase, with less waste in excreta, and a comparatively small expenditure of energy is required to keep the machine in working order.

A comparison of the results with the pigs fed on barley meal and the "analyzed fat pig" with a highly nitrogenous diet, will furnish quite as striking an illustration of the influence of proteid food, as the conditions are less complex from the absence of coarse fodder in the rations.

The "analyzed fat pig" consumed more dry substance, and nearly twice as much proteid matter, to make 100 pounds increase in live weight as the pigs fed on barley meal alone, but less energy was utilized in its increase, containing less dry substance and decidedly more was lost in excreta and a larger amount was required in repairs of the system.

TABLE 4.

The energy of food consumed by different animals to produce a given increase is expended as follows, under conditions above noticed.

	In increase.	Lost in excreta.	In repairs and other physiological processes.
	Per cent.	Per cent.	Per cent.
Oxen	12	28	60
Sheep	16	23	61
Pigs	36	14	50
"Analyzed fat pig"	29	18	52
Guernsey Cow, April	20	36	43
Guernsey Cow, May	21	35	44

The pigs fed on barley meal with their comparatively simple organs of nutrition, require less dry substance of feed and less energy to make a given increase than the ruminants with their complex nutritive machinery and large amount of coarse fodder in their rations which they are fitted to utilize; but pigs and ruminants are alike in failing to give as large returns in profitable increase with an excess of proteids in their feed.

The results recorded in table 2 must then be attributed to differences in the working machinery of the animals themelves, and the modifying conditions of the relative amount of coarse fodder and proteids in the feed consumed, but with our present knowledge of vital activities the relative influence of these variable factors cannot be determined.

The expenditures of energy by different animals in different ways, given in table 2 in foot tons of work, are summarized in percentages of the energy of feed consumed in the following table for convenience of comparison.

In the last columns of tables 2 and 4, several well defined physiological processes are grouped together from the lack of data to discriminate between them, and they undoubtedly vary in the relative expenditures of energy required in them under different conditions. Without attempting an exhaustive enumeration of these processes, the following may be noticed as of the first importance from the work performed in their activities.

Constructive metabolism in the repair of tissues; vaporization of water exhaled by the lungs and skin; work of the involuntary muscles in respiration and circulation of the blood; energy expended in mental activities and the functions of secretion and excretion; loss of heat by radiation from the body; and work done by the voluntary muscles.

It will be seen from table 4 that the largest percentage of the available energy of foods is expended in these strictly physiological processes concerned in maintaining the integrity of the animal machine. Under the conditions of feeding experiments, with fattening animals and cows giving milk, but little mechanical work is done by the voluntary muscles, and the last item of our enumeration of physiological processes might have been omitted as insignificant in relation to the enormous expenditures of energy in the other normal activities of the system to which attention has been directed.

If mechanical work is done by animals, it must be at the expense of the energy that might, under other conditions, be expended in the manufacture of animal products, as the physiological processes enumerated above must all be provided for

in fattening animals and cows giving milk with a minimum of muscular exercise, as well as in the case of animals engaged in severe muscular work.

The energy expended in mental activities is of the first importance in its influence on the efficiency of the animal machine in useful work. The nervous system, through which mental endowments are manifest, has intimate relations with every part of the animal machine and the direction in which energy is expended is largely determined through its agency.

Practical farmers are well aware that animals fail to give profitable returns for feed consumed when restless and excited through any source of disturbance, or when dissatisfied with their feed and surroundings.

The available energy of a liberal supply of nutritious food may all be expended, and even the stored energy of the tissues drawn upon to carry on the increased physiological activities resulting from mental and nervous derangements of the nutritive machinery without any expenditure in profitable production. In conducting feeding experiments and in the interpretation of their results this is one of the most difficult factors to deal with, as it may have a dominant influence on the final outcome.

The approximate estimates of the relative efficiency of different animals in utilizing the potential energy of their feed in useful work, which have been given in mere outline, will require revision and correction as we become better acquainted with the specific influence of the variable factors of food and environment on the work performed by animal machines. Even in their present imperfect form they may, however, serve to illustrate the significance of energy as a factor in animal nutrition and the futility of formulating diets and nutritive ratios in terms of their chemical constituents.

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

BY ERWIN F. SMITH.

(*Continued from p. 731*)

III.

NOTE.—A second note by Dr. Sorauer on the Bacteriosis of Fodder Beets adds a number of interesting items. This is entitled (**24a**) Die Gummikrankheit bei Runkelruben etc. It was published in *Jahrb. d. Deutschen Landwirtschafts-Gesellschaft.* Bd. 7, Berlin, 1892, Second part, pp. 206, 207, and was republished verbatim in *Zeitschr. f. Pflanzenkr.* 2Bd. 5Heft, 1892, pp. 280–281. The following abstract should therefore be read in connection with the *Remark* on p. 723 from which place the reference was inadvertently omitted.

Samples of the diseased beets were received from Vukovar in October, 1890 and in February, 1891. With the first specimens came the statement that one half of the crop was affected. The beets were drilled early in April and the weather was favorable until the middle of June; then hot and dry weather set in, continuing until harvest time in the middle of October. During all this time there was only one rain. The beets suffered severely from the heat and drouth, losing all their outer leaves and pushing new ones toward the end of September. At harvest time the disease was found more or less developed in such plants as showed wilted heart leaves. The blackening of the roots, a blue black, began at the lower extremity and continued upward after the roots were stored. The flesh of the root appeared to be uniformly blackened at the root tip, and from this part the discoloration radiated upward into the sound part so that at last it was noticeable only in the regions of the vascular bundles as brown stripes and rings, the rest of the flesh being white. On making a cross section of the beet root a drop of gum sometimes exuded within a few minutes from isolated points of the browned vascular strand. The affected beets became flabby and shriveled lengthwise and in places showed a sticky sweat or an abundant exudation of gummy masses through the uninjured surface, forming a lacquer-like covering. In bad cases gum cavities appeared in the flesh from solution of the tissues. The diseased beets contained a strikingly large amount

of grape sugar. This was determined by use of Barfold's reagent. Two milch cows fed on these roots died the second day. The symptoms were bloating, pain in the abdomen, obstinate constipation, and the prolonged vomiting of a tough yellow slime. In both cases pieces of the diseased beets were found in the first stomach along with other food, and no other reason for death was apparent. These beets (four varieties) were grown on a porous clay soil containing 5% lime and 8% humus. The soil-water level was 16 meters from the surface. The field had been very heavily dunged with stable manure.

II. THE HYACINTH (HYACINTHUS ORIENTALIS L.).

1. THE YELLOW DISEASE (1883).

(I) THE DISEASE.

(1) *Author, Title of Paper, Place of Publication.*—This disease was described by Dr. J. H. Wakker, as a result of investigations begun in the fall of 1881 at the request of the Algemeene Vereeniging voor Bloembollencultur te Haarlem, and was carried on in the laboratories of the University of Amsterdam. The first brief account appeared in 1883, in *Botanisches Centralblatt* (Bd. XIV, pp. 315–316) and forms part of a (**30**) Vorläufige Mittheilungen über Hyacinthenkrankheiten. The following year a more extended account was published in Dutch, (**31**) Het geel-of nieuwziek der Hyacinthen veroorzaakt door *Bacterium Hyacinthi* Wakker (*Onderzoek der Ziekten van Hyacinthen, en andere bol-en knolgewassen. Verslag over het jaar 1883.* Haarlem, August, 1884. 8vo, pp. 4–13, one colored plate). Two additional papers were published in Dutch, continuing and concluding the one above mentioned, (**32**) *Onderzoek etc., Verslag over het jaar 1884.* Haarlem, May, 1885, 8vo, pp. 1–11, and (**33**) *Onderzoek etc., Verslag over het jaar 1885.* Haarlem, May 1887, 8vo, pp. 1–5, and 27 to 37). These are the important papers to read and the ones which have mostly escaped mention. Finally, six years after the commencement of the investigation, Dr. Wakker published a fifth paper, entitled (**34**) Contributions à la pathologie végétale: I. La maladie du jaune, ou maladie nouvelle des jacinthes, causée par le *Bacterium Hyacinthi* (*Archives néerlandaises d. Sci. ex. et nat.*, tome XXIII, 1889, pp. 1–25, pl. I). This paper is merely

an abstract of the earlier Dutch papers. Part of the lithographic figures in the plate are, however, new.

Remark.—The papers published by Dr. Wakker in 1883 and 1884 were among the first contributions of any importance to the bacterial literature of plant diseases, but they were not, as claimed, actually the first. That honor belongs to this country, as we shall see later on when we come to take up pear blight, Prof. T. J. Burrill having published a long paper in 1880. The lack of literature and the difficulties in the way of the successful prosecution of this work at that time are well expressed in the *Verslag* for 1883: Als oorzaak van een plantenziekte waren Bacteriën nog slechts eenmaal en dot wel zeer kort (Prillieux *Bull. d. l. soc. bot. d. Fr.* 1879, p. 31 and 187) beschreven zoodat het geval van het geelziek niet alleen van praktisch, maar ook van hoog wetenschappelijk gewicht is. Is daarom het onderzoek er van zeker her belangwekkendste van alle ziekten, waarmede wij ons hier zullen bezighouden, het is ook tevens het moeilijkste omdat bij gebrek aan mededeelingen omtrent dit of een dergelijk onderwerp alles op eigen onderzoek berusten moet.

(2) *Geographical Distribution.*—This disease has prevailed extensively at times in the large bulb gardens in the Netherlands, where it is said by a majority of the Dutch horticulturists to be a new trouble, i. e., one that has appeared within the last ten years (**31**) or within the last 20 years (**34**). The writer of this digest has never been able to find the disease in bulbs imported from Holland, and does not remember to have seen any account of its occurrence in other parts of the world.

(3) *Symptoms.*—According to Dr. Wakker the first symptoms of the disease are usually in the foliar and floral organs. There is an apical browning or blackening of the leaves and scapes which color can often be traced downward into the green leaves for some distance in the form of dark stripes. The epidermis frequently ruptures longitudinally, and large irregular masses of bacterial slime exude from the rifts. The diseased parts also have a wet, unctuous appearance, and shrivel from the apex downward. Subsequently the bulbs become diseased, and clearly as a result of the preceding disease of the

foliar and floral organs. The earliest symptom of disease in the bulb consists on cross-section, of yellow dots, visible here and there in the interior of the scales or, on longitudinal section, of yellow lines which frequently extend into the plateau. From these spots, a mucilage swarming with bacteria can be obtained in drops by squeezing the scales or simply by exposing the cut bulb to the air. Sometimes the whole interior of the scale, or all of its inner or outer part, degenerates into a mass of yellow slime. If the attack is rapid, the plateau is soon invaded and the bulb rots in the ground during the fall or winter. If the progress of the disease is slow, and this is usually the case, the bulb sends up leaves and blossoms the following spring in the ordinary manner. At this time or afterwards there are distinct signs of the disease. In many cases the leaves turn yellow in lines parallel to the longer axis of the leaf. These lines begin at the base of the leaf and proceeding upward become less and less visible until they entirely disappear. In the interior of the leaf under these yellow stripes the bacterial slime is abundant and on the lower parts of the leaf it frequently finds its way to the surface, ruptures the epidermis and escapes. In such plants the bulb is always badly diseased, especially the outer scales which are the basal portion of the leaves of the previous year, and this, taken in connection with the fact that the bulbs are entirely sound in most cases where the symptoms are only visible at the apex of the leaf, renders it very probable that the latter is to be regarded as the first stage of the disease and the former as a later stage, supervening the second year. Another symptom, sometimes observed the second year, is unequal growth, i. e., a distinct curving over of the foliage toward that side of the plateau which has perished or is no longer capable of furnishing the proper supply of water and nutrient substances, the curvature being, of course, due to the one-sided growth. As a rule, diseased bulbs do not produce many bulblets, and not all of the latter are always diseased. If planted out, those which are diseased show signs of the malady in the young plants after a longer or shorter period. The leaves turn yellow, become flabby and droop, or show the characteristic longitudinal rifts in the epidermis. When such

young bulbs are cut, the plateau (central short stem of the bulb) is often found to be the only diseased part, something inexplicable if we do not admit that the disease has been transmitted to the bulblet from the mother bulb. Such is the usual form of the disease, but it will be understood that there are numerons modifying circumstances, the disease sometimes beginning lower down on the edge of the leaf, or even underground, or progressing more rapidly, the latter especially when the disease attacks full grown leaves and scapes. The most cases of the first stage of the disease are noticed in the field in May, but cases also occur much earlier in the year.

(4) *Pathological Histology.*—In spring, in the first stage of the disease, when only the tips of the leaves are attacked, microscopic examination shows the bacterial slime to be present in the intercellular spaces of the shrivelling leaf-parenchyma, but always only in small quantity. From this part of the leaf the bacteria may be traced long distances down the vascular bundles, but have not yet reached the bulbs, the latter being still entirely sound. In autumn, on the contrary, cross sections of the bulbs, if not too badly diseased, show numerous yellow dots in the scales, and on microscopic examination these are found to correspond to the xylem part of the vascular bundles (No. 34, pl. I, figs. 9, 10). The vessels of the latter are seen to be full of a thick, yellow slime, which often partially dissolves them. Here and there, the whole xylem part of the bundle may disappear, the yellow slime taking its place. In this way are formed continuous, tubular cavities, filled with isolated cells of the host plant, remnants of spiral threads, and an innumerable number of bacteria. In this stage of the disease, the sieve tubes are not yet attacked, but these are subsequently destroyed, and frequently, also, the parenchymatic tissues outside of the bundles, the substance which unites the cells being first dissolved. The second spring, a microscopic examination of the yellow striped leaves from diseased bulbs shows a similar occupation of the vessels with the same lesions, but in a reverse order, the bacteria being most abundant and the destruction of tissues greatest in the basal part of the leaf. Here the bacteria dissolve the walls of the vessels

and make their way into the surrounding tissues, first isolating and then destroying the parenchymatic cells and finally increasing to such an extent that the epidermis is ruptured and a viscid, yellow ooze escapes. The vessels of the scape are filled in the same way, but the bacterial slime was not observed in the roots. The bacteria may be distinguished in the vessels considerable distances in advance of any external or macroscopic symptoms. The bacterial mucilage is a yellow liquid, thick and viscid. Under a low power of the microscope it shows a granular structure, this being due, as we recognize on higher magnification, to the presence of bacteria. Dr. Wakker describes at some length his method of examination: "The transverse sections must be made with extreme care, such as is almost unknown in the ordinary study of vegetable anatomy. Not only is the affected tissue so soft that it is impossible, in a fresh state, to cut sections sufficiently thin, but there is also danger that the mucilage will be dragged by the knife into parts of the leaf where originally there was not a trace of it. To overcome these two difficulties good use was made of absolute alcohol. The green color was removed by this method while the yellow of the mucilage persisted, and, in consequence, became much more distinct, so that it was easier to distinguish diseased from healthy tissues."

(5) *Direct Infection Experiments.*—This disease was studied before Koch's plate method of isolation had come into general use, and most of the infection experiments were made directly from diseased to healthy plants. In the fall of 1882 bacteria were introduced into a bulb of the double white Anna Maria, and when this bulb was cut the next spring it showed distinct signs of the disease. This experiment was frequently repeated and always with the same result. For example, the whole cut surface of the scales of a bulb from which roots and leaves were cut away in summer was smeared with the bacterial slime, and in 14 days the disease was to be found in the vascular bundles of the youngest scales, and shortly after in those of the older. Slightly diseased bulbs are the best parts from which to obtain the bacteria. In badly diseased plants one runs the danger of finding all sorts of things, even Penicillium, in the de-

cayed mass; and from the leaves, before the disease has reached the bulb, it is impossible to get a sufficient quantity of the slime.

On March 27, 1884, small quantities of yellow slime were taken from some slightly affected bulbs (double red Temple of Apollo) and inserted into wounds made for this purpose in the top of the leaves of different varieties. These were examined daily for signs of disease which first appeared, in most cases, only after a month. Distinct symptoms were apparent but unfavorable circumstances caused the loss of the leaves before the downward stripe had progressed very far. These plants stood in pots in the open air and were watered regularly, but the spring was very dry. Although in this case a month elapsed before external symptoms appeared, it is not to be inferred that so long a time always intervenes. On the other hand it is likely that in natural infections even a longer time may elapse before symptoms appear, since countless numbers of the bacteria are used in artificial infections, while natural infections are probably brought about in most cases by the entrance of a few bacteria which would require more time to produce visible results. On Oct. 27, 1884, the small unfolding leaves of each of a number of sound hyacinth bulbs were wounded with a steel pen and some of the bacterial slime inserted into these punctures. The bulbs were then potted, kept in a place free from frost and examined from time to time. On Jan. 13, 1885, one plant showed the disease very distinctly. Two of the three infected leaves had stripes extending downward from the wound, each about 13 mm. Here, also, a long time intervened between the inoculation and the appearance of external symptoms.

Another series of infection experiments was begun Dec. 28, 1885, and completed in the spring of 1886. These plants were also inoculated with bacterial slime taken directly from diseased plants. These experiments were made on five plants of as many varieties, grown in carafes. All were kept in a cool place until Feb. 13, when they were transferred to a room regularly warmed. The manner of inoculation and the results obtained are here summarized: *La Tour d'Auvergne* (double

white variety). Bacteria introduced into a wound in the yellow part of a leaf. *Results:* Feb. 13. In bloom. Two small streaks extending downward from the wound. Feb. 20. Leaf accidentally broken. It was put immediately after into alcohol and subsequently hardened in absolute alcohol, whereupon microscopic preparations made and stained in the way already described, showed three vascular bundles attacked in varying degrees. Two of them evidently corresponded to the two little stripes on the leaf, while in the third bundle the disease had not made enough progress to be visible on the surface of the green leaf. *Norma* (single red variety). Bacteria introduced into the green tips of three leaves. *Results:* Feb. 13. In bloom. 1st leaf, nothing. 2nd leaf, a little spot two millimeters above the wound. 3rd leaf, a similar spot below the wound. Feb. 20. No change. Feb. 27. No change. March 6. No change. March 20. Second leaf broken; 3rd leaf show little spots 10 mm. below the wound, plant moved into the open air. March 27. No change. April 3. No change. *Coeur blanc* (single white variety). Bacteria introduced into a wound in the yellow part of a leaf. *Results:* Feb. 13. A stripe extending downward from the wound, 4 mm. Feb. 20. In bloom. No change. Feb. 27. Length of stripe 15 mm. March 6. Length of stripe 22 mm. March 13. Length of stripe 25 mm., and small spots 10 mm. lower. March 20. Length of stripe 27 mm., and small spots 22 mm. lower. Plant put into the open air. March 27. Length of stripe 27 mm., and small spots 35 mm. lower. April 3. Length of stripe 67 mm. *Crown Prince Charles of Sweden* (double blue variety). Bacteria put into wounds at the green apex of two leaves. *Results:* Feb. 13. Nothing. Feb. 20. 1st leaf, a downward stripe of 3½ mm. from one of the wounds. 2nd leaf, nothing. Feb. 27. 1st leaf, length of stripe 15 mm., and small spots all around the wound. 2nd leaf, nothing. March 6. In bloom. Length of stripe 17 mm. March 13. Length of stripe 18 mm., and a small spot 4 mm. lower. March 20. Very little change. Plant put under a bell jar on a dish containing water. March 27. Length of stripe 22 mm. Bell jar removed because leaf began to turn

yellow. April 3. No change. *Anna Maria* (double white variety). Fragments of diseased tissue introduced into wounds in the green tips of three leaves. *Results:* Feb. 13. A downward stripe from one of the wounds, length 10 mm. On the other two leaves nothing. Feb. 20. In bloom. Length of stripe, 17 mm. On the other two leaves nothing. March 6. Length of stripe 35 mm., and small spots 10 mm. lower. March 13. Length of stripe 45 mm. The stripe and bordering tissues have dried up for a distance of 35 mm. March 20. Length of stripe 55 mm., and small spots 15 mm. lower. Diseased part dry for a length of 50 mm. Leaf bent by the drying of one side. Plant put out-doors. March 27. Length of stripe 90 mm. Dry for a length of 55 mm. April 3. Length of stripe 94 mm.

Measurements were not made after April 3, but subsequently all of the diseased leaves were removed, placed in alcohol, hardened in absolute alcohol, and examined microscopically in the same manner as the leaf already mentioned, and with the same result. On the same date, Dec. 28, 1885, a quantity of bulbs, including the above varieties, were also infected and were planted out of doors where they were exposed freely to the air. Up to April 3 there were no signs of disease but a little later symptoms appeared in most of the plants. From these experiments the author draws the following conclusions: (1) the *Geelziek* or *maladie du jaune* can be induced artificially, and (2) the results of the infection make their appearance a long time after the operation.

(To be continued.)

EDITOR'S TABLE.

THE late meeting of the American Association for the Advancement of Science while less numerously attended than some others, was a larger gathering than has sometimes represented it. The meetings of the Association cannot be as large relatively to our population as those of most of the European nations, because of the longer distances which the members are compelled to transverse in order to reach them. Many of the most active workers must always be absent in the field during the summer months, especially so long as our country presents such opportunities for original research. The summer schools take away some members. The meeting at Buffalo was held in such a way as to discourage the attendance of those who regard it as merely an opportunity for junketting. The meetings extended from Monday to Friday inclusive, and Saturday only was reserved for excursions. This arrangement was greatly to the advantage of work, the maintainance of interest, and of the attendance. The members present were more than usually conspicuous as workers, and the number and value of the papers read was fully up to the best standard.

The Association decided to meet in Detroit at the unusually early date of August 9th, next year. This date was fixed on account of the approaching meeting of the British Association at Toronto on August 18th following. A cordial invitation from the citizens of Toronto to take part in the reception of the British Association was accepted, and this will follow the meeting at Detroit.[1] A respectable minority of the Association thought that we should suspend our meeting for that year, or meet formally for organization only, and then adjourn to take part in the reception of the British Association. This view carried the Nominating Committee, but was not approved by the Association. That the Association did wisely there can be no doubt, and the circumstance shows that all the wisdom in that body is not concentrated in its representatives in the Nominating Committee. The reasons put forth by the Committee for its action were plausible, but were believed to be fallacious by a large majority of the Association. One of these reasons was the assumption that the American Association meeting would necessarily be neglected by its members if the British Association meet in Toronto. The Association thought otherwise, especially as it was remembered that the second largest meeting ever held was in

[1] Not however by special adjournment as stated in Nature of Sept. 17, p. 480.

Philadelphia in 1884 when the British Association met in Montreal. As the American Association knows its own mind, we may look for one of our largest meetings in Detroit in 1897.

IN our issue for October, 1895, we referred to the organization of the Field Museum of Chicago as having failed to furnish a successful basis of operations for the prosecution of original research. At that time most of the men who could give reputation to it had left, owing to the unsatisfactory positions in which they found themselves placed. Subsequently the establishment of publications of a very meritorious character induced us to believe that proper steps had been taken by the management to place the scientific men on such a basis as to insure the future prosperity of the enterprise. Authentic information recently received shows that this anticipation was premature. Other resignations have occurred, and the institution is evidently destined to be a failure unless a reorganization is effected.

Men who have spent their lives in mercantile pursuits are generally unacquainted with the conditions necessary to original research in science. The modus operandi in the two pursuits is fundamentally different. An element of tentative experiment enters into the pursuit of science, which requires a degree of freedom on the part of the investigator which cannot be accorded to the regular employee, the results of whose work are always susceptible of full anticipation. The investigator must have full control of material of research and of the ways of getting it. In fact no one else is likely to know how to get it. He alone knows the profitable lines of work; hence he must be permitted to select his work. No one will secure a museum sooner than he, and it will be as much more valuable than can be created by any one else, as the work of an expert is necessarily more important than that of other persons. For these and many other reasons no museum can become great unless its administration is in control of scientific experts, who should be responsible to each other and to the trustees only. With an organization of this kind, composed of the class of men from whom it has already selected some of its aids, there is no reason why the Field Museum, under the liberal terms of its endowment, should not rival the greatest museums of the world.

—WE must again remind contributors to the NATURALIST that proofs of all kinds and blocks of engravings must be sent to the publishers and not to the managing editor. Failure to observe this rule often causes inconvenient delays. Manuscripts, on the other hand, should go to the appropriate editors, and not to the publishers.

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——Bulletins du Comite Geologique. St. Petersburg. XIII. Nos. 8, 9; XIV, Nos. 1–5, 1895. The Geol. Surv. of Russia.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

Development of Faces on Crystals.—Gaubert[2] makes a contribution to the subject of the growth of crystal faces by means of his experiments with the alums. An octahedron of chrome alum, on solution in its mother liquor, is rounded at its edges and angles. When the solution becomes again saturated, and the crystal begins to grow, faces of the forms (100), (110), (211) and (221) are developed, but disappear on continued growth, leaving finally only the octahedron (111). Experiments with crystals of chrome and potassium alum prove that the same faces are developed when the rounding is done mechanically instead of by solution. Potassium alum from pure water gives the form of octahedron and cube, but by rounding (211) and (221) may be caused to grow.

[1] Edited by Prof. A. C. Gill, Cornell University, Ithaca, N. Y.

[2] Bull. Soc. Fr. Min., XVIII, pp. 141–143, 1895.

Crystals of lead nitrate and of barium nitrate also develop transitory faces when rounded, returning to the original form of cubo-octahedron on continued growth. Miers has observed the formation of the face (221) by the extremely slow solution of the potassium alum crystals. Hence, it seems that these "transitory planes" may be formed either by corrosion or by growth of a rounded crystal.

Albite from Lakous, Island of Crete.—Viola,[3] by his paper on the new occurrence of albite at Lakous, adds another to the list of carefully investigated pure chemical substances. An analysis by Mattirolo, given at the end of the article, shows close agreement with the theoretical values for $Na\,Al\,Si_3O_8$ as may be seen from the following:

	Found.	Theoretical.
SiO_2	68.35	68.70
Al_2O_3	19.78	19.47
Na_2O	11.71	11.83
K_2O	.16	
Ign	.15	
	100.39	100.00

Measurements on twelve crystals, varying from 7½ to 20 mm. in diameter, agree very well in giving as crystallographic constants: $\alpha = 94° 14' 30''$, $\beta = 116° 31' 45''$, $\gamma = 88° 5' 1''$, and $\breve{a} : \bar{b} : c = .635 : 1 : .557$. The extinction angle measured against the trace 001 in a section cut parallel to 010 is 21° 30′, in the section 001 it is 3° 30′. The optical angle is approximately + 80°. Inclusions of a member of the chlorite group are found in a number of the crystals, and some small scales of hematite in others.

Forsterite from Monte Somma.—The specimens seem to be of unusual chemical purity, hence the data given by Arzruni[4] on the the basis of investigations by himself, Jolles and Thaddéeff are doubtless near the true values for pure $Mg_2\,SiO_4$. The axial ratio is found to be $a : b : c = .46663 : 1 : .58677$. Cleavage parallel to 010, distinct. In addition to the previously observed method of twinning, the plane 031 is reported as a twinning plane.

[3] Tscherm. Mitth., XV, pp. 135–158, 1895.
[4] Zeitschr. f. Kryst., XXV, pp. 471–476.

The plane of the optical axes is the base, and the optical angle is 85° 38′ for lithium, 85° 45′ for sodium, and 85° 56′ for the thallium light.

The results of analysis are:

	I.	II.
SiO_2	42.65	42.39
FeO	1.35	3.12
MgO	56.57	55.09
CaO	.29	
Al_2O_3		.23
	100.86	100.83
Sp. G.	3.223	3.245

The ratios RO : SiO_2 are 2.018 : 1 and 2.01 : 1 respectively, after deduction for probable impurities.

Fayalite and the Chrysolite-Fayalite Group.—Penfield and Forbes[5] found the fayalite from Rockport, Mass., suitable for optical and other investigations. The mineral was found in the shape of a lenticular shell in massive hornblende-biotite granite. The color is a dark resinous green, though the light transmitted by the thin edges is yellowish. The purified powder has a specific gravity of 4.318 (average of 3 determinations). The average of the two analyses is:

SiO_2	30.08
FeO	68.12
MnO	.72
H_2O	.80
	99.80

The cleavages are 001 and 010, and the reported occurrence of a cleavage 100 is considered a mistake.

The plane of the optical axes is the base, and the double refraction is negative. For sodium light. $\alpha = 1.8236$, $\beta = 1.8642$, $\gamma = 1.8736$, giving $\gamma - \alpha = .050$. The macro-axis is the acute bisectrix, $V_y = 25°$ 18′.

A specimen of hortonolite from Monroe, N. Y., was also investigated. The table given below exhibits at a glance the effect of the iron on the optical characters of the chrysolite-fayalite group:

[5] Am. Jour. Sci., CLI, pp. 129–135, Feb., 1896.

	% FeC	2 V (over α)	β
Fayalite Rockport,	68.1	49° 50′	1.864
Hortonolite Monroe,	47.3	69° 24′	1.791
Chrysolite, Auvergne,	13.0	89° 36′	1.692
Chrysolite, Vesuvius,	12.6	89° 42′	
Chrysolite, Hawaii,	10.3	91° 2′	
Chrysolite, Egypt,	9.2	91° 19′	1.678
Chrysolite, N. M.,	8.6	91° 24′	
Chrysolite, Unknown,	?	92° 14′	1.678
Chrysolite, East Indies,	?	92° 45′	1.670
Forsterite, Vesuvius,	2 (?)	93° 50′	1.657

At about 12% FeO, therefore, the optical character changes from positive to negative.

Rhodophosphite and Tetragophosphite.—The rare mineral locality at Horrsjöberg in Wermland, Sweden, is the source of these two new minerals recently described by Igelström.[6] The rhodophosphite occurs in large quantities in layers reaching a thickness of 2½ feet, so that it can be mined profitably. At one locality it is found in the form of hexagonal prisms. From the partial analysis, it appears to be chiefly a calcium phosphate, with considerable quantities of ferrous iron and manganese, also chlorine, fluorine, and sulphuric acid. The formula proposed is $20\ (RO)_3\ P_2O_5 + 4\ (Ca\ Cl_2,\ CaF_2) + Ca\ SO_4$, where $R = Ca, Mn, Fe$, or Mg. The mineral is allied to svanbergite.

Tetragophosphite occurs in "four-sided" plates, or as a coating on the containing cyanite-damourite rock. The two analyses are:

P_2O_5	36.92	33.64
Al_2O_3	40.00	41.81
FeO, MnO	9.51	9.51
MgO, CaO	7.50	6.74
H_2O	5.96	8.30

These lead to the formula $(Fe, Mn, Mg, Ca)_3\ P_2O_8 + (Al_2O_3)_3\ P_2O_5 + 3H_2O$. It is somewhat lighter blue than lazulite, which it seems here to replace. The "Gusblått-phosphat" (light blue phosphate) from the Westanå Mts., Prov. Skåne, Sweden, analyzed by Blomstrand in 1868 seems to be undoubtedly the same mineral. He assigned the formula $(Ca, Mg)_3\ P_2O_8 + (Al_2O_3)_3\ P_2O_5 + 3H_2O$.

[6] Zeitschr. f. Kryst., XXV, pp. 433–436, 1895.

Miscellaneous Notes.—Von Zeynek[7] notes the occurrence of sulphur deposited in the canals carrying 1,000,000 gallons of water per day from the hot springs at Warasdin-Töplitz in Croatia.—Rohrer[8] gives results of two very careful analyses of hematite from Elba. The average of the two is as follows: SiO_2 .49, Fe_2O_3 98.60, CaO .42, MgO .74, total 100.25.—In an article on the contact of minerals of the Adamello Group of mountains in South Tyrol, Salomon[9] gives a detailed discussion of the Wernerite from Breno, with much of the literature relating to that mineral.—Duparc and Stroesco[10] have recorded the results of their observations on the crystalline form and optical behavior of thymoquinone and eleven of its derivatives.—Gentil[11] describes the occurrence of large bundles of yellowish-white somewhat altered sillimanite needles in pegmatite from Algeria. Veins of albite and plates of muscovite are also mentioned. The same author[12] makes a note of thomsonite, stilbite and analcite from an altered basic volcanic rock occurring near Dellys in the province of Algiers.—De Gramont[13] is led by the observation of the electric spark between fragments of certain minerals which are good conductors of electricity, to a study of the spectra of the sparks thus produced. This method promises to be useful for the rapid determination of certain minerals, and for the detection of included substances which are present only in traces. The lines of the non-metallic, as well as of the metallic elements may be observed. De Gramont also describes the apparatus used by him, and gives the details concerning the spectra obtained from air from twenty-four of the elements, and from about a hundred minerals.—Termier[14] calls attention to the two forms of the dimorphous substance PbO. After discussing the optical and crystallographic properties of the orthorhombic modification, he shows that its crystals are grouped to imitate a higher symmetry. PbO is, therefore, a good example of a substance which not only shows pseudo-symmetry by the grouping of the separate crystals, but also appears in a second form in which the *molecular* grouping follows an allied higher symmetry.—Gonnard,[15] in an article on French

[7] Tscherm. Mitth., XV, p. 192, 1895.
[8] Tscherm. Mitth., XV, pp. 184–187, 1895.
[9] Tscherm. Mitth., XV, pp. 159–183, 1895.
[10] Bull. Soc. Fr. Min., XVIII, pp. 126–141, 1895.
[11] Bull. Soc. Fr. Min, XVIII, pp. 170–171, 1895.
[12] L. c., p. 374.
[13] Bull. Soc. Fr. Min., XVIII, pp. 173–373, 1895.
[14] Bull. Soc. Fr. Min., XVIII, pp. 376–380, 1895.
[15] Bull. Soc. Fr. Min., XVIII, pp. 382–390, 1895.

siderites, adds to the thirteen forms of that mineral previously known the three rhombohedra $(03\bar{3}2)$, $(30\bar{3}4)$, and $(10\bar{1}2)$.—Termier and Richard[16] conclude from their study of crystals of $Ca_4P_2O_9$ occurring in the slags of the iron works at Kladno, that they are pseudo-orthorhombic, composed of monoclinic lamellæ. Measurements of the apparently orthorhombic form agree well enough with those of Miers to show that both had to deal with the same substance. The specific gravity is 2.93–3.1, mean index of refraction, about 1.64. For red light, $2\ V = 20°$ (?), and for blue light it is about 40°.—O. Nordenskiöld[17] finds edingtonite from Böhlet, Sweden, to be orthorhombic hemihedral instead of tetragonal hemihedral, as previously supposed. Sp. G. = 2.776, plane of optical axes = 010, negative bisectrix parallel to the vertical axis, $2\ V$ for lithium light = 52° 47′, for sodium = 52° 55, and for thallium 53° 10′. The indices of refraction for the above kinds of light are also determined. The mean index for sodium light is 1.5492, and the double refraction is .016. In conclusion, the similarity of form with that of mesotype is shown by the axial ratios:

Edingtonite $a:b:c = .9872:1:.6733$

Mesotype $a:b:2c = .9785:1:.7072$

—Goldschmidt[18] figures and describes a projection goniometer by means of which the position of crystal faces is projected directly upon paper, thus doing away with the reading of angles and with trigonometrical computation. The instrument seems to be in many ways convenient, but does not give the highest degree of accuracy. A contact goniometer of similar action is also briefly mentioned.

PETROGRAPHY.[1]

Geology of Point Sal, California.—The geology of Point Sal, the extreme northwestern corner of Santa Barbara County, California, has been carefully worked out by Fairbanks[2] with special reference to the igneous rocks found there. The sedimentary rocks constituting the point and the adjacent country are of miocene or later age. They

[16] Bull. Soc. Fr. Min., XVIII, pp. 291–295, 1895.

[17] Bull. Soc. Fr. Min., XVIII, pp. 395–398, 1895.

[18] Zeitschr. f. Kryst., XXV, pp. 538–560, 1895.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Bull. Dep. Geol. Univ. of Cal., Vol. 2, p. 1.

comprise volcanic ashes, gypsiferous clays and bituminous shales, the last named of which were regarded by Lawson as tuffs. The present author declares them to be organic deposits. The igneous rocks which penetrate these beds are all basic. They include gabbros, peridotites, basalts, diabases and rocks similar to those heretofore described as analcite diabases. These latter are all now considered by the author as representing the otherwise practically unknown type of the teschenites. The augitic variety of this rock has the general structure of the diabases, in which are large poikilitic plates of augite. Between the diabasic constituents are polyhedral grains of analcite, and, in what appear to have been cavities in the rock-mass, are little groups of crystals and crystalline masses of the same mineral. The plagioclase in the rock is all zonal with nuclei of labrodorite surrounded by concentric zones of a more and more acid feldspar, the peripheral one being albite. An analysis of a coarse grained specimen gave:

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	P_2O_5	Ign.	Total
49.61	19.18	2.12	5.01	10.05	4.94	1.04	5.62	.27	3.55	=101.39

which corresponds very nearly to 43.3 per cent feldspar, with a density of 2.57, 32.3 per cent augite, 20 per cent analcite, 4 per cent magnetite and .04 per cent apatite. All of the analcite is supposed to be an alteration product of nepheline.

The basalts of the region include two types. One is the usual variety and the other an amygdaloidal and spheroidal variety that is intruded by diabases and diabasic gabbros. These last named rocks grade into one another. Both contain hornblende, some of which is regarded as secondary and some as primary. In addition to the diabasic-gabbros there are others associated with peridotites (and serpentines) in such a manner that both rocks are regarded as differentiated products of the same magma. The gabbro is sometimes massive. At other times it is possessed of a gneissic structure, often attended by a striping produced by the alternation of augitic and feldspathic bands. The structure is concluded, after study, to be the result of stretching.

Among the other basic rocks identified in the gabbro-peridotite complex are anorthosites, diorites, norites, lherzolites, picrites, saxonites, wehrlites, dunites and pyroxenites. Each type is well described and a discussion of the banding noticed in many of them is given in some detail.

Leucite-Basanites of Vulcanello.—After studying carefully the rocks on Vulcanello in the Lispari Islands, Bäckström[3] concludes

[3] Geol. För. i Stockh. Förhandl., XVIII, p. 155.

that the greater portion of them are leucite-basanites. They all contain phenocrysts of augite, labradorite, olivine and magnetite in a groundmass which is sometimes a holocrystalline aggregate of oligoclase, orthoclase, leucite and magnetite, and at other times of numerous leucites, small augites and iron oxides in a glassy matrix. The rocks are regarded as effusive types of lamprophyres (minettes or kersantites) a supposition which is the more probable from the fact that the effusives in the Lipari province are mainly feldspathic basalts, andesites, liparites and trachytes. Biotite and leucite are thought to be complementary minerals—the former separating from a siliceous magma under considerable pressure, and the latter from a magma of the same composition under surface pressure, under conditions favorable to the escape of the mineralizers fluorine and water. Leucite is not confined to rocks rich in potash, nor is it necessarily characteristic of these. Its place may often be taken by biotite.

A Squeezed Quartz-Porphyry.—A squeezed quartz-porphyry is described by Sederholm[4] as occurring at two places in the Parish of Karvia in Province Abo, Finland. In both it appears as dykes cutting granite. The rock consists mainly of microcline phenocrysts to which are often added growths of new microcline in optical continuity with the original crystals, phenocrysts of an acid plagioclase surrounded in many cases by microcline substance and quartz phenocrysts in a groundmass of orthoclase and quartz. The twinning of the microcline is more largely developed around quartz enclosures in the phenocrysts and near quartz veins than elsewhere in the crystals. The porphyritic quartzes occasionally retain their dehenhedral contours, but usually they are much deformed in outline and in their optical characteristics. Often the quartzes are so shattered that they now constitute lenticular areas of a quartz mosaic. The structure of the groundmass is in several types. In the most important one it consists of a micropegmatite of orthoclase and quartz containing shreds of chlorite, which in some cases are distributed so as to exhibit a fluidal arrangement. The granite through which the porphyry cuts is a coarse grained porphyritic variety composed of oligoclase, biotite and hornblende. On the contact with the dyke rocks it is crushed and much epidote is developed in it. Under the microscope it presents the usual aspects of a dynamically metamorphosed rock. In his discussion concerning the name to be applied to the porphyry, the author quotes from a letter by Dr. Williams in which the prefix 'apo' is defined as signifying that the rock

[4] Bull. Com. Geol. d'Finlande, No. 2, 1895.

to which it refers has been changed from its original character through devitrification.

Mica-Syenites at Rothschonberg.—Two dykes of mica-syenite cut the phyllite formation near Rothschönberg, Saxony, producing in the neighboring rocks contact metamorphism. One of the dykes weathers spheroidally, and in the kernels of the spheroids fresh material for study was afforded Henderson,[5] who found the rock to be composed of orthoclase, plagioclase, quartz, biotite, apatite and several accessory components. The feldspar and quartz both occur in grains and in crystals, the biotite in flakes. An analysis of the rock gave the figures below (I).

The second occurrence differs little from the first. Muscovite is present as well as biotite, otherwise the two rocks are practically alike in mineral composition. Its chemical composition is shown in (II).

	SiO_2	Al_2O_3	Fe_2O_3	CaO	MgO	K_2O	Na_2O	H_2O	CO_2	S	Total
I.	61.40	16.66	7.46	2.08	3.65	2.93	4.75	.76	1.54	.20	=101.43
II.	57.63	16.47	5.37	5.25	4.44	3.12	5.15	.45	2.14	.95	=100.97

The structure of both rocks was panidiomorphic, although the development of secondary quartz renders them now hypidiomorphic. They are syenitic aplites. In the neighboring phyllites new biotite has been abundantly developed and hornblende has been produced in some quantity. The free silica which is abundant in the unaltered phyllites has become combined with metallic elements in the altered forms. While the percentage of silica in specimens taken at 2 meters and 11 meters from the contact and at the contact is the same, the free quartz in the first is 43.38 per cent of the rock's mass, in the second 38.94 per cent and in the third 34.06 per cent.

GEOLOGY AND PALEONTOLOGY.

Cambrian Rocks of Pennsylvania.—During the years 1892 and '93, Mr. Walcott made an examination of the rocks of Cambrian southeastern Pennsylvania for the purpose of determining whether the lower quartzites with their superjacent limestones were of the same geologic age, in the areas included between the Potomac and Susquehanna and the Delaware and Susquehanna Rivers. The results of his work are published in bulletin form by the U. S. Geological Survey.

[5] Zeits. d. geol. Ges., XLVII, p. 534.

Mr. Walcott began the investigation in York County, where he determined the stratigraphic position of the Chickies quartzites and the York shales which are subjacent to the Lancaster (York Frazer) limestone. Paleontologic evidence shows them to be of Lower Cambrian age. The fauna of the main body of the limestone of York County, as shown by collections from three separate localities, is Cambrian. These localities indicate respectively, an Upper Olenellus zone, a horizon between the Lower and Middle Cambrian, and a lower horizon of the Middle Cambrian.

The discovery of Lower Cambrian fossils in the compressed syncline of limestone in Lancaster County, south of Columbia, indicates that the limestone on the west side of the river is of the same geologic age, and that the shales and schists beneath it are of Lower Cambrian age.

Mr. Walcott states that in York County there is no sedimentary rock other than the mesozoic new red sandstone—of later age than the Cambrian, unless it be the Peach Bottom slates and chlorite-schists of the southeastern corner of the country. He also thinks it probable from the closely related structure of Lancaster County that all the Lancaster limestones will fall within the Cambrian, unless it be some portions of the upper series, which may pass into the Ordovician. He applies this generalization to the entire extension of this series of limestone northeastward to the Delaware.

All of the quartzites that have been referred to the Potsdam necessarily fall into the Lower Cambrian, as they are beneath the limestones.

The South Mountain chain, as stated by Professor Lesley and Dr. Frazer, consists of two groups of rocks, a quartzite and an orthofelsite series, the latter being considered the superior series. Mr. Walcott's investigations lead him to a different interpretation of the geologic structure of the mountain and the relations of the rocks composing it. He finds that the "orthofelsite" is in reality the lower series, and that the complicated structure of the mountain arises "partly from folding, but more largely from the westward thrusts of masses of strata along the lines of fault of a low hade. This westward thrusting on the fault plane, complicated by previous folding of the strata, leaves masses of the subjacent, pre-Paleozoic rocks resting in various places on different members of the Lower Cambrian series, and also appears to interbed the quartzites and schists of the Cambrian in the schists, eruptives, etc., of the Algonkian."

The following are the concluding remarks in the section on Correlation:

"The discovery of the Olenellus or Lower Cambrian fauna in the Reading sandstone practically completes the correlation of the South Mountain, Chickies and Reading quartzites, and establishes the correctness of the early correlations of McClure, Eaton, Emmons and Rogers. They all considered the basal quartzite as the same formation from Vermont to Tennessee; and the discoveries of recent years have proved that the basal sandstones of Alabama, Tennessee and Virginia (Chilhowee quartzite); Maryland, Pennsylvania and New Jersey (Reading quartzite); New York and Vermont (Bennington quartzite), were all deposited in Lower Cambrian time, and that they contain the characteristic Olenellus fauna throughout their geographic distribution. The superjacent limestones carry the Olenellus fauna in their lower portions in northern and southern Vermont, eastern New York, New Jersey and Pennsylvania. To the south of Pennsylvania the lower portions of the limestones appear to be represented by shales, and the Upper and Middle Cambrian faunas are found in the lower half of the Knox dolomite series of Tennessee, and they will probably be discovered in the same series in Virginia and Maryland when a thorough search is made for them. The same may be predicted, but with less assurance, for the northern belt of limestone crossing Pennsylvania into New Jersey as the limestones between the Olenellus zone and the Trenton zone represent the intervals of the Middle and Upper Cambrian and the Lower Ordovician, or the Calciferous and Chazy zones of the New York section."

Nothing was discovered upon which could be based a line of demarcation between the Cambrian and Ordovician linestones in the series under discussion. The division is still an open question to be decided by future revelations of lithologic and paleontologic characters. (Bull. U. S. Geol. Surv., No. 134, Washington, 1896.)

Structure of Uintacrinus.—Since Uintacrinus was first made known by Grinnell, some twenty years ago, little notice has been taken of the form. Of late, however, special interest in the type has been revived, and the form comes in for consideration in several important articles. Among them are W. B. Clark's review of the Mesozoic Echinodermata of the United States[1], in which all known material is described, and the structure amply illustrated by figures. Shortly after Williston and Hill[2] gave some "Notes on *Uintacrinus socialis*" as found in Kansas. Still more recently Bather[3] has gone over all the

[1] U. S. Geol. Sur., Bull. 97, pp. 21-24, 1893.

[2] Kansas Univ. Quarterly, Vol. III, pp. 19-21, 1894.

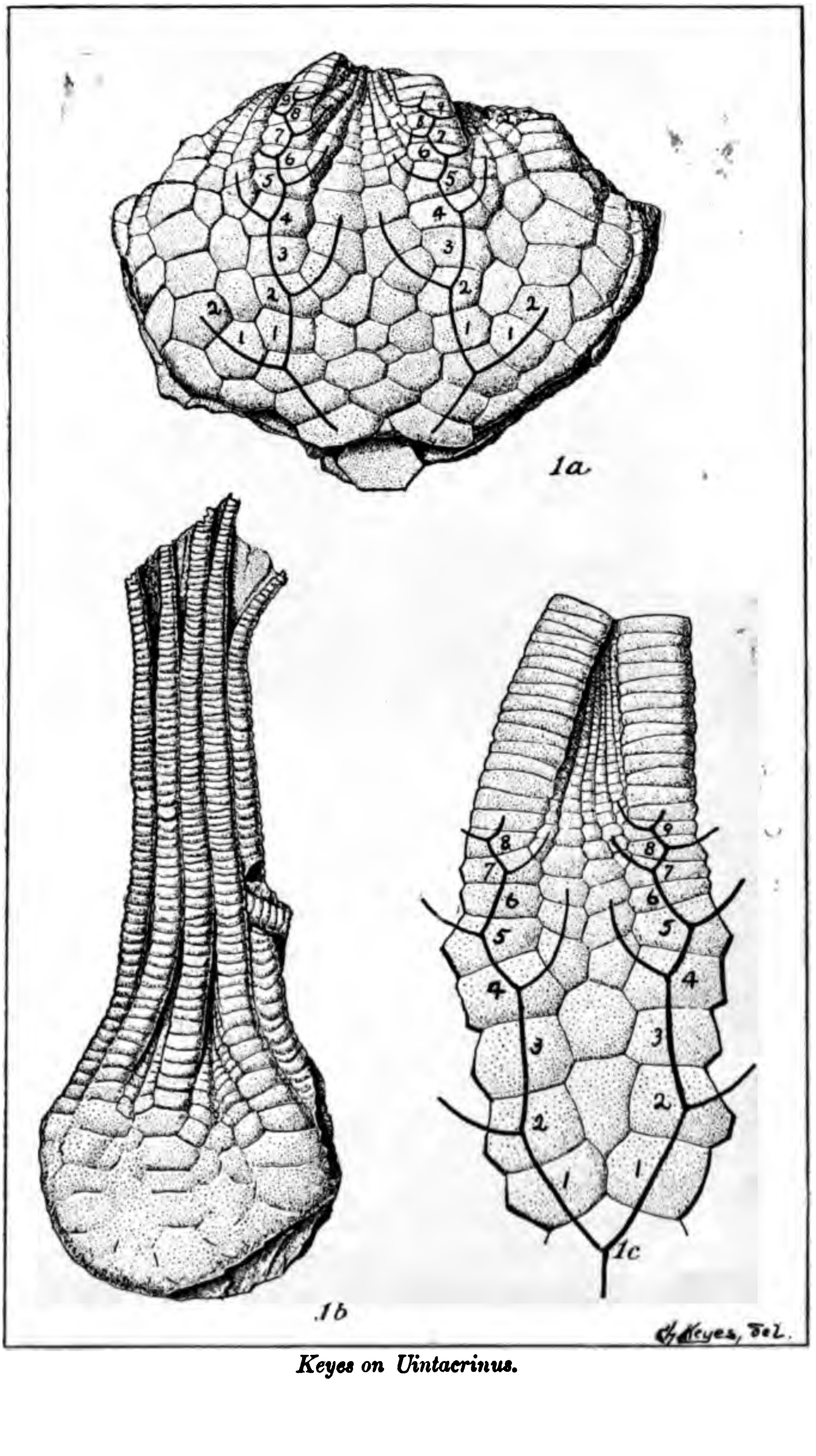
[3] Proc. Zool. Soc., London, 1895, pp. 974-1004, 1896.

available data, and has made the type of Uintacrinus the subject of a a special morphological study. In this treatment the previous work is briefly referred to; but some of it receives criticism that it does not appear to deserve, particularly since the foundation of most of the adverse comments lies not in any material error in the work referred to, but in what is manifestly a clear misinterpretation or hasty perusal of that work. It is to certain of these points in the structure of the form that attention is directed in the present note. In making the correction, however, it is not with the idea of reflecting on Mr. Bather's paper as a whole, for it is one of the most excellent contributions to echinoderm morphology that has yet appeared. The original figures in question from Bulletin 97 of the U. S. Geological Survey are herewith reproduced (Plate XV) in order to make more intelligible the exact points under consideration.

In the memoir mentioned considerable space is occupied in criticising a recent account of the species; but most, if not all of the objections urged against Doctor Clark's work, are certainly more imaginary than real. Professor Clark's figures come in for special condemnation as violating the fundamental law of the alternation of the pinnules. As a matter of fact his plate which is reproduced in the Proceedings as Plate LVI, to point out the alleged errors, not only shows that the accompanying statements are not true, but that in all three figures there is strict alternation of the pinnules in every case.

The general law in the pinnulation of the genus Mr. Bather states as follows: $IIBr_1$ none, $IIBr_2$ outer, $IIBr_3$ none, $IIBr_4$ inner, $IIBr_5$ outer, $IIBr_6$ none, $IIBr_7$ inner, $IIBr_8$ outer, $IIBr_9$ none. Two of his ten specimens differed from this general rule: one showing $IIBr_6$ inner, $IIBr_7$ none, $IIBr_8$ outer, $IIBr_9$ inner; and the other $IIBr_5$ none, $IIBr_6$ outer, IIB_7 inner, $IIBr_8$ none, $IIBr_9$ outer. He makes out the formula for the Clark figure 1a to be $IIBr_5$ outer, $IIBr_6$ none, $IIBr_7$ outer. The real formula for this is $IIBr_5$ outer, $IIBr_6$ inner, $IIBr_7$ outer, $IIBr_8$ inner, $IIBr_9$ outer. This appears clearly indicated in the figure, and Mr. Bather's statements that $IIBr_6$ has no pinnule is certainly a typographical error, for it cannot be that he mistook the rough, broken and highly raised edge of the brachial row of plates, with its deep shadow, for a suture line connecting with the first inside pinnule. The figure is of a somewhat crushed and distorted specimen, and the perspective is, perhaps, not as good as it might be. Whether or not it is the same as that figured by Meek (Bather's figure 2) is not known; but if the two are the same, the difference in the sketches are not very great nor radical, and certainly not as contradictory as Mr. Bather would have

PLATE XV.



Keyes on Uintacrinus.

boundary, to be 3000 feet. This differs considerably from the earlier estimates. (Amer. Geol., 1896.)

Bulletins No. 6 and No. 7, (1895) and No. 8, (1896) of the Illinois State Museum of Natural History contain descriptions of new Paleozoic Echinodermata, by S. A. Miller and Wm. F. E. Gurley. One new family (Thalamocrinidæ) and five new genera (Sampsonocrinus, Emperocrinus, Shumardocrinus, Thalamocrinus, Indianocrinus) are defined. In all, 156 species are described and figured.

MESOZOIC.—According to F. H. Knowlton, the fossil flora of Yellowstone Park represents three distinct stages. The first, or older flora, from the acid rocks embraces 79 forms; the second, or intermediate flora, has 30 species; and the third, or younger flora, comprises 70 forms. The author refers the first stage division to the Ft. Union or lower Eocene; the second is regarded as Miocene, but older than the Auriferous Gravels; and the third is probably of the same age as the Auriferous Gravels of California, that is, Upper Miocene. (Amer. Journ. Sci., July, 1896.)

A new fossil plant, *Salvinia elliptica*, is described and figured by Prof. Hollick. The new species is from the Upper Cretaceous of Washington State. (Bull. Torrey Botan. Club, Vol. 21, 1894.)

BOTANY.[1]

Botany at Buffalo.—In August (21 to 28) there were three botanical meetings held in Buffalo, as follows:

The Botanical Society of America met on Friday and Saturday in the High School, with eleven members in attendance. C. H. Peck, of Albany, and B. T. Galloway, of Washington, were elected to membership. The question of the desirability of a winter meeting was discussed and referred to the Executive Committee. Appropriate resolutions regarding the death of M. S. Bebb, a member, were adopted. The address of the retiring President, William Trelease, on "Botanical Opportunity," was given in open session on Friday evening. This will be printed in full in *Science* and the *Botanical Gazette*, and will also be distributed in pamphlet form by the Secretary. The following papers were accepted for presentation:

The Philosophy of Species-making. By L. H. Bailey.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

Some Characteristics of a Fresh-water Insular Flora. By Conway MacMillan.

Some Problems in Sporophyll Transformation among Dimorphic Ferns. By G. F. Atkinson.

A Species of Eleocharis New to North America. By N. L. Britton.

In the election of officers for the ensuing year, the following were chosen: John M. Coulter, Chicago, President; Charles S. Sargent, Brookline, Vice-President; Arthur Hollick, Brooklyn, Treasurer; Charles R. Barnes, Madison, Secretary; Benjamin L. Robinson, Cambridge, and Frederick V. Coville, Washington, Councillors.

The Botanical Section (G) *of the American Association for the Advancement of Science.* An unusually large number of papers were read before the Section, and it is not too much to say that in point of importance they fully maintained the high average of recent years:

1. The Relation of the Growth of Leaves to the CO_2 of the Air. By D. T. MacDougal.

2. Directive Forces Operative in Leaf Rosettes. By R. N. Day.

3. On *Crataegus coccinea* and its segregates. By N. L. Britton.

4. The Distribution of the species of *Gymnosporangium* in the South. By L. M. Underwood and F. S. Earle.

5. Morphology of the *Canna* Flower. By L. H. Bailey.

6. A Comparison of the Flora of Erie Co., Ohio, with that of Erie Co., New York. By E. L. Moseley.

7. The Significance of Simple and Compound Ovaries. By C. E. Bessey.

8. On the Bacterial Flora of Cheddar Cheese. By H. L. Russell.

9. The Terminology of Reproduction and Reproductive Organs. By C. R. Barnes.

10. A Comparative Study of the Development of some Anthracnoses in Artificial Cultures. By Bertha Stoneman.

11. The Development of Vascular Elements in the Primary Root of Indian Corn. By W. W. Rowlee.

12. Some Remarks on Chalazogamy. By J. M. Coulter.

13. The Habits of the Rarer Ferns of Alabama. By L. M. Underwood.

14. On the Stem Anatomy of Certain *Onagraceae.* By Francis Ramaley.

15. The Point of Divergence of Monocotyledons and Dicotyledons. By C. E. Bessey.

16. Notes on the Pine Inhabiting Species of *Peridermium.* By L. M. Underwood and F. S. Earle.

17. Reaction of Leaves to Continual Rain-Fall. By D. T. MacDougal.

18. Studies in Nuclear Phenomena, and the Development of the Ascospores in Certain *Pyrenomycetes*. By Mary A. Nichols.

19. The Stigma and Pollen of *Arisaema*. By W. W. Rowlee.

20. Notes on the Genus *Amelanchier*. By N. L. Britton.

21. Remarks on the Northern Species of *Vitis*. By L. H. Bailey.

22. On the Formation and Distribution of Abnormal Resin Ducts in Conifers. By Alex. P. Anderson.

23. The Development of the Cystocarp of *Griffithsia bornetiana*. By Arma A. Smith.

24. Notes on the Allies of the Sessile *Trillium*. By. L. M. Underwood.

25. On an Apparently Undescribed *Cassia* from Mississippi. By C. L. Pollard.

26. A Bacterial Disease of the Squash-Bug (*Anasa tristis*). By B. M. Duggar.

27. What is the Bark? By C. R. Barnes.

28. Embryo-Sac Structures. By J. M. Coulter.

29. Some *Cyperaceæ* New to North America, with Remarks on Other Species. By N. L. Britton.

30. Grasses of Iowa. By L. H. Pammel.

31. Ceres-Pulver: Jensen's New Fungicide for the Treatment of Smut. By W. A. Kellerman.

32. On the Cardamines of the *C. hirsuta* group. By N. L. Britton.

33. The Relation Between the Genera *Polygonella* and *Thysanella*, as Shown by a Hitherto Unobserved Character. By John K. Small.

34. An Apparently Undescribed Species of *Prunus* from Connecticut. By John K. Small.

35. The Flora of the Summits of King's Mountain and Crowder's Mountain, North Carolina. By John K. Small.

36. Parthenogenesis in *Thalictrum fendleri*. By David F. Day.

37. Notes on the Order *Pezizineae* of Schröter. By Elias J. Durand.

38. What Should Constitute a Type Specimen? By S. M. Tracy.

39. Rheotropism and the Relation of Repose to Stimulus. By F. C. Newcombe.

40. Some Adaptation of Shore Plants to Respiration. By Herman von Schrenk.

41. The Mechanism of Curvature in Tendrils. By D. T. MacDougal.

42. A Contribution to Our Knowledge of Turgor. By E. B. Copeland.

The Botanical Club. This vigorous organization fully justified its existence again. About forty papers were read. Many of these, of course, were notes, but others were of considerable value. Among the more important of these were the following:

The Distribution of *Phoradendron flavescens*, *Polypodium polypodoides* and *Bignonia crucigera* in Ohio. By W. A. Kellerman.

A Method of Distributing Fungi in Pure Cultures. By L. R. Jones.

Notes on Some Mosses. By Mrs. E. G. Britton.

Notes on *Iris*. By David F. Day.

An Improved Paraffin Bath. By F. C. Newcombe.

Notes on Oaks. By W. W. Rowlee.

Some Notes on Potato-Leaf Fungi. By L. R. Jones.

A Method of Preventing Condensation of Water in Culture Dishes. By H. L. Russell.

Notes on the Flora of Colorado Springs, Colorado. By Charles E. Bessey.

On a Species of *Epipactis*. By Elias J. Durand.

A Report Upon the National Herbarium. By C. L. Pollard.

Schizaea pusilla from Newfoundland. By Mrs. E. G. Britton.

Photosyntax *vs* Photosynthesis. By C. R. Barnes.

The Distribution of *Pinus ponderosa* in Nebraska. By Charles E. Bessey.

Some Curious Sunflowers. By J. F. Cowell.

Notes on Species of *Mnium*. By Mrs. E. G. Britton.

The Canyon Flora of the Plains. By Charles E. Bessey.

The Turgor of Mosses. By E. B. Copeland.

A Simple Apparatus for Spraying and Infecting Plants. By A. P. Anderson.

The Structure of Pseudoparenchyma in Higher Fungi. By Elias J. Durand.

Note on the Hosts of *Comandra umbellata* and *C. pallida*. By Herman von Schrenk.

The Submerged Leaves of *Salvinia natans*. By Conway MacMillan.

Nuclear Budding in *Cypripedium*. By Conway MacMillan.

An Unusual Adaptation of Conifers for Wind Protection. By Conway MacMillan.

Some Plants New to the Rochester Flora. By Florence Beckwith.

The Committee on Nomenclature made a report recommending that:

(1) A list of North American Pteridophyta and Spermatophyta be prepared for publication.

(2) A supplement be prepared to the present List of Pteridophyta and Spermatophyta of the Northeastern United States.

(3) The Rochester-Madison Rules be republished with annotations and explanations.

The officers for the next year are: President, S. M. Tracy, Agricultural College, Miss.; Vice-President, L. R. Jones, Burlington, Vt.; Secretary-Treasurer, E. Burgess, New York.

A New Manual of Systematic Botany.[1]—It is many years since American botanists have had the pleasure of examining a new manual of systematic botany designed for use in the northeastern States. We have had new editions of old books, but, unfortunately for scientific progress, through a remarkable misconception of the duties of executors, these editions were new mainly in type and binding, the additions and modifications having been purposely reduced to a minimum. It has been a matter of profound regret on the part of many of the friends and admirers of Dr. Gray that his books should receive such a treatment as to prematurely relegate them to the list of antiquated and obsolescent works. A new manual is, therefore, of peculiar interest at the present time, and this interest is enhanced by the fact that it comes from the scientific home of the older botanist, Torrey.

The volume before us is the first of the three volumes which will include descriptions and figures of all the ferns and flowering plants of the northeastern States and Canada. It is in every way a new work—new in its plan, new in its descriptions, new in its illustrations. Volume I opens with an eight-page introduction which is historical and explanatory. Here we learn that more than 4,000 species will be included, and that nearly three-fourths of these will be figured for the first time. Discussions follow on the principles of classification of plants, and the systematic arrangement adopted in the work (Engler and Prantl's). The following quotations are useful and suggestive: "The Nineteenth Century closes with the almost unanimous scientific judgment that the order of nature is an order of evolution and development from the more

[1] *An Illustrated Flora of the Northern United States, Canada and the British Possessions* from Newfoundland to the Parallel of the Southern Boundary of Virginia, and from the Atlantic Ocean Westward to the 102d Meridian. By Nathaniel Lord Britton, Ph.D., Emeritus Professor of Botany in Columbia University, and Director-in-Chief of the New York Botanical Garden, and Hon. Addison Brown, President of the Torrey Botanical Club. Vol. I, Ophioglossaceae to Aizoaceae. New York: Charles Scribner's Sons. 612 pages, octavo, $3.00.

simple to the more complex." * * "Systematic arrangement should logically follow the natural order." * * "The sequence of families adopted fifty or seventy-five years ago has become incongruous with our present knowledge, and it has, for some time past, been gradually superseded by truer scientific arrangements in the later works of European authors." "The more simple forms are, in general, distinguished from the more complex, (1) by fewer organs or parts; (2) by the less perfect adaptation of the organs to the purposes they subserve; (3) by the relative degree of development of the more important organs; (4) by the lesser degree of differentiation of the plant body or of its organs; (5) by considerations of antiquity, as indicated by the geological record; (6) by a consideration of the phenomena of embryogeny. Thus, the Pteridophyta, which do not produce seeds and which appeared on the earth in Silurian time, are simpler than the Spermatophyta; the Gymnospermae in which the ovules are borne on the face of a scale, and which are known from the Devonian period onward, are simpler than the Angiospermae, whose ovules are borne in a closed cavity, and which are unknown before the Jurassic."

"In the Angiospermae the simpler types are those whose floral structure is nearest the structure of the branch or stem from which the flower has been metamorphosed, that is to say, in which the parts of the flower (modified leaves) are more nearly separate or distinct from each other, the leaves of any stem or branch being normally separated, while those are the most complex whose floral parts are most united."

"The sequence of families adopted by Engler and Prantl, in 'Natürliche Pflanzenfamilien' above referred to, has been closely followed in this book, in the belief that their system is the most complete and philosophical yet presented. The sequence of genera adopted by them has, for the most part, also been accepted, though this sequence within the family does not attempt to indicate greater or less complexity of organization."

The nomenclature is that of the well-known Rochester-Madison Rules of the Botanical Club of the American Association for the Advancement of Science, and, in order that the student may fully understand the matter, the rules are printed in full, with explanatory notes. As this work will at once become the standard botanical manual everywhere in this region for which it is designed, the revised nomenclature will soon be more familiar than the old which it is rapidly superseding.

The work proper opens with the family *Ophioglossaceae*, the lowest of the Pteridophyta, in which one species of *Ophioglossum*, and six of *Botrychium* are described and figured. Then follow *Osmundaceae* (3

species), *Hymenophyllaceae* (1 sp.), *Schizaeaceae* (2 sp.), *Polypodiaceae* (59 sp.), *Marsileaceae* (2 sp.), *Salviniaceae* (2 sp.), *Equisetaceae* (11 sp.), *Lycopodiaceae* (11 sp.), *Selaginellaceae* (3 sp.), and *Isoetaceae* (8 sp.).

The flowering plants, as we have been calling them, are here more correctly called seed-bearing plants (*Spermatophyta*), and are properly divided into two classes—*Gymnospermae* and *Angiospermae*, and the latter into the sub-classes *Monocotyledones* and *Dicotyledones*. As one turns over the pages, reading the full descriptions and comparing them with the excellent illustrations (which are always by the side of the descriptions) the conviction deepens that this book is one of the most important systematic works yet produced in this country. This is well illustrated in the treatment of the sedge and grass families which fill two hundred and sixty-six pages. Any one who has tried to puzzle out the genera and species of grasses and sedges will not have to be told of the great advantage which good figures will give to the student of these difficult families. It is not too much to say that no publication hitherto made has done so much to popularize the study of these plants as the one now before us. Of *Carex* alone two hundred and five species are figured!

We have not the space at our command to speak of the many changes in generic and specific limits with which this work will familiarize us. Nearly all of these have been known to specialists and those who have kept their eyes on the work of the German systematists, but to many the changes will come as novelties.

Among the minor matters may be mentioned the abandonment of the absurdity of calling families "orders," thus conforming to the usage in other departments of biological science. All family names (with a very few exceptions) are now made to end in *aceae*, a commendable practice which Dr. Gray used to insist upon. Throughout the work all diphthongs are printed in separate letters (*ae*, *oe*), and not in single characters (*æ*, *œ*), thus again conforming to the German usage. In the rules for pronunciation, a little easement is made for the use of the Roman pronunciation of botanical names, which we wish had been made a little more evident. We regret to see the use of feet, inches and lines still adhered to in this otherwise modern work. The metric units are so generally used in scientific books everywhere that we are surprised at this unexpected anachronism.

As we carefully study the beautifully-printed pages of this work, we are more and more impressed with its magnitude and importance. It will give renewed life and vigor to systematic botany, and doubtless will be the means by which many a student will be led to the study of

the more difficult families. It is so valuable in so many ways that we wish it would be made available to a greater number of students. We venture to suggest to authors and publishers that they bring out an edition *without the illustrations*, and printed on thin paper, with narrow margins. A single volume, portable edition of this admirable book, would greatly extend its usefulness.—CHARLES E. BESSEY.

ZOOLOGY.

The Heart of Some Lungless Salamanders.[1]—The recent literature of zoology has, perhaps, contained nothing more unexpected and startling than that certain adult salamanders are entirely lacking in those respiratory organs which, heretofore, have been deemed indispensable to the existence of animals so high in the zoological scale as the Amphibia. This total lack of lungs and branchiæ appears the more marvelous when we remember that they are absent in forms which lead a rather active and wholly terrestrial life, as well as in those of more or less purely aquatic habits.

Two questions are naturally suggested by this apparently aberrant condition of the respiratory organs. First, what structures or organs have taken on the functions of the lungs and branchiæ? and secondly, is there any modification in the form or structure of the heart which in any way may be correlated with the above mentioned peculiarities of these lungless forms?

The first of these two questions has been discussed to some extent by Prof. Harris H. Wilder, of Smith College, who first published an account of this apparently anomalous condition. He concluded that respiration was probably carried on by the skin and, perhaps, to some extent, by the mucosa of the intestine. Prof. Camerano has also published the results of some experiments upon two European forms which bear upon this same question. He believes that in these lungless forms respiration is effected in the bucco-pharyngeal cavity, and that the skin affords no efficient aid in the respiratory processes.

In a still later paper he discusses the subject further, and tries to account for the disappearance of the lungs. Of one aquatic species (of the genus Molge) he says: "The function of the lungs as hydrostatic organs, is very marked." "In the clearly terrestrial forms one would say that the diminution in importance of the function of the lungs as hydrostatic organs induces a retrogressive development of them while

[1] Read before the Amer. Assoc. Adv. Science, Aug. 24, 1896.

at the same time the importance of the bucco-pharyngeal respiration is increased."

It appears to the writer that Camerano's conclusions need to be tested by further experiments, especially the part referring to the respiration, for the area of the dermal surface far exceeds that of the bucco-pharyngeal cavity, and the skin is also very richly supplied with blood vessels which are so close to the surface that it would appear as if the gases of the blood and air might be readily interchanged. It is hoped that time will permit of some experiments on this point during the coming year.

As to the second question, whether there is any appreciable modification of the heart in these lungless salamanders, nothing whatever has been published.

It is the object of this paper to call attention to the fact that there is a difference in the heart of those salamanders that do not have lungs and those which do have them. So far as I have examined, it is possible to distinguish between the two forms by examining the heart alone.

In order that what is said on this point may be clearly understood by every one, and in order to bring out the differences between the two more sharply, if possible, I wish first to recall to mind the structure of the Amphibian heart and then contrast with it the relations as found in the heart of a lungless individual. We may take Huxley's description of the Amphibian heart as our standard of comparison. In his Anatomy of Vertebrates he says: "The heart presents two auricles, a single ventricle and a bulbus arteriosus. A venous sinus, the walls of which are rhythmically contractile, receives the venous blood from the body and opens into the right auricle. The left auricle is much smaller than the right and a single pulmonary vein opens into it." In regard to the septum of the auricles, he says that "it is less complete in Proteus, Siren and Menobranchus (Necturus) than in other Amphibia. In Menobranchus the septum is reduced to little more than a wide meshed network of branched muscular bands, and in Proteus the existence of a septum is doubtful."

The heart of our common Newt (Diemyctylus viridescens) Fig. 1 or of the large yellow-spotted salamander (Amblystoma punctatum), for examples, corresponds perfectly with Huxley's description. In both of these forms the auricular septum is perfectly complete, the cavities of the auricles being entirely separated, except just at the auriculo-ventricular aperature, at which point the two auricles communicate with each other to some extent.

In Necturus, the septum is more or less fenestrated and, according to Huxley, it is very incomplete in Proteus and Siren, but in all of the

PLATE XVII.

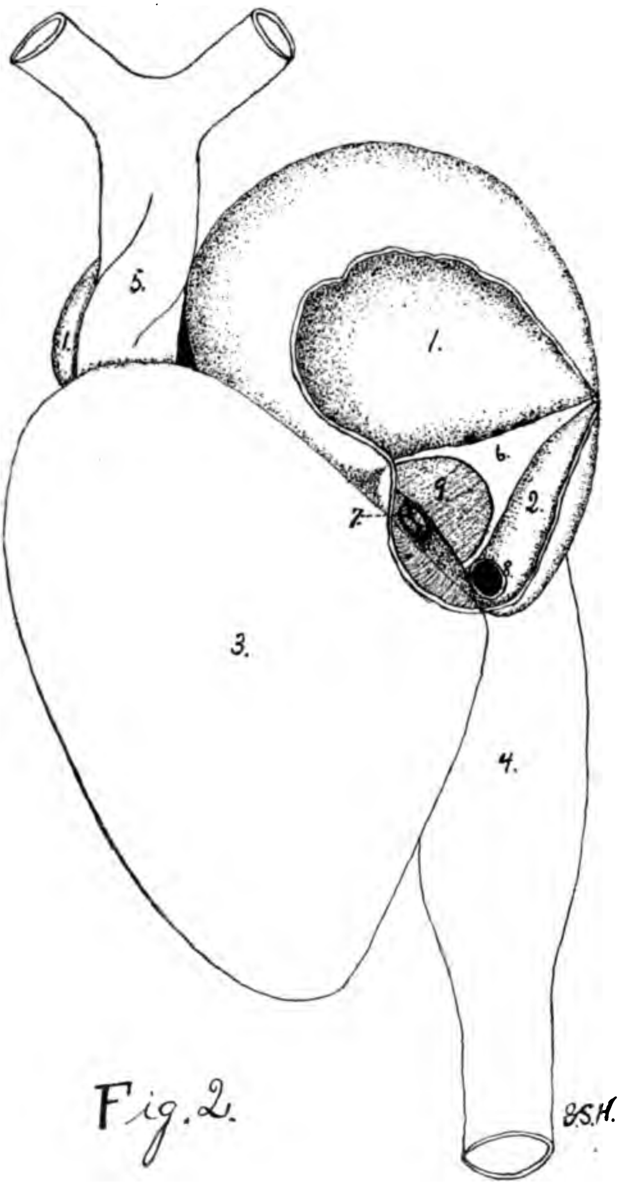


Fig. 2.

Hopkins on Desmognathus.

forms that have been mentioned, as well as in other members of the class Batrachia, the sinus venosus opens distinctly into the right auricle and the pulmonary vein into the left.

Let us now compare the heart of a lungless salamander (Fig. 2.) with the one just described. The four parts, auricles, ventricle, bulbus arteriosus and sinus venosus are clearly recognizable and, superficially examined, present nothing unusual; it is only when the cavities are opened that the differences between the two hearts become apparent. One of the first things to attract attention is the left auricle. In the lungless forms examined, it is much smaller in comparison to the right than in Diemyctylus, for example, and *no pulmonary vein was found opening into it.*

The auricular septum has only one opening through it, or perhaps, more correctly, it extends only part way across the cavity, but this aperture in the septum is so large (Fig. 2, 9.) that it is believed the communication between the two cavities is more free than even in Necturus. Just what function or functions the septum may have in these lungless forms, it seems to me, is not quite clear. That it has but little, if any use, is indicated by the way the sinus venosus opens into the auricles. In place of opening into the right auricle only, as in the forms having lungs, *it opens more freely into the left auricle than into the right.* If the ventral parietes of the heart be removed, one can look directly into the opening of the sinus venosus from either of the auricles, but more directly into it from the left than from the right, for when seen from the latter, one must look through the large opening of the auricular septum, Fig. 2, 9. In salamanders with lungs, each auricle opens in common into the ventricle with about equal freedom of communication, whereas in the lungless forms the right auricle is in more direct communication with the ventricle than is the left.

Judging from the above facts, i. e., the way the sinus venosus opens into the auricles, the freedom with which the auricles communicate with each other, and the way the auricles communicate with the ventricle, it would seem as if the heart of the lungless salamanders, functionally, was only bilocular in place of being trilocular as in the rest of the Amphibia. Morphologically, of course, it is trilocular, but whether it is so physiologically seems to me doubtful.

The heart of 8 lungless species have been examined by the writer, and so far as was made out, all of them agree closely with the description as given above. The probabilities are that in all the lungless forms similar conditions of the heart will be found. Up to the present time 17 species and sub-species, either wholly without lungs or with only

functionless rudiments of them, have been reported. In his last paper, in which are enumerated 15 of the 17 lungless species, Wilder says that "in the Salamandridæ lungless species are as numerous as those possessing lungs, and that in consequence of this, the definition of the group must be modified." It seems, however, that even with his proposed additions, the definition is still not sufficiently comprehensive, for the peculiarities in the structure of the heart certainly have almost as profound a significance as the absence of the lungs themselves, and should be incorporated in any definition that may be given. In addition to the 17 lungless species already mentioned, the writer has found an additional one, *Spelerpes gluttolineatus*.

In order that one may see at a glance in which families and genera lungless individuals are found, the following table, taken from Prof. Cope's Batrachia of North America, is appended. [The last column is taken from the papers of Wilder and others].

Families.	Genera.	No. of species.	No. species without lungs or with only rudiments of them.
Cryptobranchidae	Cryptobranchus	2	
	Megalobatrachus	1	
Amblystomidae	Amblystoma	12 [N. A.] 1 [Siam]	1 A. opacum
	Chondrotus	7	
	Linguaelapsus	2	
	Dicamptodon	1	
Hynobiidae [all Asiatic]	Hynobius	5	
	Salamandrella	2	
	Onychodactylus	1	
	Ranidens	3	
	Batrachyperus	1	
Plethodontidae	Plethodon	8	2 P. cinerus P. erythronotus P. glutinosus
	Hemidactylium	1	
	Batrachoseps	4	1 B. attenuatus
	Stereochilus	1	
	Autodax	3	1 A. lugubris
	Geotriton	? [European]	1 G. fuscus
	Gyrinophilus	1	1 G. porphyriticus
	Manculus	2	1 M. quadridigitatus
	Spelerpes	9	3 S. bilineatus S. ruber S. gluttolineatus
	Oedipina		
	Oedipus	9	
Thoriidae	Thorius	1	1 O. variegatus
Desmognathidae	Desmognathus	3	1 D. fusca D. f. brimleyorum D. f. auriculatus

Families.	Genera.	No. of species.	No. species without lungs or with only rudiments of them.
Salamandridae [Old World]	Chioglossa	1	
	Salamandra	3	
	Hemisalamandra	1	
	Triturus	6	
	Pachytriton	1	
Pleurodelidae [All found in Old World; three species in N. A.]	Salamandrina	1	1 S. perspicillata
	Diemyctylus	10 [2 N. A. species]	
	Pleurodeles	1	
	Glossolega	3	
Amphiumidae	Amphiuma	1	
Coeciliidae	(numerous)	(numerous)	

In the last column of the above table, the figures indicate the number of species in which lungless individuals have been found. Where there is a discrepancy in the numerals and the number of species following them, it indicates either sub-species or species not mentioned in Cope's Batrachia of North America.

DESCRIPTION OF FIGURES.

FIG. 1. Heart of Diemyctylus viridescens (semi-diagramatic) to show the general relations of the heart of a salamander with lungs. The ventral wall of the heart has been removed in order to show the auricular septum, the openings of the sinus venosus and the pulmonary vein, and also the relation of the auriculo-ventricular aperture to the right and left auricle.

1. Right auricle; 2. Left auricle; 3. Ventricle; 4. Sinus venosus; 5. Bulbus arteriosus; 6. Auricular septum; 7. Auriculo-ventricular aperture; 8. Aperture of sinus venosus; 9. Pulmonary vein.

FIG. 2. Heart of Desmognathus fusca (semi-diagramatic) to show relations of the heart in a lungless salamander. The ventral wall of the heart has been removed.

1. Right auricle; 2. Left auricle; 3. Ventricle; 4. Sinus venosus; 5. Bulbus arteriosus; 6. Auricular septum; 7. Auriculo-ventricular aperture; 8. Aperture of sinus venosus; 9. Opening through auricular septum.

G. S. HOPKINS, D. Sc., Cornell Univ.

On two new Species of Lizards from Southern California.—ANOTA CALIDIARUM Cope.—A single lateral fringe of conic scales, extending on three-quarters the length between the axilla and groin;

no trace of inferior fringe. Enlarged lateral gular scales only traceable below the rictus oris. Occipital horns moderate, each with a short accessory horn at the external base; internal temporal horn half as long as the occipital, with a short accessory horn at the external base; external temporal horn very short, and the temporal anterior to it presenting a serrate edge only. Infralabials presenting a serrate edge only; parietal region bounded on each side by an angulated border which overhangs the temporal region.

Squamation of the head smaller than in other species; superior labials twelve, below the middle of the eye, instead of eight or nine in *A. platyrhina* the nearest allied species. Seven subequal scales in the transverse row between the canthal rows on the frontal angle; there are five unequal scales on the corresponding position in the *A. platyrhina*. Six longitudinal rows of supraocular scales, of which a group of five or six posterior to the middle are larger, but unequal. Supraorbital rows in contact, except at points, on the median line; last superciliary presenting a sharp angle; penultimate also presenting a prominent angle. Tomia of mouth only moderately serrate; a row of conic scales rising posteriorly on the side of the neck, and above its posterior end an inconspicuous rosette. A conspicuous rosette above the middle of the humerus.

This species is nearest to the *A. platyrhina* Gir. from which it differs in various respects. The general proportions of all the parts and the coloration are about as in that species, the difference chiefly appearing in the squamation and the horns. The scales of the head are much more subdivided, and the presence of accessory horns is unique in the genus. The simplicity of the lateral fringe is also characteristic, as is also the rudimental character of the rosette on the neck.

Anota calidiarum Cope.

Catal. No.	Ariz. No.	No. species	Locality.	Source.	Obs.
8444	434	1	Death Valley, Cal.	U. S. Agric. Dept.	Alcoholic.

I may add here that in my estimation the genus Anota Hallow. is valid. It includes the species *A. modesta* Gir., *A. goodei* Stjen., *A. platyrhina* Gir., *A. maccallii* Hallow., and the above described.

SCELOPORUS VANDENBURGIANUS Cope.—This is a small species with small scales and very dark colors. There is not much difference in

the sizes of the dorsal, lateral and ventral scales; forty-five rows may be counted between the occiput and a line connecting the groins, and twelve in a head length. Between the groin and axilla thirty-five scales may be counted to an axillary area of smaller and smooth scales. The dorsal and lateral scales are keeled and mucronate; those of the inferior surfaces smooth and mostly feebly notched. Caudal scales strongly keeled and mucronate and larger than dorsals. Two parietals on each side, the anterior the larger, and extending to the narrow marginal supraocular row, so that there is only one frontoparietal on each side. A third parietal external to the other two. The frontal is not longitudinally divided. There is one series of six large supraoculars, separated from the frontals and frontoparietals all round by a series of small scales. External to the large supraoculars is a series of four much smaller polygonal flat scales much as in *S. biseriatus*. Between these and the superciliaries one row of still smaller scales (with an extra scale or two); two scales on the canthus rostralis. Head scales all smooth; six large free auricular scales. A single vertical prehumeral fold enclosing a pocket of granular scales; temporal scales keeled. The extended hind leg reaches to the auricular meatus. Temporal pores sixteen; male with postanal plates.

Color of adult male dark green above, with faint traces of a paler stripe on each side of the back, and of a few darker spots on each side of the middle line. Inferior surface dark blue, with a pale line in the middle of the abdomen. Femur spotted with blue below; tibia and tail light greenish below.

Measurements.

	MM.
Total length,	127
Length to vent,	57
Length to line of axilla,	22
Length to line of interparietal plate,	13
Length of hind leg,	38
Length of hind foot,	18
Length of fore leg,	23
Length of fore foot,	10

Sceloporus vandenburgianus Cope.

Catal. No.	No. spec.	Locality.	Donor.	Obs.
21931	1	Summit of Coast Range, San Diego Co., Cal.	Dr. E. A. Mearns.	Alcoholic.

I have seen of this species only one specimen, which is an adult male. The colors of the female may be expected to be somewhat lighter. I have dedicated it to Mr. Jno. Van Denburgh, of San Francisco, an able writer on herpetological subjects.—E. D. COPE.

Modification of the Brain during Growth.[1]—1. One of the most marked changes of the embryo brain is the formation of the great bend near its middle, giving rise to the cephalic fexure. The cause of the bend is unknown. After it is formed, the early appearance of optic fibers and those of the postcommissure and supracommissure tends to produce a comparatively fixed portion at this bend as shown by measurements of parts of the brain in soft shelled turtle and the cat. The later developing parts produce secondary changes in the form of the brain tube as the embryo takes on the specific characters of the adult.

2. The pons bend of embryo mammals is a feature of the brain of many non-mammalian embryos which do not possess a pons, and in specimens examined is associated with and seems to be due to the early and enormous development of the Gasserian ganglion and its union with the brain by the fifth nerve. Later these parts are overshadowed by the growth of other parts and the pons bend becomes obscure.

3. In the cat the pons bend is exaggerated by the formation of a great fist shaped mass of cells at the surface on each side of the meson. This mass of cells is continuous with a conspicuous layer of cells which extends upon the surface to the union of the solid wall of the oblongata with the membranous roof or tela. This layer and cell mass seem to be proliferations such as His and Herrick have described as occurring at the union of a solid wall with a membrane. Later these cells are covered by pons fibers.

4. In the soft shelled turtle and the cat, early stages show clearly that the prosotela, the membrane included by the edges of the rima in the cerebrum, is a continuation of the membranous roof of the diencephal, such that if the brain were plastic the continuation of each side could be brought to occupy the dorsal surface of its cerebrum. A condition would thus occur which is comparable with the actual condition found by Wilder in Ceratodus. It may be of value in the further determination of homologies between the brain of fishes and mammals, as in fishes the membranous roof of cerebrum and diencephal is continuous.

[1] Abstract of paper read before the Amer. Asso. Adv. Sci., Aug. 24, 1896.

5. The dorsal (sensory) and ventral (motor) zones of His, demarcated by a sulcus extending from the myel through the brain to the optic recess, have not been verified in the forms examined (cat, turtle, amphibia, bird). The indications are rather that an original segmented condition is partially disguised by a secondary formation of sulci which extend in a cephalo-caudal direction. None of the five such sulci found in the oblongata could be said to separate the sensory from the motor region. The dorsal and ventral of them demarcate raphés. One of them occurs *in* the sensory, two *in* the motor, but there is no dividing sulcus between the two regions. The extreme point to which any of them could be traced was in the region of the albicans, none of them reaching the porta or the optic recess. A second group of sulci arising in the optic and preoptic recesses extend to the porta, two of them passing through the porta to form the boundary of the striatum.

SUSANNA PHELPS GAGE.

The Lion of India.—The report that *Uncia leo* is found at the present time in the environs of Guzerate and Kutch appears to be an error. It has probably never existed in the latter locality, and is now to be met with only in the forest of Gir in the Kathiawar. It has disappeared from Rajkot, where it was abundant in 1832, from the hills of Bardo, and from many other localities where it formerly existed in great numbers, nor has it been seen for a long time in the forest above mentioned. Formerly hunters very seldom ventured in that region for fear of the bandits who were in the habit of taking refuge there, and also for fear of contracting fever. Gradually, however, the forest is being cleared away, settlements are being made, and the domain of the lion is being curtailed. To prevent total extinction of the species, the Durbar of Kathiawar has forbidden lion hunts for a period of six years. But this will do no good unless at the same time a forest reservation is made.

The popular belief that this species is without a mane in India is another error that is corrected. (Revue Scientif. Août, 1896).

Inheritance of Artificial Mutilations.—The instance cited by Mr. Norman E. Hills (in the September Naturalist) of the birth of short-tailed fox terriors, is striking in showing a larger proportion of deformed puppies than is common in such cases, but instances like the one cited are frequently noted in the press devoted to dogs, and concerning several breeds that have been mutilated for generations.

But to thoroughly consider the matter of the inheritance of artificial deformities, the cases of breeds in which the deformity is usually

natural, should be considered. I believe the tailless Manx cats generally come in that shape, and this I know is often the case with the bobtail sheep-dog of England, and this is stated of several breeds of dogs, generally of the type of the bobtail sheep-dog found in other countries, Norway, Southern Russia and elsewhere.

But the peculiar feature of this inheritance is its freakishness. Two naturally long-tailed parents have produced a tailless dog, in whom the potency was so strong that no bitch, no matter what the breed was, ever produced a full, natural tailed puppy to him. I remember of one puppy by this dog, ex his double grandam (he was the son of litter brother and sister) whose tail was of usual length, but had a deformity in it as though it were tied in a knot. Again, it is not at all uncommon for a bitch to begin production with all naturally long-tailed puppies, and after some years, change to always producing some tailless ones, even when mated with mongrel dogs, while some bitches reverse the order, beginning with tailless ones and changing to full-tailed ones. I recently noted a reported instance of just such a change of production in a Manx cat, and it seems to me that this freakishness introduces a very disturbing element into consideration of the question of inheritance.

As an allied matter, please permit me to say that the notion that if a bitch has a mongrel litter she will thereafter *always* produce puppies showing traces of the unallied sire, is rank rubbish, and on the point that this occurrence is *not* invariable, any experienced breeder will concur, as very, very few such breeders have ever seen such a case. For myself, I have bred dogs for over forty years, have bred many mongrel litters, and never saw a case of telegony—or, as we commonly call it, "influence of previous sire." That this influence does sometimes show itself, is beyond doubt; but some very extended inquiries of mine some years since, showed that it was shown only in about one per cent. of cases of mesalliances; and when it was considered that an instance of this "influence" will be remembered from its extraordinary character, while instances where it does not occur are forgotten, being the expected result, I believe that the one-tenth of one per cent. of cases will be found to be the extent of its occurrence. It is very strange that those scientific men who uphold the idea of the invariable occurrence of this "influence," all overlook the potent fact that the "influence" shows itself invariably only in the skin and hair, never affecting conformation. In view of this, the theory propounded by Dr. Jonathan Hutchinson, President Royal College of Surgeons; Dr. J. Sidney Turner, President South British Medical Society, and Everett Millais, Esq., seems sound, and bears against the idea of the bitch being herself

bastardized, that theory being that unripe ova are partially impregnated by the spermatozoa of the foreign male, not sufficiently to fully vivify them, the influence of this impregnation affecting only the epiblast, from which the skin is evolved, and a subsequent fertilization brings full life to the ova, determining other features of the fœtus. Thus, in the case of a pug bitch, which had a mongrel litter by a Skye terrior, and at her subsequent whelping by a pug dog, had some puppies with rough hair, these "influenced" puppies had the conformation of the pug all over, even to the twisted tail.

However, be the scientific part of the question what it may, the too common idea that a bitch having a mongrel litter will show influence of that litter in all future offspring, is utterly fallacious.

Yours truly, W. WADE.

Oakmont, Pa., Sept. 14, 1896.

ENTOMOLOGY.[1]

A New Character in the Colobognatha, with Drawings of Siphonotus.—In all known Diplopoda the external seminal apertures are located just behind the second pair of legs or in the coxæ of the second legs. In all Diplopoda except Polyxenus one or more pairs of legs are more or less modified to assist in copulation. In a great majority of forms the legs most modified are those of the seventh segment, but in two groups, the Omniscomorpha and Limacomorpha of Pocock the legs of the seventh segment are unmodified, while one or more pairs at the caudal extremity of the body are transformed into copulatory organs. The modification which has taken place in the Limacomorpha is very slight, for according to Mr. Pocock's descriptions and figures the last legs of *Glomeridesmus marmoreus*[2] consist of four or five joints and differ from the others mostly in being shortened and thickened. An equal or greater degree of specialization is now known to exist in other legs than those of the seventh segment, indeed, an almost equal peculiarity of structure is sometimes manifested by nearly all the male legs of certain genera of Polydesmidæ, Spirostreptidæ and Spirobolidæ. Especially noteworthy are the first pair of legs in Iulidæ, Parajulidæ and Pæromopodidæ; the second pair of legs of Stemmatoiulidæ and Parajulidæ; the third pair in Striariidæ. With-

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

[2] Journ. Linn. Soc. Zool., XXIV, 475.

out detailing the very numerous and striking contrivances displayed by nearly all the anterior legs of some Craspedosomatidæ, it is sufficient to point out that in Scytonotus[3] modifications apparently as great as those of Glomeridesmus occur as far back as the twentieth pair of legs. In the light of these facts the degree of modification shown by Glomeridesmus counts for little or nothing as an evidence of relationship with the Oniscomorpha. It might be said that Glomeridesmus has no copulatory legs at all, for the structures figured by Mr. Pocock are probably not comparable with the true copulatory legs of the other Diplopod groups, either in structure or function. The really remarkable thing about Glomeridesmus is that the legs of the seventh segment are not modified. Yet on this account we are not obliged to arrange Glomeridesmus in a separate category, for the degrees of modification to be found in the legs of the seventh segment of the other Diplopod groups are very various. It is even possible to trace, in the second pair of legs of the seventh segment of Craspedosomatidæ all the stages from the nearly normal to the completely modified condition. Thus with reference to the fact that the seventh legs are unmodified, Glomeridesmus may be looked upon as one end of a series, not necessarily farther removed from the other groups than they are from each other. Certainly the distance between the unmodified legs of Glomeridesmus and the distinctly jointed copulatory legs of Polydesmoidea and Polyzonoidea is not greater than that between those of the Polydesmoidea and the Spirostreptoidea.

If, however, we admit that differences in the position and degree of modification of legs transformed for copulatory purposes are not of themselves characters of fundamental importance in the Diplopoda, we may seem to be under the necessity of admitting in addition that the constant appearance of what we may call the true copulatory legs in the place of the anterior or both pairs of the seventh segment is an evidence that the Helminthomorpha of Pocock are a homogeneous group to the extent of having more affinity with each other than with the Oniscomorpha. If, however, facts exist which indicate that the copulatory legs may have had independent origins in any of the Helminthomorpha the evidence just referred to is largely overthrown, for the utter diversity in plan of the copulatory legs of the different orders of Diplopoda is itself a strong indication that they represent independent lines of development. Such seems to be the import of the fact that the legs which in the Merocheta[4] are primarily transformed

[3] Am. N. Y. Acad. Sci., VIII, 233 (1894).

[4] An ordinal name to cover the Polydesmoidea, Craspedosomatoidea and Callipodoidea. Cf. Ann. N. Y. Acad. Sci., Vol. IX, (1895).

into copulatory organs, the anterior pair of the seventh segment, are in the Colobognatha *entirely unmodified.* Hence we must suppose that either the legs or the function have migrated, in case we assume a common origin and attempt to homologize the copulatory legs in the two orders. The theory of migration, however, has no facts to support it, and would be equally fatal to the idea that affinity or the want of it can in the Diplopoda be inferred from the position of the copulatory legs.

The fact that the Colobognatha have eight precopulatory legs is not new, but up to this time the whole eight have been supposed to belong to the first six segments. Both Latzel and Pocock[5] give the distribution of these legs as in the second column. In reality the arrangement is that of the third column.

	Latzel.	*Siphonotus.*
First segment,	First pair,	First pair.
Second segment,	Second pair,	Second pair.
Third segment,	Footless,	Third pair.
Fourth segment,	Third pair, Fourth pair.	Fourth pair.
Fifth segment,	Fifth pair, Sixth pair.	Fifth pair.
Sixth segment,	Seventh pair, Eighth pair,	Sixth pair. Seventh pair.
Seventh segment,	First copulatory, Second copulatory,	Eighth pair. First copulatory.
Eighth segment,	Eleventh pair. Twelfth pair,	Second copulatory. Eleventh pair.

My attention was first attracted to these facts while engaged in examining specimens of Siphonotus collected in Sierra Leone in December, 1893. The creatures were abundant in decaying banana stumps in Freetow, and I secured a large quantity. Instead of curling up as nearly all the representatives the present order are accustomed to do when placed in alcohol, my specimens remained conveniently straight and pliable so that they could be mounted in alcohol or balsam and studied to advantage. Of the arrangement of the legs as here stated there can be no doubt. The drawings are mostly camera tracings made from preparations in balsam. In order to make sure of the condition in Polyzonium, the genus studied by Latzel, I cut animals in two horizontally, brushed away the internal structures and mounted in balsam.

[5] Mr. Pocock seems to have come to doubt this disposition, for he uses a "?" in front of his last statement on the subject.—Max Weber's Reise, p. 335.

Without such a preparation satisfactory observation is very difficult if not impossible in Polyzonium, for all the parts, especially the bases of the legs, are crowded together. I have examined in addition Andro, gnathus, Platydesmus, Pseudodesmus, Siphonorhinus and Siphonophora without finding any indications that the condition described is not present in all, though a final demonstration would in most cases not be easy without dissection.

Probably correlated with the comparatively slight degree of specialization which appears in the copulatory legs of the Colobognatha is the fact that in young males of Siphonotus the copulatory legs are several-jointed before maturity. Such a condition seems to be unknown in the other helminthomorphous groups.

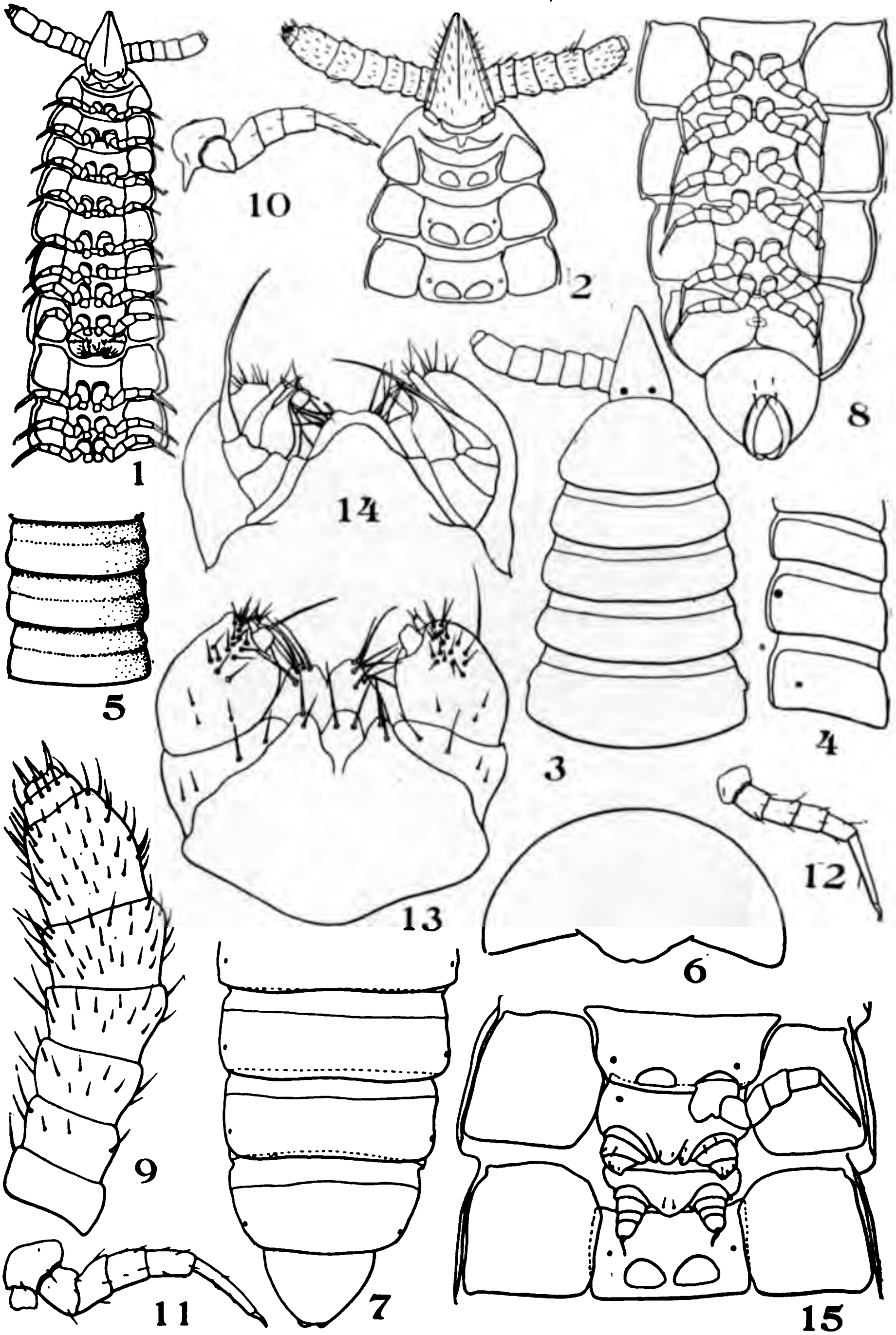
In the previous discussion there has been no intention to imply that the orders Oniscomorpha and Limacomorpha are not valid; the contention is merely that the position of the modified legs does not *of itself* justify holding them as divisions of greater weight than other natural groups of Diplopoda, some of which have been designated by ordinal names. It is to be expected that future study may result in a natural arrangement of the groups now designated as orders, but until their affinities are demonstrated nothing is to be gained by attempting to retain under one ordinal name and description animals which may prove to be widely divergent in their development history. Thus it is by no means impossible that the Colobognatha are really a group farther removed from the other Helminthomorpha than are some of these latter from the Oniscomorpha. Many of the peculiar characters of the Oniscomorpha are evidently the result of their power to roll themselves into a sphere, and are not to be assigned great weight in estimating affinities.

Systematic Note.—The genus Siphonotus has not until very recently been reported since its establishment by Brandt in 1836. Within the last year or two Mr. Pocock has described species from St. Vincent (West Indies), Java and Celebes. To me it seems doubtful whether any of these species are congeneric with Brandt's type *S. brasiliensis.* Provisionally, however, the species of which drawings are presented may be described under Siphonotus, no doubt being possible that its affinities are here rather than with any other genus yet established.

Siphonotus africanus sp. n.

Body slender, the sides parallel to near the ends, or very slightly converging cephalad.

PLATE XVIII.



Siphonotus africanus Cook.

Head smooth and shining, sparsely hirsute distad.

Eyes of a single ocellus on each side, large and strongly pigmented.

Antennæ sparsely hirsute, strongly crassate, scarcely clavate; joints increasing in length from the first to the sixth.

Hypostoma distinct, medianly deeply excavate.

Mentum distinct, on each side a large cardo (?). Other parts of gnathochilarium indistinct; the lines of the figures may represent internal structures only.

First segment longer than the others, which increase in length to the middle of the body and are scarcely shorter caudad; surface of all the segments smooth and shining.

Repugnatorial pores in a continuous series from the fifth segment, removed considerably from the margin of the scuta, except those of the fifth segment, which are also distinctly larger than the others.

Pleuræ large and entirely free, smooth and shining.

Pedigerous laminæ distinct, free; sometimes the edges of the pedigerous laminæ and the pleuræ lie one upon the other.

Penultimate segment without legs, in contact or overlapping, but not closed below.

Anal valves narrow; preanal scale wanting.

Legs sparsely hirsute, all six-jointed, with a rudimentary second joint which would make seven.

Males with a conic process from the coxæ of the second legs. All the other legs except the first have a coxal aperture from which projects a transparent membrane or secretion.

Male genitalia, see figures.

Color of living animals, pinkish, pale to dull reddish-pink.

Length of mature individuals up to 15 mm., width .75 mm.; segments of adults 39–55. The males are distinctly smaller than the females. The young male specimen from which fig. 15 was drawn has 24 segments, all but two of which bear legs, as in the mature animals.—O. F. COOK.

EXPLANATION OF PLATE.

Siphonotus africanus.

Fig. 1. Head and first nine segments, ventral view, to show arrangement of legs.

Fig. 2. Head and first three segments, ventral view, the legs removed to show pedigerous laminæ, spiracles, etc.

Fig. 3. Head and five segments, dorsal view.

Fig. 4. Segments 4–6, lateral view, to show the peculiarly enlarged pore of the fifth segment.

Fig. 5. Three normal segments, dorsal view, less magnified than fig. 3.
Fig. 6. Diagrammatic cross-section, showing relative positions of the parts of the exo-skeleton.
Fig. 7. Last five segments, dorsal view.
Fig. 8. Same, ventral view.
Fig. 9. Antenna, much magnified, ventral view.
Fig. 10. Second leg of male.
Fig. 11. Eleventh leg of male.
Fig. 12. Last leg of male.
Fig. 13. Male copulatory legs, dorsal view.
Fig. 14. Same, ventral view.
Fig. 15. Segments seven and eight of young male.—O. F. Cook.

PSYCHOLOGY.[1]

Congress of Psychologists.—The third Congress of Psychologists was held at Munich, August 4th to 9th, Prof. Stumpf, of Berlin, presiding. It was the largest and in many respects the most successful of the three. Of course the German attendance was fuller than at the last one, held in London in 1892, and German delegates are always most welcome. When we take into account the fact that Germany is to-day the country where psychology is most vigorously and successfully pursued, it follows that this Congress was, up to date, the greatest gathering of eminent psychologists ever seen. As to France, the attendance was disappointing in numbers, although the delegation was very representative; and the same is true of the British contingent. The other countries, except America, were adequately represented; the small attendance from our side of the water being a matter of the more surprise in view of the tendency of our professors to take their vacations abroad—indeed, the attendance at the last Congress in London was considerably larger.

In its general character, the tendency to allow the popular attendance upon the meetings to swamp the scientific proceedings was more marked in Munich, and it is not too much to say that this constituted a very great defect in the arrangements. The membership was over four hundred. There was a constant flow from hall to hall, and the corridors

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

were filled with bewildered persons. Some limit must be put on the popular membership at the next congress, or the scientific people will yield the field to the sightseers and amateurs. The other possible improvement comes to the front again apropos of this meeting in Munich —the crowded condition of the programme. Besides the general meetings, which came in the forenoon sessions, the committee arranged for five sections, all running simultaneously and all subject to constant give and take, as respects their audiences, from one to another. Besides the constant interruptions and great confusion which this produced, it practically prevented a person from hearing many readers whom he especially desired to hear. Since the time limits were not enforced upon the papers or discussions, one could never tell how far on this section or that had progressed, and so could not time his presence for any particular reader. Moreover, the papers were as usual so generally accepted by the committee—anyone who wanted to present something had only to send his name and topic beforehand—that many were read which were of little or no scientific value; and the titles of papers were entered on the programme in advance, so that there was no way to learn infallibly whether a particular reader had arrived and would present his dissertation or not. The gaps left by the absentees were consequently quite an unknown quantity. Every such meeting should have a committee to read and select from available papers, arrange them strictly according to unchangeable time divisions, and require each reader to report finally a day or two before the meeting as to his actual attendance, the final programme being only then printed. This would have the further advantage of ruling out titles and names which are from the first doubtful; for it is astonishing to what an extent men fail to carry out what should be their serious intention when they give their names to be printed on these Congress programmes.

So much for the general character of the Congress. Of course, this is not the place for an account, in any detail, of its scientific features. The division into sections will show something of the remarkable range that modern psychology finds itself obliged to take: "Normal," "Sleep and Hypnotism," "Mental Pathology," "Neurology," "The Senses and Psychophysics"—the titles being somewhat abbreviated in this list. In each of the sections there were some great papers and one or more lively discussions. The most interesting thing in the way of neurological work—it was presented, however, in one of the general meetings—was the paper by the veteran Flechsig on "Association Centres." It will be remembered that Prof. Flechsig has been engaged for some time on comparative studies of the brains of human infants at different ages,

attempting so to arrive at a view of the order of development of the elementary mental functions, with the corresponding progress in brain anatomy and physiology. He has published very rich results from time to time, and among them is his determination of certain so-called "association centres." He thinks that the much discussed frontal region of the brain is the location of associations of a higher and more abstract kind; and that in the region back of the well-known "motor region," extending to the visual centre in the occipital region, is a great centre for the associations which bind the sense functions together. This in brief, and without the discriminations which an accurate account of his views should make. The reason which he gives for these determinations is that only after some growth, and after the senses are well developed, do we find the great masses of connecting fibres which traverse these regions forming in the child's brain.

Apart from the question of fact, as to which Prof. Flechsig's researches may be considered as being of the greatest importance (especially when we consider his method), it is difficult to see how these regions can be, in any true sense, "association centres;" for, admitting that the connections between the sense centres run through these regions, the main thing about the associations must be the things associated, not the mere fact of connections between them. One would hardly call the bunch of telegraph wires on the housetop a "communication centre;" the loci of communication are still at the telegraph offices. Without them, the wires would be possibly even more helpless than the offices without the wires. Prof. Flechsig's paper was a model for imitation in the manner of its presentation, and its interest was enhanced by slides showing the infant's brain, in sections illustrating the periods of its growth.

Another contribution to the understanding of the relation of psychology and brain physiology was that of the well-known neurologist, Prof. Edinger, of Frankfort, on the question, "Can Psychology make use of the results so far attained in Brain Anatomy?" He did not confine himself to anatomy, but presented a series of interesting notices on the development of the nervous system in the scale of life, and made a strong plea for a corresponding genetic study of comparative psychology. Genetic psychology, he says, is so far behind analytic psychology because psychologists have confined their attention, on the anatomical side, to the cerebral hemispheres, while what they should do is to study the evolution of the nervous system all the way up, and see the progress of consciousness with it. "Gerade auf diesem Gebiete müssen anatomisch-physiologische und psychologische Studien durchaus Hand in Hand gehen." All this is true and remarkably opportune, I think,

despite the fact, that in his main illustration Prof. Edinger falls into one of the glaring fallacies into which this sort of analogy between body and mind may lead. He says there are certain creatures (fishes) which have no hemispheres, and it follows that, on the psychological side, we must deny to these creatures "all that the hemispheres are necessary for in the higher creatures." This overlooks the great principle that, in the lower forms, less differentiated structures may do what more differentiated ones do in the higher forms. To press this point consistently, he would seem to have to deny consciousness altogether to these fishes. The lesson of this paper, however, is a most timely one; psychologists, especially in Germany, are not half awake to the genetic problem, and when they do awake, no doubt it is true that the richest lessons that the physiology of the nervous system will have to teach them will be derived from such comparative study as Prof. Edinger advises.

Several papers of general interest were read in the open meetings. The President's address was rather more severe and *wissenschaftlich* than the earlier addresses of the presiding officers have been, but it was an exceedingly interesting and discriminating review of theories on the connection of mind and body. The arraignment of Parallelism was very effective—possibly more so than the positive doctrine of the paper. Prof. Ebbinghaus of Breslau gave a new way of testing the mental condition of school children at different periods and in different conditions of fatigue, etc. It differs from the methods already in vogue in that it endeavors to test the child's correlating or apperceptive faculty rather than his sense-perceptions or his memory. The method, which teachers will find extremely interesting, consists in taking a passage from some interesting narrative-text, and, after striking out various words and phrases and printing the passage with black spaces where these erasures have been made, telling the child to fill in the spaces as he thinks the sense requires. This requirement certainly calls upon the child for more than memory, and the results of its application, as reported by Prof. Ebbinghaus, seem to show its superiority; but it would appear to be applicable to children of a more advanced age, after the memory tests are outgrown. This general judgment, however, I must make with reservation, since the synopsis of the paper did not reach my hands. This may suffice to indicate the scope of the method, and to call the attention of our educational authorities to it. They will also be interested in Prof. Ebbinghaus's severe criticism of what he called the "American method" of testing the mental condition of school children by the memory tests.

The fact that the papers on "Hypnotism" were fewer than in earlier congresses, in proportion to the entire number, and that there were a bare half-dozen on thought-transference and telepathy, shows the general tendency of psychology. The hypnotic period is past, even in France. Not that the gain from the study of hypnotism has not been permanent and great; on the contrary, its results are only now getting so absorbed into the body of psychological truth that it no longer makes sensational appeals for a hearing. As to telepathy, I think there is a real decay of interest in the subject, much as this is to be deplored. The most interesting paper in the hypnotic field was a general one by Prof. Pierre Janet.

The section on the Senses and Psychophysics did much exact work. Dr. Stratton of the University of California communicated some valuable experiments of his own on the artificial reinverting of the retinal image and its effects on the sense of bodily position in space, which will be of especial interest to those who think the normal inversion of the image requires a theory.

Two other general questions of great interest were discussed, with as much ability as vehemence, by the Vice-President of the Congress, Prof. Lipps, of Munich. One of his papers was a very important contribution in the sadly neglected field of the æsthetics of visual form. I can do no more than recommend his paper in the Congress "Proceedings" (to appear very soon) to those who are concerned with elementary æsthetic principles. The other topic was the much-discussed one on the "Unconscious" in psychology. The question, Can mental states be unconscious? has a peculiar fascination, because of the great number of verbal distinctions of which it admits. It must be confessed that Prof. Lipps's paper did not make the number of these verbal distinctions less. He reaches a sort of return to the soul-substance theory—a hidden thing in which mental states, and especially tendencies of an active kind, may be preserved when we are not conscious of them. This has long ago been refuted as a general conception, I think; but the main point of interest, and that for which I bring the matter up, is that the results of pathology, dual consciousness, "multiple personality," etc., which are considered by many as giving the strongest evidence for the "unconscious," require quite a different theory. The "unconscious" of the pathologists is a body of conscious data gathered into a new and secondary consciousness of its own. While these states of mind are not conscious to the major person—and so, by a certain license, are called "unconscious"—still it is just the evidence that they are conscious in their own way and in their own seat in the nervous

system that enables us to say that they are mental. So all this evidence goes, after all, to show a correspondence between the mental and the conscious. This Prof. Lipps does not seem to see, and his treatment of the question from a purely verbal and analytic point of view was consequently very inadequate.

In the higher fields of ethics and anthropology there were interesting papers, of which my space allows the mention of only one on "Ethical Values," by Prof. Ehrenfels (just called from Vienna to Prague), and one on the "Category of Individuality in Savage Thought," by Mr. Stout, the editor of *Mind*. Mr. Stout, I may add, has just been called to a lectureship in comparative psychology in the University of Aberdeen—a novelty for the British Isles, but appropriate in the institution which Prof. Bain has made famous in connection with psychological study. The next Congress is to meet in Paris in 1900 in connection with the Universal Exposition. Prof. Ribot will be President; M. Ch. Richet, Vice-President, and M. Pierre Janet, Secretary. The International Committee for the Paris meeting has the following American representatives: Profs. James (Harvard), Titchener (Cornell), Hall (Clark), and Baldwin (Princeton).

I cannot close this letter without referring—with profound regret, which many other American students of philosophy must also feel—to the death Prof. Avenarius, of Zurich, on August 18. Where I now write, the feeling that one of the greatest philosophical thinkers of Europe no longer adorns a Swiss university is very acute; and those who know the work of Prof. Avenarius must feel it also, regardless of the place of their habitation.—J. MARK BALDWIN, in *New York Evening Post*, Sept. 12.

Mental Action During Sleep, or Sub-Conscious Reasoning.—Shortly after reading the interesting article by Professor Cope with regard to recent ethnological discoveries in Assyria, undertaken under the auspices of the University of Pennsylvania, and elucidated by Professor Hilprecht, I met with the account of a peculiarly curious dream which had been experienced by Professor Hilprecht whilst his mind was deeply occupied with these very investigations.

It is of course well known to all students of mental psychology, that the most complicated, abstruse forms of reasoning have often been carried out in dreams; and many interesting and well authenticated cases of this phenomenon will be found in Dr. Carpenter's Mental Physiology.[1] But the peculiarity of Dr. Hilprecht's dream consists in

[1] Chap. XIII, Unconscious Cerebration. Mental Physiology. W. R. Carpenter, M. D. Chap. XV, Of Sleep, Dreaming and Sonnambulism, pp. 534, 598–5.

the intensely dramatic manner in which the solution of the problem he was engaged on was conveyed to his mind. I will now simply quote from the account given to Prof. William Romaine Newbold, by Professor Hilprecht, in the first place of a train of sub-conscious reasoning during sleep which put him on the track of a satisfactory rendering of an Assyrian proper name; and in the second place of the work carried out under the influence of a strangely dramatic dream.[3]

"During the winter 1882–3, Professor Hilprecht was working with Professor Friedrich Delitzsch, and was preparing to publish as his dissertation, a text, transliteration and translation of a stone of Nebuchadnezzar I, with notes. He accepted at that time the explanation given by Professor Delitzsch of the name Nebuchadnezzar, 'Nabû-kudurru-usur,' 'Nebo protect my mason's pad' or mortar board,' *i. e.*, 'my work as a builder.' One night, after working late, he went to bed about two o'clock in the morning. After a somewhat restless sleep, he awoke with his mind full of the thought that the name should be translated 'Nebo protect my boundary.' He had a dim consciousness of having been working at his table in a dream, but could never recall the details of the process by which he arrived at this conclusion. Reflecting upon it when awake, however, he at once saw that *kudurru*, 'boundary,' could be derived from the verb *kadaru*, to enclose. Shortly afterwards he published this translation in his dissertation, and it has since been universally adopted."

Mr. Newbold then gives a translation from the account written in German by Prof. Hilprecht of his remarkable dream.

"One Saturday evening, about the middle of March, 1893, I had been wearying myself, as I had done so often in the weeks preceding, in the vain attempt to decipher two small fragments of agate which were supposed to belong to the finger rings of some Babylonian. The labor was much increased by the fact that the fragments presented remnants only of characters and lines, that dozens of similar small fragments had been found in the temple of Bel, at Nippur, with which nothing could be done, that in this case furthermore I had never had the originals before me, but only a hasty sketch made by one of the members of the expedition sent by the University of Pennsylvania to Babylonia. I could not say more than that the fragments, taking into consideration the place where they were found and the peculiar characteristics of the cuneiform characters preserved upon them, sprang from the Cassite period of Babylonian history (ca. 1700–1140 B. C.); moreover, as the first character of the third line of the first fragment seemed to be KU, I

[3] Proceedings of the Society for Psychical Research, for June, 1896, pp. 13-17.

ascribed this fragment, with an interrogation point, to King Kurigalzu, whilst I placed the other fragment as unclassifiable, with other Cassite fragments, upon a page of my book where I published the unclassifiable fragments. The proofs already lay before me, but I was far from satisfied. The whole problem passed again through my mind that March evening before I placed my mark of approval under the last correction in the book. About midnight, weary and exhausted, I went to bed, and was soon in deep sleep. Then I dreamed the following remarkable dream. A tall, thin priest of the old pre-Christian Nippur, about forty years of age, and clad in a simple abba, led me to the treasure chamber of the temple on its southeast side. He went with me into a small, low-ceiled room without windows, in which there was a large wooden chest, while scraps of agate and lapis lazuli lay scattered on the floor. Here he addressed me as follows: 'The two fragments which you have published separately upon pages 22 and 26, belong together, are not finger rings, and their history is as follows: King Kurigalzu (ca. 1300 B. C.) once sent to the temple of Bel, among other articles of agate and lapis lazuli, an inscribed votive cylinder of agate. Then we priests suddenly received the command to make for the statue of the god Ninib a pair of earrings of agate. We were in great dismay, as there was no agate as raw material at hand. In order to execute the command there was nothing for us to do but cut the votive cylinder into three parts, thus making three rings, each of which contained a proportion of the original inscription. The first two rings served as earrings for the statue of the god; the two fragments, which have given you so much trouble, are portions of them. If you will put the two together you will have a confirmation of my words. But the third ring you have not yet found in the course of your excavations, and you never will find it.' With this the priest disappeared. I awoke at once, and immediately told my wife the dream, that I might not forget it. Next morning—Sunday—I examined the fragments once more in the light of these disclosures, and to my astonishment found all the details of the dream precisely verified in so far as the means of verification were in my hands. The original inscription on the votive cylinder read: 'To the god Ninib, son of Bel, his lord, has Kurigalzu, pontifex of Bel, presented this.'

"The problem was thus at last solved. I stated in the preface that I had unfortunately discovered too late that the two fragments belonged together; made the corresponding changes in the 'Table of Contents,' pp. 50 and 52; and it being not possible to transpose the fragments, as the plates were already made, I put in each plate a brief reference to the other. [Cf. Hilprecht, "The Babylonian Expedition of the Uni-

versity of Pennsylvania," Series A, Cuneiform Texts, Vol I, Part 1, "Old Babylonian Inscriptions, chiefly from Nippur.] H. V. Hilprecht."

Upon the priest's statement that the fragments were those of a votive cylinder, Professor Hilprecht makes the following comment:

"There are not many of these votive cylinders. I had seen, all told, up to that evening, not more than two. They very much resemble the so-called seal cylinders, but usually have no pictorial representation on them, and the inscription is not reversed, not being intended for use in sealing, but is written as it is read."

Then there follows a transliteration of the inscription in the Sumerian language. Mrs. Hilprecht's statement is as follows:

"I was awakened from sleep by a sigh, immediately thereafter heard a spring from the bed, and at the same moment saw Professor Hilprecht hurrying into his study. Thence came the cry, 'It is so, it is so!' Grasping the situation, I followed him and satisfied myself in the midnight hour as to the outcome of his most interesting dream."

Signed, "J. C. Hilprecht."

A few weeks after the occurrence of this curious dream, there appeared a difficulty which Professor Hilprecht was not able to explain. "According to the memoranda in our possession, the fragments were of different colors, and, therefore, could have scarcely belonged to the same object. The original fragments were in Constantinople, and it was with no little interest that I [Mr. Newbold] awaited Prof. Hilprecht's return from the trip which he made thither in the summer of 1893. I translate again his own account of what he then ascertained.

"In August, 1893, I was sent by the Committee on the Babylonian Expedition to Constantinople, to catalogue and study the objects got from Nippur, and preserved there in the Imperial Museum. It was to me a matter of the greatest interest to see for myself the objects which, according to my dream, belonged together, in order to satisfy myself that they had both originally been parts of the same votive cylinder. Halil Bey, the director of the museum, to whom I told my dream, and of whom I asked permission to see the objects, was so interested in the matter that he at once opened all the cases of the Babylonian section, and requested me to search. Father Scheil, an Assyriologist from Paris, who had examined and arranged the articles excavated by us, before me, had not recognized the fact that these fragments belonged together, and consequently I found one fragment in one case and the other in a case far away from it. As soon as I found the fragments and put them together, the truth of the dream was demonstrated *ad oculos*—they had, in fact, once belonged to one and the same votive cylinder.

As it had been originally of finely veined agate, the stone-cutter's saw had accidentally divided the object in such a way that the whitish vein of the stone appeared only upon the one fragment, and the larger gray surface upon the other. Thus I was able to explain Dr. Peters' discordant descriptions of the two fragments."

There are, says Mr. Newbold, two especial points of interest in this case, the character of the information conveyed, and the dramatic form in which it was put. The apparently novel points of information given were:

1. That the fragments belonged together.
2. That they were fragments of a votive cylinder.
3. That the cylinder was presented by King Kurigalzu.
4. That it was dedicated to Ninib.
5. That it had been made into a pair of earrings.
6. That the "treasure chamber" was located on the southeast side of the temple.

We have a *point de repère* for the treasure chamber part of the dream, in the fact, that Dr. Peters, as far back as 1891, had told Professor Hilprecht of the discovery of a room in which were remnants of a wooden box, while the floor was strewn with fragments of agate and lapis lazuli.[4] The other points in the dream may be accounted for by the direction in which Professor Hilprecht's thoughts had been travelling, or they may not; I must confess to thinking they cannot all be so accounted for.—ALICE BODINGTON.

NOTE.—I would advise anyone interested in the subject of subconscious reasoning in dreams, to read at length the account given by William A. Lamberton, Professor of Greek in the University of Pennsylvania, of a dream in which he solved *geometrically* a difficult problem which he had attacked from its algebraic and analytic side. The *point de repère* here seems to have been a blackboard which had formerly had a functional use in the room, but which had been painted over, the black still showing through the white paint. Professor Lamberton, on opening his eyes one morning, about a week after he had determined to banish this insolvable problem from his mind, saw upon this blackboard surface a complete figure, containing not only the lines given by the problem, but also a number of auxiliary lines, and just such lines as without further thought solved the problem at once.

"I sprang from bed," says Prof. Lamberton, "and drew the figure on paper; needless to say, perhaps, that the geometrical solution being

[4] Two curious cases of the dramatic form taken occasionally by dreams will be found on p. 18, Proceedings S. P. R. for June, 1896.

thus given, only a few minutes were needed to get the analytical one." (Sub-Conscious Reasoning, Proc. S. P. R. pp. 11–13).—A. B.

"The Mimetic Origin and Development of Bird-language," and "The Evolution of Bird-song."—When one considers how many people are thinking at the same time, it does not seem strange that two persons, though widely separated and totally unknown to each other, should sometimes think not only of the same subjects but also follow in the same direction and practically at the same time, the same lines of original thought and investigation. Some of these duplicated ideas are of value in commerce; others are mere metaphysical speculations, possibly suggested by the same incidents; but at this late stage in the knowledge of natural history it does appear unusual that two people in different hemispheres and observing totally different species of animals snould have simultaneously pursued independently the same far-reaching but novel line of speculative thought.

A few days ago, Mr. J. E. Harting called my attention to an article in THE AMERICAN NATURALIST, entitled "The Mimetic Origin and Development of Bird-language," which appeared in that journal for March, 1889. He did so because I had lately written a book on the subject, *The Evolution of Bird-song* (London, A. & C. Black, May, 1896), and because the article in question discussed some of my themes. I have just finished a perusal of the article, which was indeed rather exciting, since in nearly every paragraph I found an anticipation of some theory or observation of my own, which I had theretofore believed to be original. In fact, any one reading the article and afterwards reading *The Evolution of Bird-song*, would think that I had borrowed without acknowledgment a good many ideas thrown out by Mr. Samuel N. Rhoads, the writer of the essay in question. However, I am able to prove that in 1888 I had already made investigations on exactly the same lines as Mr. Rhoads, and had recorded the results of them in writing. In the summer of 1887 I began to make systematic records of the imitations I heard from imitative wild British birds, and in the course of this study various themes were attacked, such as "the influence of combat," "the influence of the love-call," "family-voices," and "the influence of imitation," etc. I wrote essays on these themes and sent them to the late Professor Harker, F. L. S. and some of them to Mr. S. S. Buckman (now an eminent geologist), with whom I had many conversations on the subjects in question. In 1890 these observations appeared, in a highly condensed form, in *The Zoologist*, in two papers entitled "The evolution of bird-song," and published respectively in

July and August of that year. I had then of course no idea that my seemingly daring suggestions that mimicry had attuned the cries of birds to their environment, had been confirmed or anticipated in America by a writer who, whatever may be the value of his deductions, is obviously an acute observer. My papers in *The Zoologist* were severely handled last year by a writer who certainly had never heard of Mr. Rhoads' article; and this year the same writer, in favorably criticising in *Nature* my new book, employed a few congratulatory words upon my having allowed certain of my former conclusions to drop into the background.

Although Mr. Rhoads and I were working on the same track, he will I am sure allow that I have gone into the subjects in much greater detail than would be possible within the limits of an article, unless it occupied the whole of the magazine in which it appeared. Mr. Rhoads traces the origin of certain tones to noises produced by the elements, such as the bubbling of air through mud, the murmurs of streams, the sibilant sounds caused by branches being rubbed against each other by the wind, the cries of the victims of predacious birds, the croakings of amphibians, and the moaning of wind in hollow trees; and I adopted the same position, though quoting different instances, in relation to each of these features. He touches on heredity, in a way that would suggest the working-out of the theme in much the same way as I have attempted to do it. In his suggestion of the original use of the voice in "hissing or choking sounds," and in my surmise that the voice was "evolved from a toneless puffing indicative of anger, or from snorts or grunts accidentally caused," in support of which idea some pertinent evidence may be adduced, we have both, I think, advanced somewhat from Darwin's proposition that involuntary and purposeless contractions of the muscles of the chest and glottis, due to excitement of the sensorium, first gave rise to the emission of vocal sounds (*Expression of the Emotions*, pp. 83, 84).

In order to show, however, that I have not been limited to only those themes which Mr. Rhoads treats in so terse and yet so attractive a style, I would mention merely the "contents" of one of my chapters, in which some side-issues are dealt with, as follows:

"Songs are generally uttered by males: exceptions—Not until birds have attained full size: exceptions—Most frequently at morning and evening: influence of weather—Tendency to rise in pitch with vehemence—Only small birds properly sing—Singers arboreal birds generally—Effect of living amid foliage: on size, hearing, and voice—Accent in songs—Singers clad in sober hues—Devel-

opment of the eyes in detecting danger—Necessity of leisure—Labours of parent-birds—Laborious and stealthy birds habitually poor in song—Flight in song: for purposes of display—Fluttering of wings a means of address—Ventriloquism—Singing in chorus."

The study of bird-song, on the lines indicated by Mr. Rhoads and myself, is so new and so delightful a pursuit that I hasten to ask readers of THE AMERICAN NATURALIST to peruse his paper once more; for it is in America, so richly blessed with birds, that such investigations can be most easily developed, and that they promise the most accurate and helpful results.—CHARLES A. WITCHELL.

A Note on Dr. Herbert Nichols' Paper (Amer. Nat., Sept., 1896).—Readers of Dr. Herbert Nichols' paper on my article entitled "A New Factor in Evolution" will understand that its intemperate spirit should rule out all reply. I may say, however, that Dr. Nichol's "home thrusts" are all directed at my view of pleasure and pain, which he considers, quite mistakenly, the point of my paper. On the contrary the "factor" is entirely the influence of the individual's adaptations on the course of evolution; not at all the particular way in which the individual makes its adaptations. I took pains to reiterate this distinction in the paper, saying (AMER. NAT., 1896, p. 542–3) "So far we have been dealing exclusively with facts . . . . . without prejudicing the statement of fact at all, we may inquire into the actual working of the organism in making its adaptations. . . . . . Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in (race) development, is not involved in the solution of the farther question as to how the adaptations are secured." So I see absolutely no point in Dr. Nichols' criticisms.

The other question, which involves pleasure and pain, is discussed in the latter part of my paper; but it is not that, but my book which Dr. Nichols attacks with the grossest misunderstanding. I do not at all believe the main things which he attributes to me; first, the position that there are no pain nerves, and second, that there is a "psychic factor" which is an "efficient cause" in evolution. Psychologists know Dr. Nichols' hobby and allow for his intemperateness.

J. MARK BALDWIN.

Princeton, Sept. 25, 1896.

MICROSCOPY.[1]

Methylen Blue.—A few points observed in the use of Erlich's methylen blue method by the investigators in the Marine Biological Laboratory at Woods Holl, Mass. may be of general interest.

This method has been successfully applied during the past summer to the study of the nervous system in a great variety of forms including vertebrates, crustacea, annelids, echinoderms and tunicates.

Ehrlich's *intra vitam* methylen blue prepared by Grübler was used for staining the nerve tissues. The stain was applied by injecting a 1–½ per cent solution of the methylen blue made in normal salt solution, into the blood vessels, body cavity or lymph spaces or by immersing small animals or excised pieces of nerve tissue in a weak solution.

The method of application and strength of the solution were determined by experiment for each animal and tissue. During the action of the stain, the animal or tissue was kept as nearly as possible in its normal condition. Everything seems to depend on keeping the tissue alive, and in bringing the stain in contact with it in a solution of a strength suitable for obtaining the best results.

The abundant supply of oxygen to the staining tissue was of great importance in some cases, while in others it seemed to make little difference.

It was found, as suggested by Dr. C. Huber, that animals which live in the dark, stain better in the dark than in the light.

The relaxation of the tissues by the use of chloroform or chloral hydrate seemed to be more favorable for the staining of some elements of the nervous system, while others did not stain which stained in the unchloroformed animal.

It was found that recently caught and perfectly normal animals stained more satisfactorily than those which had been kept in confinement for some time, unless under very favorable conditions.

In the case of the dogfish, active animals were killed by decapitation. The stain was applied by injecting a 1–½ per cent solution of the methylen blue into the blood vessels for the central nervous system and by immersing small pieces of nerve tissue in a weak solution of the stain for the sense organs.

The length of time required for the *intra vitam* staining varied widely, annelids requiring 4–5 hours, while dogfish only require 1–½ hours, either by injection or by immersing the tissue in the stain.

[1]Edited by C. O. Whitman, University of Chicago.

When small transparent pieces of tissue were to be examined, they were fixed in a saturated solution of picrate of ammonia in distilled water from 2–4 hours and were then mounted in a mixture of equal parts of pure glycerine and distilled water to which a small quantity of picrate of ammonia is added. When opaque or large pieces were fixed in this way they were sectioned by the freezing method. After fixing in the picrate of ammonia, the tissue was placed in a saturated solution of sugar for one hour and was then transferred to a piece of blotting paper to remove the syrup from its surface. It was then placed in a thick solution of gum arabic for fifteen minutes and then transferred to the plate of the freezing microtome where it was frozen by means of liquid carbonic acid. The sections were mounted in dilute glycerine as in the other case. The principal advantage of this method is its rapidity, but neither serial sections nor those of equal thickness can be obtained.

In order to obtain serial sections by the paraffine method, the tissues were fixed in Berthe's Fluid.[2]

For vertebrates :	For invertebrates :
Molybdate of ammonia, 1 gram.	Molybdate of ammonia, 1 gram.
Distilled water, 10 c. c.	Distilled water, 10 c. c.
Hydrochloric acid, 1 drop.	Peroxide of Hydrogen, ½ c. c.
Peroxide of Hydrogen, 1 c. c.	

A different formula is used for tissues of invertebrates as less oxygen is required than for vertebrates. The fixing fluid must be cooled on ice before placing the tissue in it. After remaining in the cold fixing fluid for from 2–4 hours the tissue is thoroughly washed with cold water which generally takes about two hours although it has been continued for twelve hours without injury.

It is necessary to remove all the molybdate of ammonia by thorough washing if permanent preparations are to be secured.

The tissue is then passed rapidly, ten to fifteen minutes in each, through the ordinary grades of alcohol to absolute, all being kept cold with ice. The tissue should be left in the absolute alcohol for about two hours at a freezing temperature and the alcohol be changed several times. The stain is dissolved by dilute alcohol at ordinary temperatures.

Dr. Huber's plan of placing the tissue directly in cold absolute alcohol on removing it from the water and changing several times for a period of two hours, gave good results.

[2] Archiv. f. Mikros., Anat. Bd. 44, Heft 4.

After thorough dehydration the tissue is placed in xylol for 12–24 hours and changed several times. It is then imbedded in paraffine in the usual way.

The most complete and in every way satisfactory staining of the sensory nervous system was obtained by two or three injections of a ½ per cent solution of Erlich's methylen blue at intervals of from 15 to 20 minutes, both with vertebrates and invertebrates, as suggested by Semi Meyer.[3]

The tissues relaxed after the first injection so that more fluid was introduced by the second and third injections than by the first.

The use of chloroform was found to be wholly unnecessary by this method. Meyer uses a very strong solution of B. X. methylen blue, 5 per cent to 6 per cent, in water.

The paraffine sections should generally be quite thick (45–60 $M\mu$).—A. D. MORRILL, Hamilton College, Clinton, N. Y.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Association for the Advancement of Sciences (continued from page 779).—Prof. A. S. Packard was elected to represent the Association on the American advisory board to coöperate with the American member of the international commission on rules of nomenclature. The following were elected to represent the Association at the International Congress of Geologists to be held in St. Petersburg in September, 1897: Prof. Jas. Hall, Prof. E. D. Cope, Prof. B. K. Emerson, Prof. C. D. Walcott, Prof. W. N. Rice.

The following sectional officers were elected to serve at the meeting of 1897, at Detroit.

Vice-Presidents.—A, Mathematics and Astronomy.—W. W. Beman, of Ann Arbor, Mich. B, Physics.—Carl Barus, of Providence, R. I. C, Chemistry.—W. P. Mason, of Troy, N. Y. D, Mechanical Science and Engineering.—John Galbraith, of Toronto, Can. E, Geology and Geography.—I. C. White, of Morgantown, W. Va. F, Zoology.—G. Brown Goode, of Washington, D. C. G, Botany.—George F. Atkinson, of Ithaca, N. Y. H, Anthropology.—W. J. McGee, of Washington, D. C. I, Social and Economic Science.—Richard T. Colburn, of Elizabeth, N. J.

[3] Archiv. f. Mikros. Anat. Bd. 46. Heft 2, and Bd. 47. Heft 4.

Permanent Secretary.—F. W. Putnam, of Cambridge, Mass. (Office, Salem, Mass.).

General Secretary.—Asaph Hall, Jr., of Ann Arbor, Mich.

Secretary of the Council.—D. S. Kellicott, of Columbus, O.

Secretaries of the Sections.—A, Mathematics and Astronomy.—James McMahon, of Ithaca, N. Y. B, Physics.—Frederick Bedell, of Ithaca, N. Y. C, Chemistry.—P. C. Freer, of Ann Arbor, Mich. D, Mechanical Science and Engineering.—John J. Flather, of Lafayette, Ind. E, Geology and Geography.—C. H. Smith, Jr., of Clinton, N. Y. F, Zoology.—C. C. Nutting, of Iowa City, Ia. G, Botany.—F. C. Newcombe, of Ann Arbor, Mich. H, Anthropology.—Harlan I. Smith, of New York. I, Social and Economic Science.—Archibald Blue, of Toronto, Ont.

Treasurer.—R. S. Woodward, of New York, N. Y.

It was ordered that in future the Vice-Presidents should be called Chairmen of the Sections in the publications of the Association.

Two evening lectures were given to the citizens of Buffalo, viz., The History of the Niagara Gorge, by Dr. J. W. Spencer, and the Results of Cave Exploration and its relation to the Antiquity of Man in North America, by E. D. Cope.

The following papers in the natural sciences were read:

Geology.—*Tuesday, August 25*.—Notes on the Artesian Well sunk at Key West, Florida, in 1895, by Edmund Otis Hovey; The Hydraulic Gradient of the Main Artesian Basin of the Northwest, by J. E. Todd; The true Tuff-beds of the Trias, and the mud enclosures, the underrolling, and the basic pitchstone of the Triassic Traps, by B. K. Emerson; Volcanic Ash from the North Shore of Lake Superior, by N. H. Winchell and U. S. Grant; The Tyringham (Mass.) "Mortise Rock," and pseudomorphs of Quartz after Albite, by Ben. K. Emerson; The Succession of the Fossil Faunas in the Hamilton group at Eighteen Mile Creek, N. Y., by Amadeus W. Grabau; Development of the Physiography of California. Lantern pictures, by James Perrin Smith; Synopsis of California Stratigraphy, by James Perrin Smith; Ancient and Modern Sharks and the Evolution of the Class, by E. W. Claypole.

Wednesday, August 26.—The Discovery of a new Fish Fauna, from the Devonian Rocks of Western New York, by F. K. Mixer; The Cuyahoga Preglacial Gorge in Cleveland, Ohio, by Warren Upham; The Operations of the Geological Survey of the State of New York, by James Hall; A Revision of the Moraines of Minnesota, by J. E. Todd; Origin of the High Terrace Deposits of the Monongahela

River, by I. C. White; Observations on the Dorsal Shields in the Dinichthyids, by Charles R. Eastman.

Zoology.—*Tuesday, August 25.*—On the Entomological Results of the Exploration of the British West India Islands by the British Association for the Advancement of Science, by L. O. Howard; Warning Colors, Protective Coloration and Protective Mimicry, by F. M. Webster; On Life Zones in West Virginia, by A. D. Hopkins; On the Variations of Certain Species of North American Odonata, by D. S. Kellicott; A Case of Excessive Parasitism, by L. O. Howard; Notes on the Occurrence of Dragonflies in Ohio in 1896, by D. S. Kellicott; Scyllarus and Anemonia—A Case of Semi-commensalism, by Edward L. Rice.

Botany.—*Tuesday, August 25.*—The Relation of the Growth of Leaves to the CO_2 of the Air, by D. T. MacDougal; Directive Forces Operative in Leaf Rosettes, by R. N. Day; On Crataegus coccinea and Its Segregates, by N. L. Britton; The Distribution of the Species of Gymnosporangium in the South, by L. M. Underwood and F. S. Earle; Morphology of the Canna Flower, by L. H. Bailey; A Comparison of the Flora of Erie Co., Ohio, with that of Erie Co., New York, by E. L. Moseley; The Point of Divergence of Monocotyledons and Dicotyledons, by C. E. Bessey; On the Bacterial Flora of Cheddar Cheese, by H. L. Russell; The Terminology of Reproductive Organs, by C. R. Barnes; A Comparative Study of the Development of Some Anthracnoses in Artificial Cultures, by Bertha Stoneman.

Anthropology.—*Tuesday, August 25.*—Resolution on the death of Capt. Bourke, Secretary of the Section, followed by a memorial by Washington Matthews.

A Ceremonial Flint Implement and Its Use among the Ancient Tribes of Tennessee, by Gates P. Thurston; Symbolic Rocks of Byfield and Newbury, Mass., by Horace C. Hovey; An Analysis of the Decoration upon Pottery from the Mississippi Valley, by C. C. Willoughby; Brief Description of the Prehistoric Ruins of Tzac Pokoma, Guatemala, by John Rice Chandler; Human Relics from the Drift of Ohio, by W. Claypole; Fresh Geological Evidence of Glacial Man at Trenton, New Jersey, by G. Frederick Wright.

Sociology.—*Tuesday, August 25.*—The Monetary Standard, by W. H. Hale; The Competition of the Sexes and Its Results, by Lawrence Irwell; Fashion—A Study, by S. Edward Warren; Citizenship, Its Privileges and Duties, by Stillman F. Keeland; An Inheritance for the Waifs, by C. F. Taylor.

Wednesday, August, 26—The Proposed Sociological Institution, by James A. Skilton; What is True Money? by Edward Atkinson; The Value of Social Settlement, by Aaron B. Keeler; The Wages Fund Theory, by Aaron B, Keeler.

Thursday, August 27.—Better Distribution of Forecasts, by John A. Miller; The Tin Plate Experiment, by A. P. Winston; Relics of Ancient Barbarism, by S. F. Kneeland; Suicide Legislation, by W. Lane O'Neill.

Geology.—*Wednesday, August 26.*—Wednesday afternoon was given to a meeting in commemoration of the sixtieth anniversary of of Professor Hall's work in connection with the New York Survey.

The presiding officer spoke briefly in behalf of the Association; and Professor Joseph LeConte, President of the Geological Society, spoke for that Society.

Professor Hall was present and responded in person.

James Hall, Founder of American Stratigraphic Geology, by W. J. McGee; Professor Hall and the Survey of the Fourth District, by John M. Clarke.

Thursday, August 27.—Sheetflood Erosion, by W. J. McGee; Post-Cretaceous Grade-Plains in Southern New England. By F. P. Gulliver.

Thursday Afternoon.—Excursion to Eighteen Mile Creek.

Saturday, August 29.—Excursions.

Zoology.—*Wednesday, August 26.*—Notes Upon Cordylophora, by C. W. Hargitt; Modification of the Brain During Growth, by Susanna Phelps Gage; Structure and Morphology of the Oblongata of Fishes, by B. F. Kingsbury; Differentiation of Work in Zoology—in Secondary Schools, by William Orr, Jr.; Field Work and Its Utility, by James G. Needham; Appendages of an Insect Embryo, by Agnes M. Claypole; Experiments Upon Regeneration and Heteromorphosis, by C. W. Hargitt; The Penial Structures of the Saurians, by E. D. Cope; A Note on the Membranous Roof of the Prosencephal and Diencephal of Ganoids, by B. F. Kingsbury.

Anthropology.—*Wednesday, August 26.*—Indian Wampun Records, by Horatio Hale; Seri Stone Art, by W. J. McGee; The Beginning of Zoöculture, by W. J. McGee; Resolution upon the appointment of a Committee to report on "The Ethnography of the White Race in the United States," by Daniel G. Brinton; Onondaga Games, by W. M. Beauchamp; Meaning of the Name Manhattan, by William Wallace Tooker; Kootenay Indian Place Names, by A. F. Chamberlain; Kootenay Indian Names of Implements and Instruments, by A. F. Chamberlain; Aboriginal Occupation of New York, by W. M. Beauchamp;

Clan System of the Pueblos, by F. W. Hodge; The Psychic Source of Myths, by Daniel G. Brinton; The Limitation of the Comparative Method in Anthropology, by Franz Boas.

Sociology.—*Wednesday, August 26.*—The Proposed Sociological Institution, by James A. Skilton; The Value of Social Settlement, by Aaron B. Keeler; Human Reciprocity, by Mary J. Eastman; Better Distribution of Forecasts, by John A. Miller; The Tin Plate Experiment, by A. P. Winston; Relics of Ancient Barbarism, by S. F. Kneeland; Suicide Legislation, by W. Lane O'Neill; Crime Against Labor, by Edward Atkinson.

Geology.—*Thursday, August 27.*—Notes on Certain Fossil Plants from the Carboniferous of Iowa, by Thomas H. Macbride; The Making of Mammoth Cave, by Horace C. Hovey; The Colossal Cavern, by Horace C. Hovey; Sheetflood Erosion, by W. J. McGee; Glacial Flood Deposits in the Chenango Valley, by Albert P. Brigham; Origin of Conglomerates, by T. C. Hopkins; Origin of the Topographic Features in North Carolina, by Collier Cobb; The Cretaceous Clay Marl Exposure at Cliffwood, N. J., by Arthur Hollick; Post-Cretaceous Grade-Plains in Southern New England, by F. P. Gulliver; The Eocene Stages of Georgia, by Gilbert D. Harris; The Origin and Age of the Gypsum Deposits of Kansas, by G. P. Grimsley; Geomorphic Notes on Norway, by J. W. Spencer; The Slopes of the Drowned Antillean Valleys, by J. W. Spencer; The "Augen-Gneiss," Pegmatite Veins, and Diorite Dikes at Bedford, Westchester Co., N. Y., by Lea McI. Luquer and Heinrich Ries; Notes on Kansan Drift in Pennsylvania, by E. H. Williams; Preliminary Notes on the Columbian Deposits of the Susquehanna, by H. B. Bashore.

Thursday Afternoon.—Excursion to Eighteen Mile Creek.

Zoology.—*Thursday, August 27.*—The Peritoneal Epithelium in Amphibia, by Isabella M. Green; Presented by Simon H. Gage; The Heart of the Lungless Salamanders of Cayuga Lake, by Grant S. Hopkins; Observations on the Chameleon, *Anolis principalis*, by Geo. V. Reichel; Energy in Animal Nutrition; Relative efficiency of Animals as machines, by Manly Miles; Some Abnormal Chick Embryos, by C. W. Hargitt; On a peculiar Fusion of the Gill-filaments in certain Lamellibranchs, by Edward L. Rice; The Relationships of the North American Fauna, by Theodore Gill.

Botany.—*Wednesday, August 26.*—The development of the vascular elements in Indian Corn, by W. W. Rowlee; Some remarks on chalazogamy, by J. M. Coulter; The habits of the rarer ferns of Alabama, by L. M. Underwood; On the stem anatomy of certain Onagraceæ, by

Francis Ramaley; The point of divergence of Monocotyledons and Dicotyledons, by C. E. Bessey; Notes on the pine inhabiting species of Peridermium, by L. M. Underwood and F. S. Earle; Reaction of leaves to continual rain fall, by D. T. MacDougal; Studies in nuclear phenomena, and the development of the ascospores in certain Pyrenomycetes, by Mary A. Nichols; The stigma and pollen of Arisæma, by W. W. Rowlee; Notes on the genus Amelanchier, by N. L. Britton; Remarks on the northern species of Vitis, by L. H. Bailey; On the formation and distribution of abnormal resin ducts in Conifers, by Alex. P. Anderson; The development of the cystocarp of Griffithsia bornetiana, by Arma A. Smith; Notes on the allies of the sessile Trillium, by L. M. Underwood; On an apparently undescribed Cassia from Mississippi, by C. L. Pollard; A bacterial disease of the squash bug (*Anasa tritis*), by B. M. Duggar; What is the bark? by C. R. Barnes; Embryo-sac structures, by J. M. Coulter; Some Cyperaceæ new to North America, with remarks on other species, by N. L. Britton; Grasses of Iowa, by L. H. Pammel; Ceres-Pulver: Jensen's new fungicide for the treatment of smut, by W. A. Kellerman; On the Cardamines of the *C. hirsuta* group, by N. L. Britton; The relation between the genera, Polygonella and Thysanella, as shown by a hitherto unobserved character, by John K. Small; An apparently undescribed species of Prunus from Connecticut, by John K. Small; The flora of the summits of King's Mountain and Crowder's Mountain, North Carolina, by John K. Small; Parthenogenesis in *Thalictrum fendleri* by David F. Day; Notes on the family Pezizaceæ of Schröter, by Elias J. Durand; What should constitute a type specimen, by S. M. Tracy; Rheotropism and the relation of response to stimulus, by F. C. Newcombe; Some adaptation of shore plants to respiration, by Hermann von Schrenk; Influence of rainfall upon leaf forms, by D. T. MacDougal; The mechanism of curvature in tendrils, by D. T. MacDougal.

Thursday, August 27.—The Beginning of Zoöculture, by W. J. McGee; Onondaga Games, by W. M. Beauchamp; Meaning of the Name Manhattam, by William Wallace Tooker; Kootenay Indian Place Names, by A. F. Chamberlain; Kootenay Indian Names of Implements and Instruments, by A. F. Chamberlain; Physical and Mental Measurements of Students of Columbia University, by J. McKee Cattell; Anthropometry of the Shoshone Indians, by Franz Boas; Recent Discoveries and Discussions as to Pygmy Races, by R. G. Haliburton; The Papago Time Concept, by W. J. McGee; Notes on the Theological Development of one Child, by Fanny D. Bergen; Certain

Shamanistic Ceremonies among the Ojibways, by Harlan I. Smith; Notes on Certain Beliefs concerning Will Power among the Siouan Tribes, by Alice C. Fletcher; Pueblo Indian Clans, by F. W. Hodge; Mescal Plant and Rite, by James Mooney.

Geology.—*Friday, August 28.*—Glacial Flood Deposits in the Chenango Valley, by Albert P. Brigham; Origin of Conglomerates, by T. C. Hopkins; Origin of Topographic Features in North Carolina, by Collier Cobb; The Cretaceous Clay Marl Exposure at Cliffwood, N. J., by Arthur Hollick; Post-Cretaceous Grade-Plains in Southern New England, by F. P. Gulliver; The Niagara Falls Gorge, by George W. Holley; The Algonquin River, by G. K. Gilbert; The Whirlpool Saint Davids Channel, by G. K. Gilbert; Profile of the Bed of the Niagara in its Gorge, by G. K. Gilbert; Origin and Age of the Laurentian Lakes and of Niagara Falls, by Warren Upham; Correlation of Warren Beaches with Moraines and Outlets in Southeastern Michigan, by F. B. Taylor: Notes on the Glacial Succession in Eastern Michigan, by F. B. Taylor; The Eocene Stages of Georgia, by Gilbert D. Harris; The Origin and Age of the Gypsum Deposits of Kansas, by G. P. Grimsley; Geomorphic Notes on Norway, by J. W. Spencer; The Slopes of the Drowned Antillean Valleys, by J. W. Spencer; The "Augen-Gneiss," Pegmatite Veins, and Diorite Dikes at Bedford, Westchester Co., N. Y., by Lea McI. Luquer and Heinrich Ries; Notes on Kansan Drift in Pennsylvania, by E. H. Williams; Preliminary Notes on the Columbian Deposits of the Susquehanna, by H. B. Bashore.

Anthropology.—*Friday, August 28.*—Finger Prints of American Indians, by Fred. Starr; The Temple of Tepoztlan, Mexico, by M. H. Saville; The Preservation of Local Archæological Evidence, by Harlan I. Smith; Character and Food, by George V. Reichel; Some Indian Rock and Body Painting in Southern California, by David P. Barrows; Recent Explorations in Honduras by the Peabody Museum, by F. W. Putnam; Pueblo Indian Clans, by F. W. Hodge; Mescal Plant and Rite, James Mooney; Finland Vapor Baths, by H. W. Smith; Cupped Stones, by Franz Boas.

Saturday, August 29.—An excursion to Niagara City, Falls and Gorge was given by the citizens of Buffalo. The party took steamboat to a point on the Canadian side above the Falls, and then took trolley down the river. Fine views of the Falls were had. At Lewiston the river was crossed in a steamboat, and the trolley line up the river carried the party past the rapids and the whirlpool, of which magnificent views were obtained. After dinner at the International Hotel, the establishments of the Power Company and of the Carborundum Company were examined, and the company returned to Buffalo.

SCIENTIFIC NEWS.

Dr. G. Brown Goode, Assistant Secretary of the Smithsonian Institution and Director of the National Museum, died in Washington, Sept. 6th.

Dr. Goode was born in New Albany, Ind., February 13, 1851. As a boy he developed a fondness for natural history. In time he prepared for college and entered Wesleyan University, Middletown, Conn., where he graduated in 1870. During his college career he devoted himself assiduously to the study of natural history, to which his tastes inclined, rather than to classical studies. He took an active part in assembling and arranging collections that culminated in the museum now preserved in the Orange Judd Hall. It was while so engaged that he met Prof. Baird at Eastport, Me., and at once became associated as a volunteer in the work of the United States Fish Commission. The acquaintance thus formed with Prof. Baird continued until the death of Mr. Baird and had very much to do with Dr. Goode's subsequent career.

During the winter of 1872–'73 he made his first trip to the West Indies, and collected fish, which he exchanged with the Smithsonian Institution, and on his return, at the invitation of Prof. Baird, he devoted a part of his time to the institution. In 1873 he joined permanently the staff of the Smithsonian Institution, and has since continued in its service, becoming Assistant Secretary in 1887.

Meanwhile, he took an active part in the preparation of the exhibit of the Smithsonian Institution at the Centennial Exhibition at Philadelphia in 1876, and owing to the illness of Prof. Baird, the charge of that work devolved largely on him during the continuance of the fair. At the end of the exhibition the care of the collections that were then given to the United States government was mainly assigned to him, and to him more than any one else is due the present condition of the National Museum.

His volunteer connection with the United States Fish Commission meanwhile continued, and it was through his relation there that he acquired the well earned reputation of being the leading authority on the fishes and fisheries in the United States. It was this that led, in 1880, to his appointment to the charge of the fisheries division of the tenth census. On the death of Prof. Baird, although the entire care of the Museum fell upon him, Dr. Goode was made Fish Commissioner,

and continued in that place, which position he retained only until the the law could be amended, making that office an independent one. His relation to the Fish Commission led naturally to his serving as United States Commissioner to the Fisheries Exhibition in Berlin in 1880, and in London in 1883.

His experience gained at the Centennial Exhibition in 1876 resulted in his being placed at the head of the Smithsonian Institution and National Museum exhibits at the expositions in New Orleans, Cincinnati, Louisville, and Atlanta. He also served in charge of the exhibition of the National Museum at the Columbian Exposition, in Chicago, in 1893.

In all of this work his remarkable genius for museum administration manifested itself, and the success of the national exhibits was largely due to his remarkable abilities.

Of his many publications the most important were in the line of his chosen science, ichthyology. Among them, worthy of commendation, are "The Game Fishes of the United States," 1879; "The Fisheries and Fishing Industries of the United States" (7 vols.), 1884; "American Fishes," 1887, and with T. H. Bean, "Oceanic Ichthyology," 1893. His writings on museum work include, "Plan of Classification for the World's Columbian Exposition," 1890, and "Museums of the Future, 1890, both of which are quoted as authority the world over. Mention must be made of his interest in genealogy. As a boy he began the preparation of the genealogy of his family, which resulted in 1888 in "Our Virginia Cousins." This led naturally to his being chosen one of the editors of the "Wesleyan Book," and later to his active participation in the founding of the American Historical Association, in the proceedings of which he published "The Origin of the National Scientific and Educational Institutions of the United States" in 1890.

Dr. Goode was one of the founders of the District of Columbia Society of the Sons of the American Revolution, becoming at once one of the officers, and since 1894, its President. He was also a Vice-President of the Sons of the Revolution and a Deputy Governor of the Society of Colonial Wars. In the scientific societies of Washington City he was ever a prominent member, having been President of the Philosophical Society and the Biological Society. He had been President of the Cosmos Club, and was at the time of his death an overseer of the Columbian University, and in many other ways had been actively associated with the intellectual progress of the National Capital.

In addition, he was a member of many of the leading scientific societies, both in this country and abroad, including the Zoological Society of London, the National Academy of Sciences in the United States, and was recently elected Vice President of the American Association for the Advancement of Science for the Section on Zoology.

Dr. Goode had received the degree of Ph. D. from the Indiana University, and that of LL. D. from Wesleyan University, and his services at the Madrid Exposition gained for him the decoration of Isabella.

Dr. Goode was respected and loved by all who knew him, and he was recognized as a fit successor to Professor Baird, the founder of the National Museum.

Josiah Dwight Whitney, Professor of Geology at Harvard University, died at New London, N. H., at the age of 77 years. He was graduated from Yale in 1839, and from that time until his death he was actively engaged in geological research. His field work included a survey of New Hampshire, a geological exploration of the Lake Superior region, and a survey of the mining regions of all the States east of the Mississippi. In 1855 he was appointed State Chemist of Iowa, and was a member of the faculty of the Iowa State University, later also held the position of State Geologist of California. He was appointed Professor of Geology at Harvard in 1860. In time this position was guaranteed him for life in consideration of the gift of his geological library to the museum of that institution. Prof. Whitney was one of our ablest geologists, and like many men of genius he belonged to the genus irritabile. He was an educated musician, no doubt finding that music is useful for "nerves." He had little patience with lay stupidity, and did not always conciliate "the powers that be."

It will be recalled that the Third International Zoological Congress (Leyden, Sept., 1895) appointed an International Commission of five members to study the various codes of nomenclature in use in different countries, with a view to arriving at a more definite international agreement upon the points of difference to be found in these codes. This Commission is composed of Dr. Raphael Blanchard (France), Prof. Carus (Germany), Prof. Jentink (Holland), Dr. Sclater (England) and Dr. Stiles (United States).

It will also be recalled that Dr. Stiles requested the appointment of an American Advisory Committee, to which he might "submit for approval or disapproval all of the questions which he intended to sup-

port in the meetings of the International Commission and with which he might advise regarding concessions to be made or requested in those points upon which American opinion differs from the views held in some of the other countries."

This Advisory Committee has now been completed and is made up as follows:

Dr. Gill, representing the National Academy of Science.

Dr. Dall, representing the Smithsonian Institution.

Prof. Cope, representing the Society of American Naturalists.

Prof. Wright, representing the Royal Society of Canada.

Prof. Packard, representing the American Association for the Advancement of Science.

The September issue of the Western Field and Stream, published in St. Paul, Minn., is noteworthy as presenting a scheme for the protection of the game of the country. Briefly, it contemplates dividing the entire territory of the United States from the Atlantic to the Pacific, into two concessions along the line of the forteith parallel of latitude, or near it, for each of which there shall be uniform laws and uniform close time, the whole to be under the police surveillance of the National association for the protection of game and fish through its multifarious state auxilliaries. The close time for the northern concession will be from January 1 to September 1, and in the southern concession from February 1 to September 1, during which no shooting shall be allowed on any kind of game whatever, excepting that woodcock and shore birds of the order Limicolæ may be shot in August. The general close time for all kinds of inland fishes, recognized as game fishes, to extend from October 1 to June 1, excepting that fishes of the family Samonidæ, may be caught in April and may. These close seasons conform very nearly to the distribution, habitat, and breeding seasons of the various animals which are sought to be protected; and where they do not, especial exceptions may be made, if deemed expedient. The laws which are to dominate will inhere by legislative enactment; uniform in all the states, and coöperative throughout. Emergencies and bodily stress will always stand in plea for exemption from penalty for violation of the laws when well proven.

By private gifts, a Japanese fellowship in economics has been established at the University of Wisconsin, and Mr. M. Shiozawa, of Tokyo, Japan, has been elected to the fellowship for the coming year. Mr. Shiozawa is highly recommended by two distinguished Japanese pro-

fessors, Professor Iyenaga, of the Higher Commercial College, of Tokyo, Japan, and Professor Motora, of the Imperial University, of the same city. He is spoken of as one of the talented young men of Japan, and it is expected he will do a great work for his native country. He has already graduated from a Japanese college, and has published results of his work. He is now on his way to this country to enter upon his studies at Madison.

The members of the Geological Society of France attending the meeting to be held in Algiers, October 6, 1896, will have an opportunity of examining the following localities: October 8–13, Sahel d'alger, Massif de Blida, Médéa; October 14–18, Kabylie du Djurjura; October 19–26, Constantine, Batna and Biskera. In addition to these excursions there will be one preliminary to the meeting. Mr. Brive proposes, October 3–5, to conduct a party to Chélif and Dakra to study the Miocene and Pliocene beds of these districts.

The Brooklyn Institute of Arts and Sciences has in process of construction a Museum of Arts and Sciences. It is proposed to group on one of the four porticos the names of seven great and representative men in science; on another, seven great and representative men in art; on a third, seven great and representative names in philosophy; and on the fourth, seven great and representative names in the realm of the "practicum" or the application of science and art to the so-called material wants of men.

Information has been received that Prof. Daniel G. Elliott, of the Field Museum, Chicago, who is now travelling in Somaliland, has returned to Berbera from Gallas Mountains. He intends to make arrangements at once for exploring the interior of the country. He has been fortunate in securing a good collection of the fauna of the country, including quite rare species.

The well-known naturalist Mr. Charles H. Sternberg has been collecting fossil plants in the Dakota Group in Kansas during the past season, and has obtained a collection of fine specimens which he offers for sale, either as a whole, or by the single specimen.

The first or "general" part of Dr. Richard Hertwig's *Lehrbuch der Zoologie* has been translated by Professor George W. Field of Brown University, and will be published soon by Henry Holt & Co.

Errata of Paper on The Mushroom Bodies of the Hexapod Brain in the August No.

Page 644, line 6, after "Fig" insert I.
" 646, " 31, before "uses" insert and.
" 647, " 11, for "brahlets" read branchlets.
" 647, " 16, insert coma at the end of the line.
" 647, " 31, insert coma at the end of the line.
" 647, " 34, for "Formol-" read formol-.
" 648, " 1, for "which" read a bundle that.
" 648, after "Fig." insert I.
" 649, " 3, for "afferent" read efferent.
" 649, " 19, after "some" insert of.
" 649, in note for "Dujardin" read Dujardin's.
" 649, line 11, for "crythrocephala" read erythrocephala.
" 649, " 12, for "Gehirus" read Gehirns.
" 649, " 12, for "Diehl" read Dietl.
" 649, " 17, for "Ordunung" read Ordnung.
" 649, " 19, for "Fourmics" read Fourmis.
" 649, " 24, for "Retzins" read Retzius.
" 649, " 30, for "trachéátes" read tracheates.
" 649, " 32, for "Mémorie" read Mémoire.

F. C. KENYON.

THE

AMERICAN NATURALIST

VOL. XXX. November, 1896. 359

PINEY BRANCH (D. C.) QUARRY WORKSHOP AND ITS IMPLEMENTS.[1]

BY THOMAS WILSON.[2]

Prof. W. H. Holmes, one of our most accomplished and versatile members, on Nov. 16, 1889, read before our Society an extended report of his investigations in the quartzite boulder quarry at Piney Branch, which was published in the American Anthropologist, January, 1890, pp. 1–26, and is being reprinted in the 15th Annual Rep., Bur. Ethnology. The work described consisted of an excavation up the face of the hill near Piney Branch, in the form of trenches, one 75 feet long and some places 10 feet deep, and other shorter but similar trenches and soundings in the same neighborhood. At the conclusion of his paper, I stood up and complimented him upon the character of the work, and saying that heretofore speculation and theory in the office and with pen and ink, had been employed, instead of actual excavation in the field with the pick and shovel. I congratulated him and the Society upon the new era inaugurated. I said the fact that I did not agree to his conclusions had naught to do with this question and did not prevent me

[1] Read before the Anthropological Society, Tuesday Evening, December 4, 1894.

[2] Curator of Prehistoric Anthropology, U. S. National Museum, Washington, D. C.

from rendering to him the credit for his new mode of investigation, it being the same I had chiefly pursued during my residence in Europe.

Synopsis of the points made in opposition to Mr. Holmes' theories.

1. I concede that Mr. Holmes made a faithful and correct report of his excavations at Piney Branch and of the objects he found there, and I take no exceptions to that part of his paper.

2. But I except to his conclusions. I propose to show that his conclusions are erroneous and that the pretended facts (outside the quarry), on which he based these conclusions, are not facts but assumptions.

3. I propose to show that the objects which he calls "shop-refuse" and which as such should only be found in quarries and on shop-sites, have been equally wide-spread as are the finished implements which he declares to have been the sole aim of the workman in opening the quarry.

4. I propose to show the leaf-shaped implements which he calls "blanks" and which he says were merely material prepared at the quarry for convenience of transportation and to be worked into implements, were themselves finished implements, well-known and commonly used throughout the world in prehistoric times for spears, knives, daggers, etc., with and without handles; and sometimes put to secondary uses, as all other implements might be when broken or the need for their use had passed.

5. I propose to examine the aboriginal village-sites of the District and its neighborhood, in search of the leaf-shaped blades or "blanks" which Mr. Holmes so confidently asserts were carried from the quarry to "other fields" and for other "destinies," and I will show that the number treated as he says, was insignificant when compared with the material procured and the work done.

6. I propose to show the number of caches in the neighborhood to be insignificant; then, that the number of leaf-shaped blades, "blanks," *not in caches*, was also insignificant; then, that those of quartzite which alone could have come from Piney

Branch quarry, are still more insignificant in number, and, finally, that the number of finished implements, such as arrow- and spear-heads, scarpers perforators, etc., *of quartzite* which alone could have been made from Mr. Holmes' "blanks," is even less than insignificant when compared with the mass of implements made of quartz, felsite, argillite, chert, other material than quartzite.

7. I propose to show that, while his facts are assumptions, (always excepting the excavation) he has committed the double error of deducing a wrong conclusion from them, and I will show not only that the leaf-shaped blades (his blanks)—and along with them his finished implements, were not only *not made* from the "double turtle-back," but that they could not be made from it; and that Mr. Holmes' elaborate theory of manufacture as shown by his arranged series of "single turtle-backs," "double turtle-backs," leaf-shaped blades (blanks) and finished implements, being made one after another, each out of the one preceding, will break in two in the middle, because he cannot make the leaf-shaped blades (blanks) out of the double turtle-back without first practically reducing it to its original condition of an unworked pebble.

8. I will contend that the objects found in the quarry should be admitted in evidence and compared with other similar implements in the determination of their age. If this is not done and we are confined for evidence of age, to the quarry and its surroundings, under the assertion that it belonged to modern Indians, then I will attack its antiquity and attempt to show that it may be even more modern than the Indian, and that the quarry might all have been made while digging boulders with which to pave Pennsylvania Avenue in early times. I do not assert this to be true, but if we are deprived of the evidence of the worked implements and driven to surface indications, I will contend that there is evidence (1) of the trench having been entirely filled and carefully levelled so that the contour of the hill shows no trace of excavation which would not have been done by the Indian; (2) that the only other indications of age are the depth of soil on the surface and the size of the trees growing thereon, both of which might have

been accomplished since the laying out of the city of Washington.

9. I make further objection to Mr. Holmes' theory that it assumes jurisdiction of all questions, controverted or not, concerning the age of the quarry, and decides them in such manner as to preclude all further examination. If his theory that the quarry was opened and worked and the implements made by the modern Indian be correct then his decision closes the investigation and passes a final judgment from which there is no appeal.

I.

A portion of the interested public seem to be of the opinion that Mr. Holmes' excavation at Piney Branch was a severe blow to the possibility of a Paleolithic Period in the United States, if it did not destroy that theory altogether. I was not shaken in my faith. My judgment and, I may say without egotism, my years of study of the subject, have given me such an understanding of it as that the excavation at Piney Branch has not caused me to reverse my opinion. If Paleolithic Man existed in America, the traces will be found elsewhere, and a final adverse decision cannot be made upon evidence from a single locality. Therefore I could bide my time.

If the excavation at Piney Branch belonged really to prehistoric times, it is equally favorable to a Paleolithic Period as against it. The investigation reveals nothing incompatible with that theory. The conclusions of Mr. Holmes as announced in his paper were opposed to this, but conclusions are not facts, and renowned investigators have been known to discover many facts, all true, on which they based conclusions which were all error. There are some things in Mr. Holmes' paper not facts but conclusions stated as though they were facts, frcm which I entirely dissent. Some of these, I propose to examine. In this paper, the facts of discovery, as stated by Mr. Holmes, will be admitted ; the errors of argument, theory and conclusion will be combatted.

Prof. Holmes' first five pages (Amer. Anthrop. IV. Jan., 1890.) are employed with the history of the locality ; pp. 5 to 9 are filled with a description of the work done and the

method of doing it; at the foot of page 9 commences his description of the art-product. As Mr. Holmes proceeds in his paper with the classification and study of these implements, he announces a primary distinction between those which bear evidence of design and those which do not—p. 11. Every archæologist knows of this as a prime necessity. Every one tries to keep to this distinction. The difficulty is in doing it.

True advancement in science depends on the correctness of the conclusion. To rightly decide this, decides the whole question, not only in this, but in almost every case concerning prehistoric man. Mr. Holmes has no difficulty in this regard. He has supreme confidence in his own ability and admits no possibility of mistake or error. He says same paper, (ibid.) p. 11, "With these distinctions (of design) in mind, the archæologist has but little trouble in recognizing and separating all classes of products and the *uninitiated* with a little careful study may readily learn to do the same. Having handled the products of this shop constantly for a period of several weeks, I have familiarized myself with every variety of form and shade of contour, and *do not feel the least hesitation* in presenting the results of my selection and classification." He then describes his Plate IV, (my Plate XIX), in which "is presented a series of worked stones from this site, which represents every variety of product and epitomizes the entire range of form. Beginning with the boulder *a* from which two chips have been taken, we pass through successive degrees of elaboration, reaching final forms in *k*, *l*, *m*, long leaf-shaped blades. * * If it be asked how I know this series is complete * * the discarded remnants tell the story, * * if every entire flaked tool had been taken from the spot, the record would remain with a certainty that is absolute."

He describes, *seriatim*, the manufacture through the first two stages, and he showed practically before the audience the two processes employed, which consisted of the simple operation of "grasping a boulder in either hand, strike the edge of one against the other so as to detach a flake, then a second and third until the circuit (of the pebble) is completed as shown in *a* to *d* and *n*, Plate IV, thus making a typical turtle-back."

(Fig. 1). The stone was turned in the hand and a second series of blows given on the other side, and the result was a "double-turtle-back," (Fig. 2). The third stage is described in an uncertain and indefinite way, rather out of keeping with the rest of the paper. "If the form (of the first-two stages) developed properly, the work was continued into what I have called, for convenience, the third stage. It consisted in going over both sides a second and perhaps a third time, securing, by the use of small hammers and by deft and careful blows upon the edges, a rude but symmetrical blade. A profile is given at *p.* in Plate IV." It is to be remarked, as a matter of importance, that his manipulations are confined to the first two stages and do not enter upon or pretend to show the third process which reduces the object

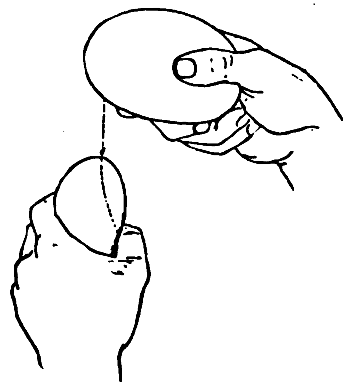


FIG. 1.
Free-hand on direct percussion; first step in shaping an implement from a bowlder. (Prof. Holmes fig. 6).

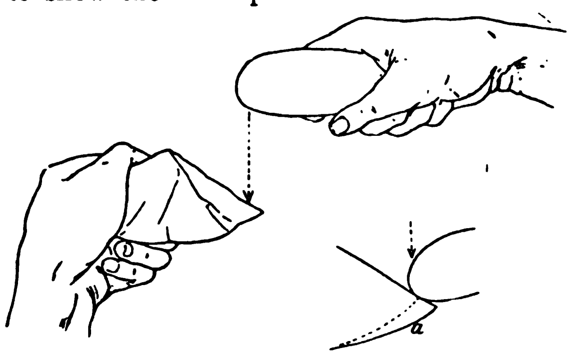


FIG. 2.
Direct percussion; manner of Striking where the edge is sharp. (Prof. Holmes fig. 7).

from a thick and rude implement into "a straight and symmetrical blade less than one-half inch in thickness." There his process fails, and he has not, nor do I believe he

can, by any such process as he has indicated, make out of either his first or second stages, the leaf-shaped implement of the third stage. (I will refer to this later).

His notes on Plate IV are descriptive of the processes and products, and in this he speaks with the air of a master, describing with particularity the intentions and desires of the aboriginal maker, telling wherein he was unsuccessful and specifying the causes of rejection.

First stage—one side worked.

"*a.* Boulder with two flakes removed.

b, c, d. Specimen worked on one side only and had *probably been rejected on account of perverse fracture or excessive thickness.*

Second stage—both sides worked.

e. A few flakes removed from the back; *fracture perverse.*

f, g. Carefully worked on both sides, but still excessively thick, *hence the rejection.*

h. Broken by a stroke *intended to remove a prominent hump.*

Third stage—both sides worked.

i. Neat in shape, but with a ridge or hump on the back which *many strokes have failed to remove.*

j. Unsymmetric broken blade.

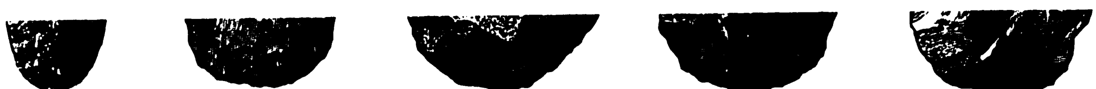
k, l, m. Thin, neat, broken blades. * * The last specimen of the series, *m*, is, perhaps, *the most advanced form found,* but that *it was not finished* is clear. * * It is highly improbable that we have in the whole series of products of the quarry, here epitomized, *any finished tool,* either whole or represented by fragments. This should *not be regarded as an opinion only; it is a conclusion based upon evidence that cannot be lightly treated* by the scientific investigator."

This is sufficiently positive to admit of no mistake as to the meaning of the writer. It is an *ex cathedra* opinion. It is the conclusion of one who knows, and who knows that he knows. It is the "Thus Saith the Lord" of holy writ. It is the *ipse dixit* of one who speaks by authority rather than the opinion of one who is making his first essay in his new appointment as Archæologist. This short sentence decides off-hand the

the whole question at issue, for it has been my contention that implements such as were described by Mr. Holmes have been found in many parts of the United States and nearly all over Europe, and have in the latter country always been treated as finished implements belonging to the Paleolithic period; and I respectfully submit that my side of the case is not to be overturned by a declaration made with whatever loudness or clamor, or with whatever positiveness and determination, and which, while declared as a fact, is naught but an assertion.

After having described the manufacture, closing with the third process, the making of the leaf-shaped implements, he says, page 13, "Having followed the process to the end, I wish to call *especial attention to the fact*, if my theory be correct, that when this thin blade was realized, the work of this shop and *the only work of this shop was ended.*" I ask—What right has Mr. Holmes to make this statement? What evidence is there of it? How can he know its truth? How can he know that the "turtle-back" may not have been intentional on the part of the workman, as well as the leaf-shaped blade? How can he know aught about the intention of the workman except, as can every other person, from the implements themselves and the condition in which they are found and the objects with which they are associated? Continuing, "The process and the machinery had accomplished all that was asked of them and all that they were capable of accomplishing."

What evidence of truth have we in these assertions? Again, "the neat, but withal rude blades, and they only, *were carried away and to destinies which we may yet reveal*," p. 13. Who knows what blades were carried away, and what blades *only* were carried away? Is this not an unwarranted assumption? "Further work, additional shaping, employed other processes, and *was carried on in other fields.*" Why? How? In what fields? What further work? What additional shaping? What processes? Where carried? And who knows aught about it? If the writer of these statements had been himself a first-cousin of the paleolithic man and personally present with his kinsman at the close of the Glacial epoch, making notes and sketches of the quarry workshop of Piney Branch,



(18753) (7500) (19969) (13351) (7499)

Leaf-shaped implements, half size. The upper one from France, the lower ones from the United States.

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PLATE XX.

(15753) (7506) (19969) (13351) (7499)

Leaf-shaped implements, half size. The upper one from France, the lower ones from the United States.

he could not have spoken with greater positiveness and more certainty of knowledge. One might almost be pardoned, if he continued the reading to the foot of page 13, for believing that the writer considered himself not only omniscient but omnipresent. For he says, "That every implement resembling the final form, made from a boulder or similar bit of rock, must pass through the same, or much the same stages of development just described, whether shaped to-day, yesterday or a million years ago, whether in the hands of the civilized, the barbarian or the savage man."

I envy Mr. Holmes his confidence in his acquaintance with the times and men of high antiquity. Such intimate acquaintance with abstruse and unknown problems is equalled only by Dr. Jock Hornbook's acquaintance with medicine and knowledge of drugs; and he repeats this assumption when he says, "There were no examples of successful quarry-products left upon the ground, all forms available for further shaping or for immediate use *were carried away*, being the *entire products of the shop*, and the *only reward* for the long-continued and arduous labor involved in their production."

In the same strain, in the next paragraph, p. 14, he urges us to keep these facts clearly in mind and then says "it is almost superfluous to expend words in showing that all forms found in the workshop other than the thin blade, accidentally lost, are mere waste," and he declares with a heroic "pang of regret" that he is compelled to drop the turtle-back, single or double, wholly and forever from the category of implements and to consign it to the oblivion of "failures." And, as if to make an end to the discussion and settle the question forever, he, in the next sentence, extends his denunciation to all similar forms throughout the Potomac Valley. We should "cast them at once and without hesitation into the refuse."

In the same manner, and with similar sentences, he settles and decides in an off-hand manner, as though it rested upon some great and well-known law and well demonstrated evidence, the very question at issue, and considers that his discovery has put it beyond the pale of intelligent discussion.

"All forms found in the workshop other than the thin blades accidentally lost are *mere waste;* * * * this spot is a great workshop where tools were shaped or rather roughed out, and *these things are the failures.*" P. 14.

This is the precise question, decided so dogmatically and with such an *ex cathedra* opinion, which is the point of the discussion; for it has been contended by those who believe in the probability of a paleolithic period in America, that whatever these implements were, they were not failures, not waste or debris, but were intentionally made, and whether they were or were not implements of the paleolithic man, they correspond in a remarkable degree with undoubted paleolithic implements found in nearly every country of Europe.

Mr. Holmes admits, p. 17, "that to a limited extent (he might well have said unlimited) the rude forms—the turtle-back and its near relatives—*are also found scattered over the Potomac Valley outside the shop on the hills.*" He might have added that they were to be found practically all over the United States. I propose to show that they are even more plentifully scattered over the surface of the hills and fields in the neighborhood of Mt. Vernon than around Washington. "This (the above) would seem to conflict with my former statement that all these are failures and were left upon the factory sites," and he adds "It is time therefore, that I should define a stone-age workshop." Mark the adroitness with which he confines the implements within a workshop and yet accounts for their general dispersion throughout the country. He accomplishes it by defining a workshop to extend all over the country. "It (a workshop) is any spot where an individual desiring to make an implement, picks up one or more boulders or bits of stone, proceeds to shape what he desires.

His definition of a workshop is on a par with his other argument. This definition leads him into reasoning in a circle: (1) all turtle-backs are the failures of the workman; (2) this is proved by all the failures being left in the workshops as debris; (3) wherever you find a turtle-back was made, there was a workshop; (4) wherever you find it in a workshop, it was left among the debris. Can any process of reasoning be more

vicious than this? "There is so far no evidence that any inhabitant of the Potomac Valley ever aimed to make by flaking alone any other than the attenuated form," p. 17. "This process leads inevitably to the production of blades *in numbers*, and the supply for the entire year was to be obtained probably within a small fraction of a year, the working period being determined by the season, by tribal movements, or by the limitations of time."

Mr. Holmes disposes these leaf-shaped implements by declaring them (p. 18) to have "been roughed out in numbers to a stage of advancement that made them portable, and at the same time brought them within reach of the processes which he employed in finishing, that they were carried away to the villages and buried in damp earth (cached) until the time came for flaking them into the final forms required by the art. * * * This history of these quarry forms is to be completed by their final distribution among the inhabitants of the various tribes, where we have witnessed the final step in the shaping process —the shaping out of specific forms with a bone tool—and their final adaptation to use and dispersal over the country."

I scarcely know what answer to make to all this. It is so complete and perfect an assumption that one scarcely knows how to make a rational argument against it. I shall refer later to, and show the error of fact contained in this assumption. These leaf-shaped forms have been found in every age of the prehistoric man, in almost every country in the world, they have been of every size—from 16 inches in length down to $\frac{3}{4}$ of an inch. Pl. XX. They are made with all degree of fineness, have preserved their general form and characteristics, and we may imagine in a general way the use for which they were intended. They may have been used as arrow or spear-heads, or they may have been inserted in a shaft or handle and used as spears, or by shortening the handle, as knives; (figs. 3, 4) or they may have been wrapped with hide, bark or grass and this served as a handle (fig. 5). But that they were completed implements ready for whatever use they might have been intended, no prehistoric archæologist of whom I have any knowledge has ever doubted. A duplication and extension

of the uses of nearly every implement may be attributed to the prehistoric man as has been done by the civilized man. No one can prescribe the limits within which a sailor's sheath knife may not be used. A hollow-handled awl is sold in our stores to-day for 50 cents representing itself to be an entire set of tools. The same duplication, changing of purpose and

3 4 5

FIGS. 3, 4. Leaf-shaped implements of jasper, chipped to shape, fastened in wooden handles with pitch or bitumen—to be used as knives, from the Pacific Coast, half size.

FIG. 5. Leaf-shaped blade, chipped, of mattled absidian, wrapped in other skin, used for knife. Collected by Capt. P. A. Ray, from Hoopa Valley, Cal., half size.

adaptation to other purposes, may not have been impossible with the prehistoric man in his employment of these leaf-shaped implements, but that all the leaf-shaped implements, the products of this quarry, and by consequence, all others, should have been but prepared material, wrought from the pebble, to be carried to the home of the man who made them, to be flaked into some unknown and unsuspected implement and then peddled over the country, is an assumption which

has so little foundation as to weaken rather than strengthen his argument. It is only referred to as being on the level with the rest of the paper and to show what a large proportion of it is assumption, and how slight is its foundation of fact.

The method of determining the kind and use of implements, their mode of manufacture and the expected benefit which induced the prehistoric man to expend himself upon them, by comparing him with ourselves, with putting him in our places, or putting ourselves with our knowledge and skill, culture and spirit of invention, in his place, and then deciding everything he did from our standpoint, is one of the errors of modern archeologists and one which leads them far from the right path. This discussion leads Mr. Holmes into the processes of the manufacture of flaked stone tools, and he explains direct or free-hand percussion, declares its limitation, how it was the only method known in early times, and throughout pages 15 and 16, explains the details, giving philosophic dissertations upon the art of stone flaking or chipping and, of the development of the spirit and technology of art required in this work of making leaf-shaped blades. No better answer could be given to this theory than the exhibition of the finely flaked flint implements all prehistoric, coming, some of them, from Scandinavia, Mexico, and a large number belonging to the paleolithic period, found throughout the interior of France in what M. de Mortillet calls the "Solutrèen epoch," M. Reinach and others, the "Cavern epoch." Yet Mr. Holmes has never been able to reproduce one of them or to overcome the difficulties of their fabrication.

(*To be Continued.*)

THE GEOGRAPHICAL DISTRIBUTION OF BATRACHIA AND REPTILIA IN NORTH AMERICA.

BY E. D. COPE.

As is well known, the aggregates of organic beings called faunæ and floræ correspond in part with the natural land divisions of the earth's surface, but not exactly. The first classification of the primary faunæ was proposed by Dr. P. L. Sclater in 1858, as follows:

1. *Paleartic.*—Europe, Northern Africa, Northern and Central Asia.

2. *Ethiopian.*—Africa south of the Great Desert, and Madagascar.

3. *Indian.*—Southeastern Asia and the Malay Archipelago.

4. *Australian.*—Australia with New Guinea and the adjacent islands, New Zealand and Polynesia.

5. *Nearctic.*—North America as far south as Mexico.

6. *Neotropical.*—Central and South America and the West Indies.

Subsequently Dr. A. R. Wallace proposed that the name Oriental be used in place of Indian.

In 1868 Prof. T. H. Huxley proposed that the world's areas be arranged in two divisions, Arctogæa and Notogæa; the former including the Palearctic, Indian, Ethiopian and Nearctic of Sclater, and the latter including the Australian and Neotropical regions. To the last two he added the Novo Zealanian for New Zealand, and he proposes to change the name of the Neotropical to Austrocolumbian.

In 1871 Dr. J. A. Allen proposed the following faunal divisions: I. Arctic Realm; II. North Temperate Realm; III. American Tropical Realm; IV. Indo-African Tropical Realm; V. South American Temperate Realm; VI. African Temperate Realm; VII. Antarctic Realm; VIII. Australian Realm.

In 1874 Sclater modified his system as follows: He retained the term Arctogea in the Huxleyan sense. To the Neotropical region he gave the name of Dendtrogea, and to the Australian

he gave the name Antarctogea, omitting New Zealand and Polynesia, which he constituted a fourth division, Ornithogea.

In 1878 Heilprin proposed the name Holarctic, to include Sclater's Palearctic and Nearctic regions. He also proposed two transitional regions; that of the Old World he called Mediterranean and that of the New World the Sonoran, the latter a term already introduced by Cope for a division of the Nearctic of Sclater.

In 1884 Gill proposed the following primary divisions or realms: 1. Anglogæan (N. American); 2. Eurygæan, or Eurasian; 3. Indogæan; 4. Afrogæan; 5. Dendrogæan, or Tropical American; 6. Amphigæan, or Temperate South American; 7. Austrogæan, or Australian; 8. Ornithogæan, or New Zealand; 10. Nesogæan, or Polynesian. Prof. Gill justly insisted on the importance of fresh water fishes as furnishing definitions of natural faunal realms and regions.

In 1890 Blanford published a system of geographic zoology in which he adopted the primary divisions of Huxley, and divided the Arctogaean region into the following: Malagasy, Ethiopian, Oriental, Aqulonian (= Palearctic and northern part of Nearctic), and Medio-Columbian (S. part of Nearctic).

In 1896 Lydekker proposed the following divisions: I. Notogæic Realm; regions: 1. Australian; 2. Polynesian; 3. Hawaiian; 4. Austromalayan. II. Neogæic Realm; regions: Neotropical. III. Arctogæic Realm; regions: 1. Malagasy; 2. Ethiopian; 3. Oriental; 4. Holarctic; 5. Sonoran. Lydekker makes use of paleontologic evidence in this connection. While this treatment of the subject is important from the point of view of origin, it is often irrelevant, since the distribution of vertebrate life in each geologic age was different from that in each other geologic age.

In an essay on the geographical distribution of North American Reptilia published in 1875, the present writer adopted the first system of Sclater. After a lapse of twenty years, the light thrown on the subject by various investigators suggests the following modifications. In the first place the recognition of the close similarity of the life of the northern regions of the earth, requires more definite formulation than was accorded it in

Sclater's first system, by the union of his three divisions of Nearctic, Palearctic and Indian into one, for which the name Arctogæa is appropriate. The enclosure of his Ethiopian division in it as proposed by Huxley, does not seem to me to be proper, in view of the important types of fishes and reptiles which characterize it; *e. g.*, the Crossopterygian, Dipnoan and Scyphophorous fishes, and the Pleurodire tortoises. In the fishes, indeed, the Ethiopian region has as much affinity with the Neotropical fauna as with any other, in its Characin and and Cichlid families, and in the Dipnoan subclass. The presence of the Dipnoi and the Pleurodire tortoises ally it to the Australian fauna as well. It is for these reasons that Prof. Gill proposes to combine the southern hemisphere realms into a single "Eogaean" division. The northern affinities of the Ethiopian realm are, however, too many to permit us to regard this arrangement as a just expression of the facts. Thus, it has Insectivorous Mammalia, Firmisternial Batrachia Anura, and Cyprinid fishes, none of which are Australian or Neotropical types. The course that remains under the circumstances is to regard the Ethiopian Realm as fully distinct from the other three. The definitions of the four primary divisions are then as follows:

The *Australian realm* is peculiar in the absence of nearly all types of Mammalia, except the *Ornithodelphia* and the Marsupials; in the presence of various Ratite birds, in great development of the Proteroglyph serpents, and absence of the higher division of both snakes and frogs; *i. e.*, *Solenoglypha* and *Firmisternia;* in the existence of *Dipnoi* (*Ceratodus*) and certain Isospondylous families of fishes. On the other hand many of the lizards and birds are of the higher types that prevail in India and Africa, viz.: the *Agamidæ* and the *Oscines.*

The *Neotropical realm* only possesses exclusively the Platyrhine monkeys and the great majority of the humming birds. It shares with the other Southern regions the Edentate and Tapiroid mammals; Ratite, Pullastrine, and Clamatorial birds; Proteroglyph snakes; Iguanid Lacertilia, the Agamids being entirely absent; Arciferous frogs; and Characin, Chromid, Osteoglossid, and Dipnoan fishes. It has but few types of the

Northern regions; these are a few bears, deer, and oscine birds. Insectivorous mammalia, Viperid serpents, and Ginglymodous, Halecomorphous and Cyprinid fishes are wanting, except on the northern border.

The *Ethiopean realm* is that one which combines the prevalent features of the Arctogean realm with the southern hemisphere types already mentioned, together with some found elsewhere only in the Indian region, and a very few peculiar. The two latter classes not being mentioned elsewhere, they may be here enumerated. The region shares, with the Indian alone, the Catarrhine monkeys, the *Elephantidæ, Rhinocerotidæ*, Nomarthrous Edentata and Chameleons. Its peculiar types are the *Lemuridæ, Hippopotamidæ* and *Protelidæ, Cryptoproctidæ* and *Hyracoidea* among mammals, and *Polypteridæ* and *Mormyridæ* among fishes. It possesses in common with the Neotropical realm characinid, cychlidi, and dipnoan fishes; Pleurodire tortoises and Ratite and Trogonoid birds; and differs from it in the absence of arciferous Batrachia and crotalid snakes, and presence of dendraspid, causid, atractaspid and viperid snakes.

The *Arctogean Realm* is characterized by the absence of types conspicuous elsewhere, and by the presence of a few peculiar forms. Among fishes it lacks Dipnoi and Crossopterygia, Osteoglossidæ, Characinidæ and Cichlidæ. It lacks Pleurodire tortoises and Ratite birds. Ginglymodous fishes and Urodele Batrachia are nearly confined to it, merely extending a little over the border of the Neotropical. Its Cryptodire tortoises extend both into the Neotropical and Ethiopian. Anguid lizards are confined to it. It shares most of its Mammalia with other regions. The Insectivora it shares with the Ethiopian, and its deer and camels with the Neotropical. The genus Ursus is very characteristic, one aberrant species only extending into the Neötropical.

From what has preceded it is seen that the primary differences between the faunæ of the realms are to be found to a large degree in the lower vertebrata, the fishes, Batrachia and Reptilia. These forms furnish stronger distinctions than the birds and mammals, owing to their greater inability to traverse

natural boundaries. Neglect of these indications has led to much of the difference of opinion in the question of geographical distribution, which have been founded principally on the conditions presented by the birds and mammalia.

In this system fragments of existing or old continents, which have been subjected to conditions unfavorable to particular forms of life otherwise prevalent in them, are, as in the system of Sclater, disregarded. Thus, islands generally are not regarded as presenting conditions definitive of divisions of the first rank, as was done by Huxley and Gill in the case of New Zealand, and Gill and Lydekker in the Polynesian Islands. The temperate regions of Africa and South America are certainly not separable from the tropical portions, as divisions of primary rank, as was done by Allen, who is followed as to South America by Gill. With equal propriety western North America might be separated from Mississippi and Atlantic North America, on account of the great deficiency of its fish fauna. In estimating faunistic affinities one has to give similarities over a given area more weight than differences, where the differences are only due to absence of types.

Finally, it must be remembered that there are geographic points of transition between all the realms.

I. The Arctogean Realm.

This realm includes three regions, viz.: the Indian, the Holarctic and the Medicolumbian. I have already defined the first two in general terms. The third is the transitional of Heilprin, the Sonoran of Merriam and Lydekker, and the Neotemperate of Townsend. It embraces what is left of the Nearctic of Sclater after the subtraction of the Holarctic. As the name Sonoran has been previously given by me to one of the districts of this region, I have preferred to use for it the name given by Blanford.

The faunal characteristics of these regions may be enumerated af follows:

Indian Region.—Presence of Holostomatous fishes. Absence of Ginglymodous and Halecomorphous and Salmonid fishes. Presence of Cœciliid Batrachia. Absence of Trachystomatous,

Amphiumid, Cyptobranchid, and Arciferous Batrachia. Presence of Agamid lizards, and Anigiostomatous and Viperid snakes. Presence of Phasianid, Eurylæmid, Nectariniid and Pittid birds. Absence of Tyrannid and of several nine-quilled oscine families. Presence of nomarthrous Edentata, of Viverridæ, Hyænidæ, Tupæidæ and Tarsiidæ. Presence of Rhinocerotidæ, Tapiridæ, Proboscidia, and Catarrhine Quadumana, and Anthopomorpha. Absence of Didelphyidæ, Procyonidæ and Scalopidæ.

Holarctic Region.—Absence of Holostomatous and Halecomorphous fishes. Presence of Ginglymodous and Salmonid fishes. Absence of Trachystomatous, Amphiumid and Cœciliid Batrachia, and absence of the Arcifera except the family Discoglossidæ (two species of Hyla excepted). Absence of Angiostomatous and presence of Viperid snakes. Presence of Phasianid, and absence of Eurylæmid, Nectariniid, Pittid and Tyrannid birds, and of several nine-quilled oscine families or subfamilies. Absence of Nomarthrous Edentata, of Viverridæ, Hyænidæ, Tupæidæ, Rhinocerotidæ, Tapiridæ, Proboscidia, Quadrumana and Anthropomorpha (except Homo).

Medicolumbian Region.—Absence of Holostomatous fishes; presence of Ginglymodous and Halecomorphous fishes. Presence of Trachystomatous, Amphiumid, Aciferous and Firmisternial Batrachia, and absence of Cœciliidæ. Presence of Iguanid, and absence of Agamid and Chamæleonid lizards; absence (except three species) of Angiostomatous and of Viperid snakes. Absence of the Indian types of Passeres mentioned, and presence of Tyrannid Clamatores, and several groups of nine-quilled Oscines (Icteridæ Mniotiltidæ, Tanagridæ). Absence of all the specially Indian mammalia, and of the Holarctic Erinaceidæ, and presence of Didelphyidæ (one species), Scalopidæ and Procyonidæ.

In defining these regions I have restricted myself necessarily to types of tolerably high rank, and have not referred to species. This is because species are not generally characteristic of entire divisions, but only of parts of them. One cannot, however, be absolutely exact in such major definitions, since a number of the conspicuous types in each are not universally distributed over these areas.

In comparing the Holarctic with other realms, I have already referred to the number of types which it possesses in common with the Ethiopian, not found in the Neotropical. It has also several in common with the Neotropical, which do not occur in the Ethiopian. These are the Arciferous Batrachia, the Crotalid snakes, and the deer (Cervidæ). The Medicolumbian division of the Holarctic shares other forms with the Neotropical. These are Didelphyidæ and Procyonidæ among Mammalia; Tyrannid, Icterid and Tanagrid birds; Kinosternid tortoises and the Arciferous Batrachian family Hylidæ.

Some of the forms of the Holarctic region are not uniformly distributed over it. Thus the Ginglymodous and Spatulariid fishes only occur in the eastern parts of the eastern and western continents. The same is true of the Silurid genus Amiurus and the Loricate genus Alligator. The Crotalid snakes are not found in the western parts of Eurasia. The Batrachian Cryptobranchidæ have the same distribution.

VII. The Medicolumbian Region.

This region was formerly included in the Nearctic of Sclater, and the area thus constituted has the following geographic boundaries. To the south it includes the plateau of Mexico, including the central valley. The Neotropical area bounds it to the east and west, occupying the low-lands or Tierra Caliente to a point 150 miles south of the Rio Grande on the east, (Townsend, Texas Academy of Science, 1895, p. 87), and to Mazatlan, or some point not far from it, on the west. The high land of Oaxaca is its extreme southern outpost. Its northern boundary is thus described by Merriam.[1] The "Boreal" (Holarctic realm) "Province extends obliquely across the entire continent from New England and Newfoundland to Alaska, conforming in direction to the trend of the northern shores of the continent. It gives off three long arms or chains of islands which reach far south along the three great mountain systems of the United States, a western arm in the Cascades and Sierra Nevada, a central arm in the Rocky Mountains, and an eastern

[1] Biological survey of the San Francisco Mountain; N. Amer. Fauna, No. 3, 1890, p. 24.

arm in the Alleghanies, and these interdigitate with northward prolongations of the Sonoran" (Medicolumbian) "province, which latter completely surrounds the southern islands of the Boreal" (Holarctic) "system."

The faunal relations of the Medicolumbian realm may be tabulated as follows:

Agrees with Holarctic in	Differs from Paleartic in	
	Peculiar Forms	Neotropical Forms
Mammalia in general		Bassaridæ.
		Procyonidæ.
Except	Antilocapra	Megadermatidæ.
	Mephitis	Dicotyles.
	Scalopidæ	Didelphys.
		Cathartidæ.
		Tanagridæ.
Birds, except		Icteridæ.
		Clamatores in general.
		Trochilidæ.
	Meleagridæ	Odontophorinæ.
		Alligators.
		Tiid and Gerrhonotid lizards.
		Iguanid lizards.
Emyd tortoises	Chelydridæ	Cinosternidæ.
Lachesinæ	Crotalinæ	Elapid venomous snakes.
Raniform frogs		Arcifera.
Scaphiopidæ	Plethodontidæ	Engystomidæ.
	Amblystomidæ.	
	Desmognathidæ.	
Diemyctylus		
Cryptobranchidæ		
	Trachystomata.	
	Necturus.	
	Amphiumidæ.	
Percid fishes	Aphredoderidæ	
Cottidæ	Percopsidæ.	
Haplomi	Amblyopsidæ.	
Accipenseridæ.		
Spatulariidæ.		
Cyprindæ	Plagopterinæ.	
	Catostomidæ.	
Gasterosteidæ.		
Salmonidæ.		
	Amiidæ.	
	Lepidosteidæ.	
Petromyzon		

Baird[2] divided this region into three districts, which he termed the eastern, central and western. The eastern occupied eastern North America to the central plains, where they exceed 800 feet above sea-level. The western included the territory between the Cascade and Sierra Nevada Mountains and the Pacific Ocean. In my paper of 1875,[3] I adopted the eastern, central and western districts (calling the last the Pacific), and proposed two other districts, viz.: the Austroriparian for the Louisianian division of the eastern of Verrill, and the Sonoran for the southwestern and Mexican Plateau faunæ. Merriam, in 1890,[4] proposed a different arrangement. Using the name Sonoran for the entire Medicolumbian Region he divided it into "(1) an *Arid* or Sonoran subregion proper, occupying the table-land of Mexico, reaching north into western Texas, New Mexico, Arizona, and southern California; (2) a *Californian* subregion, occupying the greater part of the State of that name; (3) a *Lower Californian subregion;* (4) a *Great Basin* region, occupying the area between the Rocky Mountains and the Sierra Nevada, extending as far north as the plains of the Columbia; (5) a *Great Plains* subregion, occupying the plains east of the Rocky Mountains, and extending north to the plains of the Saskatchewan; and (6) a *Louisianian* or *Austroriparian* subregion, occupying the low-lands bordering the Gulf of Mexico and the Mississippi, and extending eastward south of the Alleghanies to the Atlantic seaboard, where it reaches as far north as the mouth of Chesapeake Bay." According to his arrangement the Eastern Region of Baird and myself is not mentioned.

This classification may be applicable to birds and mammals; but it is not applicable to the fishes, Batrachia and Reptilia, which are much more exact indicators of the histories of faunæ, owing to their inferior powers of migration. The eastern district or subregion is more nearly allied, from this point of view, to the Austroriparian than the latter is to the Sonoran proper, or arid region. This is due, as Baird previously pointed out,

[2] Amer. Jour. Sci. Arts, XCI, 1866, p. 82.
[3] Bulletin U. S. Natl. Museum, I, 1875, p. 55.
[4] N. American Fauna, 1890, No. 3, p. 24.

to the great difference in rainfall between the part of the continent lying eastward of the 100th meridian and that part which lies west of it. This difference is coincident with a profound difference in geologic age between the regions west of that meridian and the eastern district, the former having a short continental history as compared with the latter.

I, however, agree with Merriam in the abolition of the "Central" as a subregion of Medicolumbia.

The relation of the several zoological divisions to these subregions are as follows: The eastern subregion is the original centre of distribution of all the fishes peculiar to the Medicolumbian region, except only the Plagopterine Cyprinidæ. It is the centre of distribution of all the Batrachia, with the following exceptions: The degenerate types of Trachystomata and Amphiumoidea probably originated in the Austroriparian subregion, and the species of Bufo in the Sonoran. The eastern subregion is also the source of the aquatic Testudinata. On the other hand the Sauria of the eastern and Austroriparian subregions are an overflow from the abundant lizard life of the Sonoran region, excepting the family of the skincs, and the genus Anolis, the latter being of Neotropical origin. The snakes also are mainly Sonoran types, including especially the true rattlesnakes. The copperheads and ground rattlesnakes are on the contrary indigenous to the eastern subregion. The Pacific subregion has close affinities with the Sonoran, but of a largely different kind as to its lizards, while the Batrachia have the character of the eastern types as far as they go.

The distribution of types indicates six principal subdivisions, which I call the Floridan, Austroriparian, Eastern, Sonoran, Western, and Toltecan subregions. The Floridan subregion includes the greater part of the peninsula of Florida, being bounded approximately on the west by the Suwanee River. The Austroriparian subregion extends northward from the Gulf of Mexico to the isothermal of 77° F. It commences near Norfolk, Va., and occupies a belt along the coast, extending inland in North Carolina. It passes south of the Georgia Mountains, and to the northwestward up the Mississippi Valley to the southeastern part of Illinois. West of the Mississippi the bound-

ary crosses Missouri, extends south along the southern boundary of high lands of Texas, and reaches the Gulf at the mouth of the Rio Grande. The Eastern subdivision is the most extended, reaching from the isothermal line of 77° F. north and from the Atlantic Ocean to the elevated plains west of the Mississippi River. Many of its forms extend up the bottoms of the rivers which flow to the eastward through the plains. The Sonoran subregion extends from the limit of the Eastern as far west as the Sierra Nevada, and south, including Nevada, New Mexico, Arizona, Sonora and the Plateau of Mexico, including the State of Chihuahua, and, perhaps, Durango. It does not cross the Sierra Nevada, but includes the entire peninsula of Lower California. It extends northward on the east side of the Sierra Nevada as far as, including the arid region of British Columbia. It occupies the valley of the Rio Grande, and extends into Texas as far as the Rio Pecos. It extends southward in western Mexico as far as Mazatlan. The Western subdivision extends from the Pacific coast to the Sierra Nevada to an uncertain distance on the Lower Californian Peninsula. At the north it crosses the Sierra Nevada, skips the narrow strip of the Sonoran in Washington, and extends to the Rocky Mountains, including northern Idaho and western Montana. The Toltecan subregion includes the States of Guanajuato, Mexico, and the adjacent elevated regions of Michoacan, Oaxaca and Puebla, including the Alpine regions of the southern Sierra Madre. It is probable that another subregion should be added, the Tamaulipan of Townsend. This is a dry region extending from near the mouth of the Rio Grande to the Rio Soto la Marina in the State of Tamaulipas. More information regarding the fauna of this country is desirable.

The faunal peculiarities of these subregions are well marked. The three subregions included in eastern North America differ from all the others in the abundance of their turtles and the small number of their lizards. Prolific of life, this area is not subdivided by any marked natural barriers. Hence, though its species present great varieties in extent of range, it is not divided into districts which are very sharply defined. The warmer regions are much richer in birds, reptiles and insects

than the cooler; and as we advance northward many species disappear, while a few others are added. The natural division of the eastern part of the continent is then in a measure dependent on the isothermal lines which traverse it, which accord also quite closely with its geologic history.

The *Floridan subregion* is distinguished by the presence of several peculiar genera of Batrachia and Reptilia, and by a number of peculiar species. A special feature is the almost total absence of Batrachia Urodela. The genera are:

BATRACHIA:
Lithodytes,
Pseudobranchus.

SERPENTES:
Stilosoma,
Rhadinæa,
Seminatrix,
Liodytes.

SAURIA:
Rhineüra,
Sphærodactylus.

Lithodytes and Sphærodactylus are West Indian Neotropical genera, and Rhadinæa, besides being Neotropical, extends into the eastern part of the Austroriparian subregion. Five genera are then peculiar. The peculiar species will be enumerated later. Several species of mammals are confined to this region. The genera of birds that do not range north of it are, according to Allen:

Certhiola
Zenæda, *Oreopelia*, *Starnænas* } Pigeons.
Rostrhamus, *Polyborus* } Raptores.
Aramus, *Audubonia* } Waders.
Phœnicopterus.
Haliplana, *Anous* } Terns.

The isolation of the Floridan subregion is due to the fact that the nucleus of the peninsula (which is of Eocene age) was separated from the continent during the greater part of neocene time. If at at any time connected with the Antilles, the period was of short duration.

The *Austroriparian region* possesses many peculiar genera of reptiles not found elsewhere, while the region north of it possesses none, its genera being distributed over some or all of the

remaining regions. The number of peculiar species in all departments of animal life is large. It presents the greatest development of the eastern reptile life. Sixteen genera of Reptiles and eight of Batrachia do not range to the northward, while ninety-nine species are restricted in the same manner. The peculiar genera which occur over most of its area are:

SAURIA:

Anolis.

SERPENTES:

Elaps,
Haldea,
Cemophora,
Tantilla,
Compsosoma,
Farancia.

TESTUDINATA:

Macrochelys.

LORICATA:

Alligator.

BATRACHIA:

Engystoma,
Manculus,
Amphiuma,
Siren.

I have omitted from this list ten genera which are restricted to one or the other of its subdivisions. The *Siren*, the *Cemophora*, the *Anolis*, and the *Alligator*, are the most striking of the above characteristic genera. No genus of lizards is peculiar excepting *Anolis*, which has its greatest development in other than the Nearctic continent. Among Serpents a few genera of Neotropical character extend eastward along the region of the Mexican Gulf, as far as the Atlantic coast, which are not found in any of the northern regions; such are *Compsosoma*, (Central American); *Tantilla*, and *Elaps* (Sonoran). On the other hand, *Abastor*, *Virginia*, *Haldea*, and *Storeria*, embrace serpents which it shares with the Eastern region.

This region is the headquarters of the Batrachia, especially of the tailed forms. The majority of species of the tailless genera are found here, especially of *Hyla* (tree-toads), *Rana*, and *Chorophilus*.

There are no less than nine genera of birds which do not, or only accidentally, range northward of this district. They are, according to Allen:

Plotus,	*Conurus,*
Tantalus,	*Chamæpelia,*
Platalea,	*Campephilus,*
Elanus,	*Helinæa.*
Ictinia,	

All these genera, excepting the last, range into South America or further.

Among mammals, but few species and one genus (*Sigmodon*) are confined to it. *Lepus aquaticus* and *L. palustris*, the cotton rat, etc., and a few others, are restricted by it. The fish fauna is very similar to that of the Eastern region.

The *Eastern subregion* differs from the Austroriparian almost entirely in what it lacks, and agrees with it in all those peculiarities by which it is so widely separated from the subregion. No genus of mammals is found in it which does not range into other regions, excepting *Condylura* (star-nosed mole); but numerous species are confined to it, not extending into the Austroriparian. These number from twenty to twenty-five. Among birds, the following genera are, according to J. A. Allen, shared with the more southern region only: *Quiscalus, Siurus, Helmitherus, Protonotaria, Parula, Mniotilta.* No genus of Reptiles, and but one of Batrachians (*Gyrinophilus*), is confined to this region; but it shares all it possesses with the Austroriparian. It has but four genera of lizards, viz.: *Sceloporus, Cnemidophorus Liolepisma* and *Eumeces.*

The *Sonoran subregion* is characterized in the lower vertebrate fauna, by great poverty in fishes, batrachians and tortoises, and abundance of lizards and snakes. Among fishes it lacks the orders Ginglymodi, Halecomorphi and Chondrostei, and possesses only one peculiar group, the Plagopterinæ, a division of the Cyprinidæ. Of usual Holarctic types it possesses only Isospondyli (Salmonidæ) and Plectospondyli; Percomorphi and Nematognathi being absent. The rivers that intersect its central district contain these types, but they must be reckoned as belonging with their bottom lands to the Eastern subregion; the high plains only belonging to Sonoran. The true drainage area of the Sonoran subregion is that of the Colorado.

No genus of Batrachia is peculiar to it, and the following divisions are wanting: Proteida, Trachystomata, Amphiumoida, and all Pseudosauria, except Amblystomidæ (one species). The genus Bufo is the only one that is well represented.

The following genera of reptiles are peculiar to it:

Uta,	*Anota,*
Uma,	*Lichanura,*
Sauromalus,	*Phyllorhynchus,*
Callisaurus,	*Chionactis,*
Dipsosaurus,	*Chilomeniscus.*

It shares the following genera with the Central American subregion of the Neotropical Realm only:

Ctenosaura.
Eublepharis (also in the Indian region).
Phyllodactylus (also in the Columbian Neotropical).
Heloderma.
Hypsiglena.
Salvadora.
Rhinechis (also Holarctic of Eurasia).
Trimorphodon.
Tantilla (also in Brazilian Neotropical).
Cinosternum (also in Brazilian Neotropical).

The following genera of the Sonoran subregion enter the Texan district of the Austroriparian subregion:

Holbrookia,	*Hypsiglena,*
Crotaphytus,	*Rhinochilus,*
Phrynosoma,	*Cinosternum.*
Gerrhonotus,	

Many species are peculiar to this subregion, as will be shown later on.

The *Western subregion* is distinguished by the absence of most of the types of fishes of the humid part of the continent, and the presence of a few. Thus, the Ginglymodi, Halecomorphi and Catostomidæ are absent, while Percomorphi are present. The Batrachian fauna lacks the Proteida, Trachystomata and Amphiumoidea, while Pseudosauria are abundant,

excepting Cryptobranchidæ. All the families of Salientia characteristic of Medicolumbia are present except the Engystomidæ. Among reptiles the genus Charina is entirely characteristic, and Gerrhonotus of the Toltecan and Sonoran faunæ ranges its entire length. It is especially distinguished by the absence of the following genera: First, all of the Iguanidæ exclusively characteristic of the Sonoran fauna, there remaining only *Crotaphytus*, *Sceloporus*, and *Phrynosoma*, which also enter the Texan district of the Austroriparian; by the absence of *Heloderma*, *Ophisaurus* and *Liolepisma*. Among snakes, by the absence of true water snakes (genus Natrix), and the small burrowing Natricinæ, of Opisthoglyph forms, and of poisonous snakes of the genera *Elaps* and *Systrurus*. No genus but *Charina* can be cited as of universal distribution, which is not at the same time found in some other subregion; but several genera occur in one or the other of its districts which do not occur elsewhere. Similarly no genus of birds or mammals can be exclusively assigned to its entire area; but *Chamæa* of the former class and *Haplodontia* of the latter are restricted to particular portions of it.

The *Toltecan subregion* is characterized by the genera it lacks as well as those which it possesses. Thus, it lacks all the genera of Sauria above cited as characteristic of the Sonoran subregion, including those enumerated as passing over into the Austroriparian ecept *Phrynosoma*. It also lacks the following genera of snakes which are found in the Sonoran:

Lichanura,	*Zamenis*,
Pityophis,	*Phyllorhynchus*.
Ophibolus,	
Chilomeniscus,	

From the Austroriparian subregion it differs in the lack of all the numerous genera of Fishes and Batrachia Urodela, which characterize it, excepting only *Spelerpes*. It lacks also the following genera of snakes: *Cyclophis*, *Virginia*, *Haldea* and *Carphophiops*; and *Natrix* is very sparsely if at all represented.

In its positive characters the Toltecan subregion combines certain forms of both the Sonoran and Austroriparian subre-

gions. Of the former character are *Spea*, *Phrynosoma*, *Barissia*, *Gerrhonatus*, *Hypsiglena* and *Salvadora*; of the latter kind, *Spelerpes*, *Liolepisma*, *Osceola*, *Storeria* and *Systrurus*. Characteristic of Medicolumbia generaly: *Amblystoma*, *Rana*, *Sceloporus*, *Eumeces*, *Diadophis*, *Eutænia*, *Crotalus*. Peculiar genera:

Siredon,	*Hemigenius*,
Thorius,	*Epiglottophis*,
Malachylodes,	*Ogmius*,
Conopsis,	*Ophryacus*.

Neotropical genera: *Oedipus*, *Anolis*, *Celestus*, *Atractus*, *Ninia*, *Drymobius*, *Bothriechis*.

(*To be continued.*)

FOSSILS AND FOSSILIZATION.

BY L. P. GRATACAP.

I.

A fossil, in Paleontology, is any indication of life which has become entirely or partially altered in its substance or condition by the mineral or chemical agencies of its environment. As an " indication " it embraces the widest possible series of remains which have, or could have, any connection with living organisms, from the bones of a vertebrate, the hard parts of an invertebrate, the foliage, fronds, seeds and wood of plants, to the fillings of worm burrows, the tracks of insects, reptiles, mammals, mollusca and crustaceans, and those problematic impressions which have been referred to Medusa, or those by Prof. Hall to the soft parts of an Orthoceras. And it also includes the stony casts formed by the entrance of extraneous mud or silt, sand or chemical deposits within the hard parts of animals upon their death and the disappearance of the soft parts by decay. The hard parts, upon removal by solution, leave the impressions of their interior upon this soft filling which faithfully copies the contour and size of the organism.

These casts form a large group of fossils, and sometimes afford most important information as to the vascular markings and muscular tissues of both crustaceans and brachiopods, though they often perplex the paleontologist by their meagre and unsatisfactory characters. Again, the moulds of exteriors, the phase of preservation complementary to casts must be classed with "fossils." Such impressions often present the superficial ornamentation of shells, and by filling them with soft material as sulphur, wax, or rubber, a reproduction of the original organism in size and form can be satisfactorily obtained. The application of the word "fossil" may be even extended to designate those doubtful evidences of organic life, such as the mixture of the minerals, serpentine and calcite, which have yielded, upon microscopic inspection, some suggestions of organic structure, and with which the famous name of *Eozoon* has been associated, and over which a notable controversy exists to-day. However the term "fossil" is used, its exact meaning from *fodeo* to dig, refers to the most common circumstance connected with the search for fossils, *viz.:* the excavation of rocks or earth, and hence, literally, a *fossil* is a thing *dug out*, implying a past stage of existence in which it has undergone burial and hinting at its subsequent exhumation. This association has no invariable applicability. Fossils are found exposed upon ancient beaches very slightly covered, as in the shell beds at Beauport, New York, where the valves of *Saxicava rugosa*[1] "form a bleached white mass, twelve feet thick, perfectly stratified, and with only sufficient sandy matter to form the lines of division between the strata" (E. Emmons, Geol. N. Y., Pt. IV, p. 129); they are spread upon the surface of wide extents of territory as in parts of Syria where thousands of cardiums, in the form of casts, are seen upon the road and in the fields, from which wagon-loads could be secured, though not a fragment of a shell or a piece of a hinge-tooth, for purposes of identification, are visible (O. Fraas, Aus dem Orient, Pt. II, p. 73). Similarly, in the cretaceous beds of Texas in the

[1] A Lamellibranch or bivalve shell now found living along our coast, from Georgia to the Arctic Ocean; very common from Massachusetts Bay to Labrador, occurring from low-water mark to 50 fathoms or more.

neighborhood of Neu Braunfels, Exogyras and Gryphaeas, are thickly strown over the ground, disengaged by sub-aerial weathering and surface waters from their enclosing marls (F. Roemer, Kreidebildungen von Texas, etc., p. 14, et seq.). Indeed, under some circumstances inhumation quickly destroys fossil remains from the acidic qualities of the soil, as in the neighborhood of Cumanacoa, Venezuela, S. A., as reported by Humboldt[2] (Travels in Equinoctial Regions of America, Vol. I, p. 228, Bohn's Edit.), and we think that, in many instances, fossils have undergone silicification more rapidly when brought under surface conditions where exposed to mineral waters, than they would have if covered in completely, and so removed from the influences of terrestrial circulation.[3]

Yet the word *fossil* is, of course, a distinct reference to a creature living in the past, and, as such, very properly implies entombment of some sort, and, as a fact, fossils are generally embedded in rocks or alluvial and diluvial beds, in clay banks or thinly aggregated beach sands. They may be subsequently exposed by weathering and by removal, but they indicate some sort of initial burial. Their chronological significance indicating the successive phases of animal life in geological time implies a stage-like superimposition with the earliest fossils at the bottom and the latest at the top. As, in fact, such superimposition is only partially, and then locally, perfect—no section of the earth's surface revealing a sheer and consecutive ascension of all the known strata, containing fossils—we constantly find the fossil-bearing rocks forming the surface of wide

[2] Dr. Schwernfurth, in his "Heart of Africa," mentions a soil in which the natives bury their drums, stools, etc., "to give them a permanent blackness." The vegetable acids developed by decomposition in such areas would act more or less corrosively on bone. Mr. J. Richardson tells me that in one year the entire carcass of a cat buried in rich soil had completely disappeared. On the other hand, "the fossil bones of the megatherium of the elephant and of the mastodon, which travelers have brought from S. A., have all been found in the light soil of the valleys and table-lands." Humboldt.

[3] Dr. Otto Kuntze (Nature, Vol. 19, p. 314) has insisted that his observations show that silicified trunks of trees "originate only in air; the siliceous water rises by capillary attraction in the stem, but only on the outside of the trunk does the siliceous solution become solid by drying in the air; from the outside the silicification of the wood cells enters verv slowly to the inner part."

regions, and many fossils are in a more desirable condition, as specimens for study, when surface weathering has revealed them from the matrix and brought them into a clean and significant relief, as in the case of chain corals (*Halysites catenulatus*) in the Niagara Limestone of western New York, or when the solvent of carbonated waters has left them accessible, in a friable and open ferruginous sand, as in the weathered rinds and exteriors of the siliceous limestones of the Schoharie Grit of eastern New York.

Fossils represent the hard parts of animals or such portions of their soft parts as have become replaced by mineral materials. The fleshy organic elements of an animal undergo decomposition and disappear, even though enclosed in sediments of great thickness. Water-carrying oxygen finds its way around them and slowly introduces those putrefactive changes which result in disintegration and solution. But in this process, if it is prolonged, there is a substitution of earthy matter, and an infiltration of silt will assume the form of muscular bands, retain the outline of muscular scars and blood-vessels, while a more obscure course of substitution may slowly replace the chitinous, horny, or even fleshy, appendages with silica, iron pyrite, and other mineral species, preserving them with microscopic fidelity. Many forms of animal life, like the Medusæ, Ctenophoræ, Holothurians and Worms, from their soft consistency, are necessarily almost excluded from a representation amongst fossils, being, at the best, only indicated by impressions. The occasional and unusual preservation of the fleshy parts of extinct animals in ice can hardly be regarded as a contradiction of this universal rule.

The various stages of the natural process by which organic beings become fossils[4] may be conveniently regarded as three —first, the *placement* of the object in its position preparatory to fossilization; second, its sepulture or burial, and third, the mineralogical or chemical changes by which it assumes its

[4] The word "fossil" receives a curious application by old writers, especially by the celebrated Werner, who used it to designate any mineral object extracted from the surface of the earth. Thus minerals become fossils, and he speaks of "solid fossils," and he divides them into "hard, semi-hard, soft, and very soft." Also see Pinkerton's Petrology.

permanent form or condition as a fossil. The history and discussion of these three steps form a complete history of fossilization.

The placement of an object will vary in its conditions, according to the habitat of the organism. The most general distinction will be between terrestrial (for the most part, vertebrates) or marine (for the most part, invertebrates) organisms, where the contrasted conditions involve contrasted habits and varying accidents and associations. Upon the surface of the land animals may die in numbers, either upon plains or elevated regions, or in the depths of forests, but the rapid action of decay, or the ceaseless activity of flesh-eating insects may soon dissipate their remains, even when the bony skeleton is imposing and highly developed.

The bones of buffalo remain upon the western prairies for four or five years, at least, in a recognizable condition.[5] In the region about Miles City, Montana, the buffalo abounded as late as 1880. After that time, the remorseless zeal of hunters and the avarice of trade had reduced their numbers and brought them to the verge of extinction. According to W. T. Hornaday, "over the whole of this vast area their bleaching bones lie scattered," and many of these have certainly been exposed to the weather for a period of four years, while their condition when collected warranted the expectation of their remaining sensibly unchanged many more. (See the Extermination of the American Bison, Smithsonian Report, 1887, p. 508.)

Upon the visit of the Challenger to Hard Island, of the McDonald Group in the Antarctic seas, Prof. Moseley observes that upon a sandy glacial plain there were strown "bones of the sea-elephant and sea-leopard, those of the former being most abundant. There were remains of thousands of skeletons, and I gathered a good many tusks of old males. The bones lay in curved lines, looking like tide-lines, on either side of the plain above the beaches, marking the rookeries of old

[5] It is interesting to note that Captain Stansbury observes in his Exploration and Survey of Great Salt Lake, that "carcasses of buffalo left on the open prairie are not unfrequently completely cured, or rather 'mummified' in the sun, so that they seldom exhibit any sign of decay."

times, and teaches of slaughter of the sealers. Some bones occurred far up on the plain, the elephants having, in times of security, made their lairs far from the water's edge. A few whales' *vertebræ* were also seen lying about." In dry or temperate regions, bones resist disintegration indefinitely.

In Canada, at Helena, a vast collection of thousands of buffalo skulls are seen, the enduring vestiges from a slaughter of wild buffalo surrounded by Indians at that place.

The structure of bone in exposures where the climate passes through severe extremes is an element of weakness in their preservation. Winding canals (Haversian canals) traverse the substance of hard bone and secure connection with a series of lacunae, distributed through the substance of the bone, by means of minute tubuli (canaliculi). These tubuli radiate in complex tangles from the lacunae. Sarcodic or marrow-like matter permeates these delicate passages, and, in the Haversian canals, blood corpuscles circulate. In the less dense portions of bones, as the extremities of the leg- and arm-bones, head-bones, etc., a cancellated or cellular structure forms a porous area, while long cavities (marrow-cores) filled with marrow occupy the axes of the longer bones. The decomposition of these organic contents at first weakens bone, and commences an insidious process of splitting. Subsequently the bone becomes filled with water, and the labyrinthine chain of chambers, small and large, are saturated. Cold succeeds, and from the expansion produced by frost the bone is shivered with an incalculable number of microscopic rents. These increase until cohesion is overcome and the bone falls apart.

The mammalian remains may also be placed in soft and water-saturated districts, as swamps and estuaries, lake sides and spring bottoms, whither animals have been attracted by supplies of water or because herbage and prey were equally abundant. Bones and osseous remains of vertebrates buried in rich carbonaceous soil must, to some extent, yield to the corroding action of organic acids. The important roll played by organic acids as agents of decomposition has been recognized. The numerous orders of oxygenated compounds known as acids, and which result from the accumulation of vegetable

matter in fermentable masses, have come to be regarded as most universal and persistent in their influence. It has been shown by Dr. H. C. Bolton that organic acids may be successfully employed for the detection and separation of mineral species, and Dr. A. A. Julien (Proc. A. A. A. Sci., 1879) has gathered together in a comprehensive review such inferences of their geological action as observation of their influence today permits. These acids[6] dissolve iron salts and effect disintegrating effects upon hydrated or soluble quartzes. Such active and omnipresent agencies must exert a very appreciable influence upon animal remains, and in conjunction with carbonated waters must render their preservation precarious. Terrestrial vertebrates, whose remains in the soil of forests or grassy plains would be exposed to the injurious attacks of these vegetable extracts and products, would run some considerable risk of being destroyed.[7] The *placement* of such fossils must be somewhat modified for their effectual integrity. It is true that swamps in whose periods of existence many successions of spagnum layers with the associated accumulation of related and contemporary plants have stored up great quantities of organic debris, have been the repositories of bones, and the great vertebrates, whose bones have become inhumed in their acid-laden depths, have been extracted in a reasonable state of preservation. But it is also true that these heavy bones have worked their way down through the superficial and organic layers to clay marl and sandy bottoms, where they were largely protected from the corrosive action of the humus acids which infiltration and drainage of surface waters would have partially removed.

[6] The vegetable or organic acids are mainly humic, crenic, apocrenic, with which there is an adventitious mixture of oxalic, malic, acetic and fumaric acids, which, according to Julien, are introduced "at least temporarily by the leaves, stems, etc., of most plants, many of which are rich in raphides made up of minute crystals of these acids or their salts."

[7] It is a matter of common knowledge that the bones of goats swallowed by boas undergo solution in the animal acids of their host, so that the calcareous exuviae scarcely equals a tenth part of the original mass of bones, while the so called *album graecum* in the faecas of dogs, hyaenas, etc., represents the residue of digested bones.

The Warren mastodon found at Newburgh, N. Y., in 1845, was embedded in a bed of shell-marl, above which rested a layer of red moss, over which, upon the surface, spread a "thickness of two feet of peat-bog." This specimen was in a very perfect state of preservation, revealing almost the entire skeleton of *M. giganteus*. The Cambridge mastodon was also in an admirable state of completeness, though the nature of the enveloping matrix seemed less favorable. This specimen was taken out at Hackettstown, Warren Co., N. Y., in 1845. The character of the deposit in which it was buried was distinctly organic, consisting of one foot of decayed leaves, six inches of whitish sand mixed with vegetable matter, and a yellow layer, resembling manure, offensively odorous. It would seem a reasonable inference that this large quantity of plant debris would have been unfavorable for the perfect preservation of the bones, and that the organic acids resulting from its decomposition would have aided in their removal. Of course, any protection from the air by the overlying seal of earth, clay, or water, would retard and prevent the oxydation by which the elements of cellulose become converted into acids; and bones immersed in vegetable remains under such circumstances may remain, as practically in this case, exempt from the dissolving agencies of vegetable acids. The Cohoes mastodon, found at Cohoes, N. Y., was taken from a river pothole, into which the remains had been carried, and was surmounted by vegetable debris, but had lodged upon an underlying bed of marl and comminuted shale. This skeleton was in excellent condition.

The Ward mastodon, now at the American Museum in New York City, was found at Newburgh, N. Y., in a swampy wet corner of a potato field, and was in a fair state of preservation.

Commenting upon the position of such vertebrate remains, Dr. Warren says: "In nearly all these different spots, the bones have lain at the depth of from five to ten feet below the surface. The same fact is true of deposits near Niagara, described by Sir Chas. Lyell; of those in Virginia, Long Island, the salines of Ohio, Kentucky, and most other places in the western and southern country of the United States.

"The overlying deposits are generally a foot or two of mud, the same thickness of clay, a layer of peat sometimes intervening, and below the clay shell-marl containing everywhere the relics of fresh-water testacea of existing species; some of them perfect, others decomposing. Sir Chas. Lyell, in his geological tour through the State of New York, found at Genessee, the bones of the mastodon in a bed of shell-marl below the peat, corresponding, he remarks, with the situation of the fossil elks of Ireland, generally considered to have been buried in bog-mud or peat swamps, but which, in fact, lie in a stratum of shell-marl."

It seems probable that the enormous quantities of moa bones found in the turbary area at Glenmark, New Zealand, afford some grounds for questioning the destructive influence of vegetable acids. According to Dr. Von Haast,[8] a large swampy tract in Glenmark, covering a depressed region and partaking of the mingled characters of an estuarine and lacustrine basin, contains an incredible number of the skeletons of these great birds. The bones occur here in separated patches or nests, and the impression, made by their distribution, is that of a sudden flight of groups of the birds over this marshy delta which has sunk in places beneath them, and thus entrapped them in constantly increasing numbers. Bones of twenty or thirty individuals, of all sizes and ages, and lying closely packed in spots about five or six feet in diameter, are found with no bones near them, as if, at particular points, the birds had disappeared, one after another, in the enveloping mass of vegetable débris and soft mud. Evidences here are everwhere plentiful of successive freshets by which accumulations of trees, seeds, stems and drift timber have been formed, which, with the growth of bog plants, created a deep vegetable blanket in which the moa bones are immersed. Four to seven feet of pure black peat are succeeded by two to three feet of more impure peat, in which the bird bones are more commonly laid, and under these a hard clay bottom completes the section. It

[8] Geology of Canterbury and Westland, New Zealand, J. Von Haast. See also Ann. & Mag. Nat. Hist., Aug., 1844, Rev. W. Colenso; Transac. New Zealand Inst., Vols. IV, VI, J. Von Haast.

would seem reasonable to expect a plentiful production of humic acid and its allied compounds under these circumstances, and if, as Julien asserts, these acids attack the phosphates of alkaline earths (phosphates of alumina lime and magnesia), the preservation of the moa bones appears either exceptional or contradictory.

In this connection it must be remembered that in all such vegetable infusions a considerable amount of tannin must accumulate, and its astringent action upon the gelatine of bone has a tendency to protect the bone along the interior walls of its cavities and canals. Lyell is at some pains to illustrate, in his Principles, Vol. II, pp. 508–510, the preservative properties of peat, but these illustrations relate more to its antiseptic properties for the preservation of animal tissues. Thus, in a peat-moss in the Isle of Axholm, Lincolnshire, Scotland, a body of a woman was preserved six feet below the surface; bodies of two persons in Derbyshire, England, were kept quite uninjured in moist peat, and pigs were found intact in a peaty soil near Dubuerton, Somersetshire.

There is also a possibly protective action exercised at times by the organic acids themselves when they concentrate upon a nucleus of bony fragments, precipitates of iron oxide or amorphous silica. This is done by their reduction of iron salts forming organic compounds, or by combination with silica in the dissolved silica of the infiltrating streams. The iron is liberated from solution by oxydation and the silica by decomposition, and both iron oxide and soft silica may be thus introduced into the interstices of the bone and serve as agents of induration. It is said by Von Haast that the moa bone layer at Glenmark is somewhat reddish. This may be attributed to ferruginous encrustations. Again, the action of organic acids on such material as the harder class of bone must be somewhat limited by dilution, and the constant percolation of water from surface water-courses and rains must considerably neutralize the corrosive power of the readily dissolved vegetable fluids. Again, these vegetable fluids are quite liberally employed in making defensive combinations with the mineral matter brought to them in complete solution or mechanically

suspended in the streams passing over and through marshes, swamps, bogs and deltas, and are so divested of any destructive power upon bone. And in any case, the elaboration of these acid products which we are considering would be partial or completely suspended at such depths as are usually given for the repositories of vertebrate remains. Yet, however diverted or minimized may be the action of organic acids and carbonated water upon bone, there can be little doubt that it is considerable, and an important means in many cases of imparting to them much fragility or of entirely disintegrating them.

(*To be Continued.*)

THE BACTERIAL DISEASES OF PLANTS: A CRITICAL REVIEW OF THE PRESENT STATE OF OUR KNOWLEDGE.

BY ERWIN F. SMITH.

(*Continued from p. 804*)

IV.

II. THE HYACINTH (HYACINTHUS ORIENTALIS).

(II) THE ORGANISM: *Bacillus hyacinthi* (Wakk.) Trev. (1883).

1. *Pathogenesis:*
   (A) Yes.
   (B) Yes (?). The poured plate method was not then in general use. Inoculations were made directly from diseased plants into sterile nutrient fluids, or into tubes of nutrient gelatin, and the resulting cultures may not always have been pure ones, although the writer's own experience has shown conclusively, in case of melon wilt—a somewhat similar disease—that it is often possible to obtain pure cultures in this way, if the culture

media is sterile to begin with and the necessary precautions are taken to exclude surface contaminations and air-borne germs. His experiments were, however, checked and controlled by means of poured plates, whereas Dr. Wakker had advantage of no such exact method. Nevertheless, he seems to have worked with great care, and states positively that although the bacteria were often transferred from diseased plants to the culture media, and also from one tube of media to another, the results were always the same, which could scarcely have been the case were intruding organisms present.

(C) Yes (?). Infections with artificial cultures had not been secured up to March, 1895, and do not appear to have ever been very numerous or very successful. The only experiment which seems to come properly under this head was begun March 4, 1886. The inoculations were made from a liquefied gelatin culture, the fluid being inserted into fresh cuts on the scapes of several (more than five) varieties of hyacinths. In a week all of the scapes began to dry out and soften, from the summit downward; and fifteen days later the greater part of each one was either entirely dry, or soft and flacid. An earlier effort to infect from a bouillon culture failed (Verslag, 1884).

(D) Yes; in part. On microscopic examination of the scapes mentioned under *C* it was easy to determine in them the existence of the yellow disease; but this did not extend into the bulbs. "These experiments [referring to those mentioned under I (5) as well as this one] were repeated and varied with, in general, concordant results."

Conclusion.—Pathogenic nature rendered probable.

Remarks.—As will be seen later on, this organism was imperfectly described, and any bacteriologist having opportunity to repeat and extend Dr. Wakker's experiments should by all means embrace it.

2. *Morphology:*

(1) *Shape, size, etc.*—The bacteria which Dr. Wakker regards as the cause of this disease are represented on his Plate I, Figs. 1–8 (34). They are two to four times as long as broad, with an ordinary length of about 2.5 μ. Their form is therefore more or less that of a cylinder, but with rounded ends. They are said to agree tolerably well in size and shape with *Bacterium Termo*. The organism was described as *Bacterium Hyacinthi* in 1883, but was placed under *Bacillus* by Trevisan in 1889. When these bacteria have been in a nutrient liquid for some time a certain number become longer than they were, and now measure 4 μ, while the ordinary length is only 2.5 μ. Later on, as the nutrient matters of the liquid are becoming exhausted, the bacteria diminish in size more and more, and gather into motionless groups, which often have circular outlines and which grow by the accession of new individuals, while the motile bacteria become less and less numerous. Bacteria from the dry slime were found to be only about half the ordinary size, but on placing them in nutrient fluids they resumed their normal size. Examinations were made in hanging drops of nutrient fluid.

(2) *Capsule.*—No mention of any capsule.

(3) *Flagella.*—No mention of flagella. The organism is said to be actively motile in culture fluids. Even those kept for some time in a dry state are said to have acquired motility on placing them in nutrient fluids. In the yellow, viscid slime, as taken from the plant, they are not motile; but motility begins as soon as this is diluted with a $\frac{3}{4}$ per cent. salt solution, or with a suitable nutrient fluid. "After a short time all is life and motion; the bacteria, in the form of straight but very flexible rods, are to be seen moving about actively; individuals in repose are rare. Among the undivided bacteria there are many which are in process of division, and which then show two individuals moving together; these, however, soon separate to continue an independent existence." It will probably be found that the organism is also motile in the plant in early stages of the disease, *i. e.*, before it has multiplied to such an extent as to fill the vessels. Dr. Wakker himself says: "It is evident that

though they exhibit no visible motion in the slime they cannot be entirely without motion in penetrating into the bulb."

(4) *Spores.*—The bacillus produces endospores. Their development and germination was followed with so much care that it appears worth while to give a somewhat detailed account.

"In the cultures already described [those at room temperatures] no spores were found. These had, therefore, to be sought in some other way. They were finally obtained from the liquid cultures by keeping them at a higher temperature. Drops of nutrient fluid containing the bacteria were placed in an enclosure having a uniform temperature, night and day, of 35° C., *i. e.*, at a temperature exceeding the mean temperature of the room by about 20° C. Ordinarily, at the end of ten days, spores appeared, and the characteristic agglomerations of small, motionless individuals did not appear. Subsequently it was found that a temperature of 35° C. was not absolutely necessary for the formation of the spores. In fact, during the summer, when a rather high mean temperature prevailed in the room, some cultures produced spores without artificial heat; but these were never as numerous as those formed in the tubes kept at the uniformly higher temperature of the enclosure. The spores of *Bacterium Hyacinthi* (34, pl. I, fig. 1) have that lively bluish brilliancy which is usually so characteristic of the spores of bacteria, and which is caused by the strong refraction of the light. These spores are always a little longer than broad, and form in the interior of the largest rods near the middle, although ordinarily slightly nearer one of the extremities. In consequence of their strong refrangibility it is difficult to decide with certainty whether the rod is swollen around them, as has been indicated for several similar species. This swelling, if it exists, must be very slight, since the spore is not thicker than the bacterium itself. Besides the rods with ripe spores, there are, ordinarily, a great number engaged in forming spores, and these still move about in a lively manner. On the contrary, when the spores have reached full development, the rods which contain them remain motionless and their wall soon disappears, so that the spores become completely free. In this state they

are about 1 μ in length, while their breadth does not exceed $\frac{1}{2}$ or $\frac{2}{3}$ of this size. Each rod produces only one spore. If these spores are allowed to dry on the glass where they have been formed, they may be kept for a long time, and subsequently on placing them in a nutrient liquid the development of new bacteria may be observed. At the time of germination, which is hastened the same as sporogenesis, by an increase of temperature, the spore begins to swell and its cylindric form changes to an ellipsoid. The strongly refractive power is also gradually lost, the middle of the spore first becoming dull while the brilliant gleam still persists more or less at the two extremities (34, pl. I, figs. 2 and 3). Here we have the condition which must be considered as the commencement of germination. The wall is split into two portions, which remain united at one side. The central part of the spore from which the refringence has entirely disappeared, is the place where the two halves of the spore open one from the other, and here a baculiform body of slight refringence was observed pushing out (pl. I, figs. 4 and 5). During the growth of this body the sheen also diminishes very greatly at the two extremities of the spore, and soon there is a state which cannot be indicated better than by likening the germinating spore to a hammer, the two portions of the wall of the spore representing the head while the handle is formed by the rod which has issued from the spore (fig. 6). Often after a longer or shorter time, the rod, one end of which is squeezed between the two parts of the wall of the spore, begins an oscillatory movement, and thus succeeds in freeing itself, whereupon it moves through the liquid in the manner common to bacteria, the empty wall being left behind (fig. 6c). In other cases, after escaping from the spore, the young bacterium remain motionless in front of the empty wall for a long time before swimming away. Finally, the rod sometimes drags the empty wall after it (fig. 8). In all cases the rod which has escaped is an ordinary bacterium which soon divides in the manner already described."

The author never found spores in the living hyacinth. This, he says, accords with de Bary's observation on *Bacillus anthracis*, he having never found spores in the living animal.

This absence of spores in the living plant is also in harmony with the fact that in the nutrient liquid the formation of spores begins only when the alimentary substances are exhausted. This, naturally, is never the case in the living bulb. It is not impossible, however, that when diseased bulbs have been entirely destroyed spores may form in the remaining mass if the temperature is favorable. An effort was made to prove that these spores were actually developed from the hyacinth bacillus by allowing a drop of fluid containing them to dry on a slide for some time, and then placing that part of the slide bearing the dry spores in contact with the fresh cut surface of a bulb. In three weeks the yellow disease was discovered in the vessels of the bulb, and it was at once apparent that it had already been developing in these for some time. This experiment was repeated several times, and always with the same result. This, indeed, is not full proof; but when old cultures are used very few vegetative rods are left, and the infection is believed to have resulted principally from the germination of the spores in the sticky fluid that oozes from the cut scales, the bacteria finding their way from this into the vessels.

(5) *Zooglœa.*—No special mention of zooglœa. Possibly the more or less circular or globular groups of motionless rods which commonly appeared in the cultures as they became exhausted are to be regarded as such.

(6) *Involution forms.*—No mention of any involution forms.

3. *Biology.*

(1) *Stains.*—This organisms stains very readily in the most diverse analin colors. The author made a variety of experiments to determine the best method of staining the bacteria in place in the tissues. He obtained the best results with analin browns, especially phenyline brown (Bismark brown), but states that many other colors may be used, *e. g.*, eosine, methyl violet, analin yellows and picric acid. The yellow stains have the special advantage of giving to the preparation almost exactly its natural color. To stain in place, sections made from alcoholic material should be put into a saturated alcoholic solution of the analin brown, left for a few minutes, and then transferred to strong alcohol containing an

extremely small quantity of hydrochloric acid. In this the color rapidly disappears, especially if the fluid is stirred with a glass rod. At the end of a very short time, the length of which varies in different cases, certain parts will be seen to have preserved their color, if the disease is present, while the rest of the section has already bleached. The sections must now be removed immediately to a dish of pure, anhydrous spirits of turpentine, in which they are left until thoroughly penetrated by the liquid. They may then be examined directly or first mounted in Canada balsam, after which they may be kept indefinitely. When the work has been well done the sections will be brown in those parts which contain the bacteria and which were originally yellow, while in all other parts they are colorless.

(2) *Gelatin.*—The culture media was made by adding to water containing glucose and a little meat extract, enough gelatin to give a solid, clear yellow, perfectly transparent mass at ordinary temperatures. This was sterilized by heating from time to time to 100° C. It was then carefully pipetted into tubes which were plugged with cotton, and re-sterilized by heating every day to 100° C., for some days. Pipettes, tubes and cotton plugs had previously been heated to 140° C. Tubes prepared in this way were unplugged, infected with bacteria taken from a diseased bulb (the transfer being made by means of a platinum wire previously heated to redness), quickly closed, and then left at the ordinary room temperature. The organism makes a good growth on gelatin. The gelatin is readily and completely liquefied.

" *Experiment of June 12, 1885.*—The above described operations were made this day, and two days later I saw in all the tubes the gelatin liquefy under the influence of the bacteria. Examination showed that the part not yet liquefied also contained bacteria, so that the latter must first penetrate into the gelatin and then cause its liquefaction. The formation, in the part of the gelatin which is still solid, of white globules consisting entirely of bacteria, served to make this fact very apparent. Bubbles of gas which can only arise from the action of these organisms also developed in it continually. After a short

time the whole mass was liquefied, and the bacteria were found at the bottom of the tube as a thin whitish layer. The liquid is then a clear brown, darker than the original gelatin. The contents is almost odorless.

"This experiment was repeated very often, and always gave the same result, only in subsequent experiments, it happened sometimes that the white globules did not appear. This, however, is not surprising, since I then employed a mixture (glucose, extract of meat, gelatin) of slightly different composition, and since, moreover, the temperature was not always the same. On peut naturellement infecter aussi quelques tubes au moyen de Bactéries prises dans d'autres tubes; cela n'a jamais rien changé aux résultats."

No gelatin roll or plate cultures were made, and the behavior of the organism in stab and streak cultures is not carefully described.

(3) *Agar.*—No account of any experiments on agar media.

(4) *Potato, etc.*—Nothing mentioned.

(5) *Animal Fluids.*—The first artificial medium was made by adding a little meat extract and grape sugar to a decoction of meat which had been kept for some time in spirits, and was freed from the latter by washing and boiling in distilled water. It was then boiled for an hour in an additional quantity of distilled water and the sugar and meat extract added. It remained clear for ten days, was then reboiled, cooled quickly, and a small quantity of the yellow slime introduced, the greatest care being used throughout to avoid contaminations. The second day this fluid became distinctly clouded, and this clouding increased for four days, and then remained the same. The inoculations that failed were from this culture. The slime used to infect this culture came from a single vascular bundle of a freshly cut bulb. It was scraped off on a flamed cover-glass, which was then thrown into the fluid. The organism also grew well in a solution of meat extract to which glucose had been added. This was the fluid culture medium ordinarily employed, and there is no mention of any other. The exact composition of the medium is not given.

(6) *Vegetable Juices.*—None mentioned.

(7) *Salt Solutions and other Synthetic Media.*—No mention of any; but since the organism is not strictly parasitic it is inferred that it can grow and maintain itself for a long time in a variety of organic substances.

(8) *Relation to Free Oxygen.*—The organism is aerobic and probably also facultative anaerobic, although no mention is made of any experiments to determine this point.

(9) *Reducing and Oxidizing Power.*—Peptonizes gelatin.

(10) *Fermentation Products and other Results of Growth:*

(a) *Gas Production.*—Organism produces gas in meat extract gelatin containing grape sugar. Kind of gas not determined.

(b) *Formation of Acids.*—No statement.

(c) *Production of Alkali.*—No statement.

(d) *Formation of Pigment.*—In the vessels of the plant the organism produces a bright yellow color, which is soluble in glycerin, but insoluble in water and alcohol. This pigment became darker on drying. The dextrose, meat extract gelatin became darker colored (clear brown) after liquefaction.

(e) *Development of Odors.*—The organism produces little or no odor either in the plant or in the artificial cultures. This absence of odor may be used to distinguish the disease from other hyacinth diseases, some of which are very malodorous.

(f) *Enzymes.*—Evidently not studied. Organism produces at least two; one capable of peptonizing gelatin, and another which dissolves the cellulose of the hyacinth.

(g) *Other Products.*—None mentioned.

(11) *Effect of Dessication.*—The organism can be kept for a long time in a dry state without dying, *e. g.*, on a glass plate. It shrinks to about one-half normal size, but on placing again in suitable fluids it recovers its former size and makes a new growth. One of these hanging drop cultures was begun in a somewhat different way. The bacterial slime was not taken directly from a bulb but from a glass plate on which it had been placed and dried long before. The slime and the nutrient fluid were then mixed in the same manner as before; but instead of rods 2.5 μ long, the bacteria were now smaller. Moreover, at first they were distributed through the liquid passively,

and a longer time passed than in the other cultures before their own movement appeared. Nevertheless, after some hours, it began, and first as a simple rotation. At the same time it was determined that the dried bacteria had been reduced to about one-half the ordinary size. But the following day they had resumed the ordinary size, and then also showed the characteristic backward and forward movement. From this point on the culture presented the identical phenomena described above. This shows that the bacteria of the *maladie du jaune* can live for a long time in a dry state, and that on drying they are reduced to dimensions comparable to those which they assume in a liquid in which the alimentary substances are becoming exhausted. I infer that this dry mucilage did not contain spores.

(12) *Thermal Relations:*

(a) *Maximum for Growth.*—Not determined.

(b) *Optimum for Growth.*—Not determined. The organism grows at living-room temperatures, and also in the thermostat at 35° C.

(c) *Minimum for Growth.*—Not determined. The natural progress of the disease in the hyacinth fields appears to be slow, and probably low temperatures may have something to do with this.

(d) *Death Point.*—Not determined.

(13) *Relation to Light.*—Not determined.

(14) *Vitality on Various Media.*—Seems to be capable of living for a considerable period in various media.

(15) *Effect on Growth of Reaction of Medium (acid, neutral, alkaline.*—No statement.

(16) *Sensitiveness to Antiseptics and Germicides.*—No statement.

(17) *Other Host Plants.*—No mention of any. Some speculation as to origin of the disease, but no facts.

(18) *Effect upon Animals.*—No statement. Probably not tried.

(III) Economic aspects:

(1) *Losses.*—No statement as to the extent of damage done by this disease. The disease is spoken of in one place as the chief subject of his investigations, and in another place the organism is called a "dangerous parasite."

(2) *Natural Methods of Infection.*—Little that is definite can be gathered from Dr. Wakker's writings. The sticky slime which oozes from rifts in the affected leaves is highly infectious, adheres to whatever it touches, retains its vitality for some time, and is readily borne about on light objects. He discusses the possibility of the germs entering through the blossoms, and considers that wounds are more likely sources of infection, because the attacked blossoms would fall off quickly and carry the germs with them. It probably enters the plant through wounds made by man or animals. Dr. Wakker thinks it especially likely to enter through wounds of the scape made in cutting the flower, or through injuries done to the young scales by pulling leaves, or by cutting healthy bulbs with infected knives in process of making incisions in the bulb, or of separating the scales for purposes of reproduction. It is evident, however, from the fact that the greater number of the plants are first attacked at the tip of the leaf, that some other unknown method of infection is the more common one. Dr. W. thinks the infection often takes place very early in the spring and generally through the air, the sticky bacterial exudate from the leaves, etc., being carried to sound plants by wind and rain, or by flies and other insects which frequent the hyacinth fields on warm days (Verslag, 1883). For various reasons Dr. Wakker thinks that the parasite may sometimes enter through the uninjured leaf, *i. e.*, through the stomata, but does not appear to have induced the disease in this way. Wounds are always moist, and the bacterium finds food ready for its use in the dead cells of the wound, whereas if it enters through the stomata it must make its own food from the start. The stomata are also very small, and infection through the uninjured leaf surface is probably uncommon.

(3) *Conditions Favoring the Spread of the Disease.*—Dr. Wakker states that the spread of the disease is favored by wet weather, and that dry weather and continuous sunshine are the best preventives. If the much lessened prevalence of the disease in 1883 as compared with 1882 is to be attributed in part to the precautionary measures taken, it is not less certain that the frequent rains of 1882 did great injury to the plants in this

particular. "In 1883 innumerable were the cases in which I observed that the descending stripe on the leaves was dried out and stopped, so that the bulb was not attacked." The rapidity of the infection depends largely on the temperature, the dampness in the surrounding air, and on the amount of water in the plant itself. The location of the wound might also make a difference.

(4) *Methods of Prevention.*—An inquiry among the growers elicited the statement that there is a great difference in susceptibility. This Dr. W. thinks cannot be denied. Some varieties are very subject; others, in the same beds or gardens, have not been known to be attacked. Many varieties formerly held to be exempt from the disease are now known to be subject; but some remain which have never yet shown the yellow disease, and this cannot be ascribed to mere accident; on the contrary, it can be explained only by assuming that predisposition or readiness to be attacked here plays a prominent part (Verslag, 1883). Anatomically, so far as known, all are alike. Lists of "very susceptible," "less susceptible" and "not susceptible" varieties are given, from which it would appear that single varieties are more susceptible than double ones, and the exemption of the latter is not due to their lesser number. All of the double red varieties and most of the other double sorts are exempt, or but little subject to attack. These lists are based on statements furnished by only seven growers, but include many varieties (Verslag, 1885). Of thirteen varieties said to be very susceptible by several or most of these seven growers only one is double, la Tour d'Auvergne. On this account, difference in receptivity is suggested as a means of combatting the disease. New varieties must not be originated from susceptible ones. Seedlings should be derived from hardy sorts, and by artificial fecundation, the pollen of susceptible varieties being excluded. Otherwise, through the agency of insects, the resulting cross may prove susceptible. The law of heredity is shown still more rigorously in non-sexual reproduction. It is best, therefore, to discard sensitive sorts and try to obtain new ones which are more robust.

In the division of bulbs for propagation the greatest care should be taken never to cut a healthy bulb with a knife which

has been in contact with a diseased plant, at least not until it has been disinfected.

There is another point to which the author desires to call special attention, viz., to the removal of leaves which begin to show signs of the disease at the tip. On May 20, 1883, the diseased leaves were entirely cut away from seventeen hyacinth plants. On September 26th, sixteen of these bulbs were entirely sound, although rather small. The other bulb was entirely decayed; but from what cause, it was no longer possible to determine. Planted in pots these sixteen bulbs blossomed in April, 1884. The following June they were dug up once more, and on cutting them open all were found to be sound. This experiment was tried on many other bulbs, and always with the same success. It was also tried by several horticulturists in their fields with results entirely confirmatory. It is, therefore, certain that the bulb can be preserved by the judicious removal of diseased leaves.

Since the bacteria have always penetrated much further into the leaf than is to be seen with the naked eye, the whole leaf should be removed even when only slightly attacked. The frequent complaint that cutting off the diseased parts does no good, shows that not enough attention has been paid to this. Of course, when the bulb is already infected, cutting off the leaves amounts to nothing (31).

Finally, it goes without saying that the debris of diseased hyacinths should not be left in the field or near it, as one might be tempted to do on account of its value for manurial purposes. All such debris should be thrown into a deep ditch and disinfected with quick lime.

Remark.—Considering the time when this piece of work was done, it is remarkably good, and in all of the papers cited the internal evidence indicates a careful, conscientious, brilliant investigator. There can be no doubt that the disease is due to a bacterial parasite; but to complete the proof that the disease is due to the specified organism it should be obtained by infections with pure cultures obtained from single colonies. The organism thus isolated should also be studied under a wider range of artificial conditions than were employed. Indeed, excluding the pathogenic test, it is more than doubtful if the organism could be identified from the description.

EDITOR'S TABLE.

The rules of zoölogical nomenclature formulated by Strickland and adopted by the British Association for the Advancement of Science, in 1842 have been observed by most zoölogists ever since. They are eminently fair, and conducive to the best interests of science, and in broad contrast in certain details to some individual opinions which have been promulgated in recent years.

There is a minor point in which it seems to us that the Stricklandian rule might be amended, and we recommend it to the consideration of the international zoological congress committee on nomenclature. This is the question of the presence or absence of the annectant i in the root of proper names of the second declension—to which most proper names belong. Shall we write Boggsus or Boggsius: Keenus or Keenius; Levius or Leviius, etc.? The British rule provides (Proceeds. Brit. Ass. Adv. Sci., 1842, p. 115) that after a consonant the termination of proper names shall be *us* gen. *i*; while after a vowel the i shall be inserted, so that we have *ius*, gen. *ii*.

This rule, however, does not exactly conform to the usage of the Romans, which was not regular. Thus they wrote Catullus, Catulli, but Sallustius, Sallustii; Corvus, Corvi; Horatius, Horatii, etc. After vowels the custom also varied, but generally the i was omitted since it is unnecessary on the score of euphony. The Romans were, as well known, guided by euphony in the matter, hence the irregularity. It is evident that we should be guided by the same principle, but that in doing so we should endeavor to formulate a rule which shall have no exceptions. Naturalists cannot be expected to remember exceptions in a subsidiary matter like nomenclature.

The reversal of the Stricklandian rule would apparently accord best with the spirit of Latin word composition. That is, an i should be inserted after the root of all proper names of the second declension which ends in a consonant, and no i should be inserted where the root terminates in a vowel. Names of the first class never sound badly with the i, while most of them,—notably those whose roots end in labials and dentals, do sound badly. A vowel precedes the us euphoniously. Thus Dana, Danaus; Perrine, Perrinius; Secchi, Secchius; Gaudry, Gaudrius. Those ending in o and u are not of the second declension, unless made so by the addition of the consonant v, as Sello, Sellovius; Yarrow, Yarrovius.

Our much esteemed contemporary, *Natural Science*, had, in a recent number, three short articles devoted to the denunciation of the describing of species in biology; calling the practice in one of them "a most unprofitable" kind of work. Now comes our equally esteemed colleague, *The Revue Scientifique* (1896, p. 440), and remarks as follows, anent of the recent work of Messrs. C. H. Merriam and E. S. Miller on North American Mammalia: "But really is there not more interesting work to be done on the fauna of the United States? This work, which consists in enumerating and describing species, which is within reach of the most mediocre intelligence, this fastidious care which should be left to those who are not capable of ideas, is this the only work which tempts American Zoologists? Is there not other occupation for their scientific activity? Cannot Mr. Hart Merriam stimulate work of a biologic character?"

We regard the expressions above quoted as an indication of a mild form of megalomania which is not unfrequently found among the users of mechanical appliances in the biological laboratory. The most intelligent cultivators of these important branches of biologic research are, however, well aware that the exact determination of species is fully equal in importance to their own pursuit, for the following reasons, among others. If we regard biology to consist of two branches, evolution and physiology, we define evolution, with Darwin, as the origin of *species*. For physiology the question of species is not so important. Species are, however, what the labors of the ages have produced, and it is necessary to know them in order to pursue any branch of evolution (as embryology or paleontology) intelligently. The work of the embryologist and paleontologist who does not know the species whose origin he seeks to explain is greatly lacking in precision. Linnæus states that the tyro knows the higher divisions, but only the expert knows species. We also especially deny that the discrimination and description of species is within reach of the most mediocre intelligence. On the contrary, no kind of work in biology imposes as much on all the mental faculties which are used in scientific work. Those who have not attempted it have little idea what is involved in a diagnosis or an analytical key. Finally, as regards the mammalogic work of Messrs. Merriam and Miller, we consider it of the utmost importance. They are pointing out the results of the evolution of Mammalian life in North America, which it is the business of the embryologist and the paleontologist to explain. And in this field the work of Messrs. Merriam and Miller is the best that has ever been done in any country.

The most important result of the Nansen Arctic exploration which has been so far given to the public is the discovery that the ocean has the great depth of nearly 2000 fathoms north of Franz Joseph's Land. This is the average of the oceanic depths, and the knowledge of its extension to the point nearest the pole yet attained, is a distinct gain. It dispels the idea that the pole can be reached overland from the side of Siberia, and shows that the nearest land approach, as suggested by Peary, is by way of Greenland. While this discovery does not destroy the hypothesis that land exists near the pole, it weakens it. The theory will not become extinct until the northern rendezvous of high arctic migratory birds has been discovered. The remarkable discovery of a territory free from glaciers and covered with vegetation in Grinnell Land, and along the north coast of Greenland, by the Greeley Expedition, opens up interesting possibilities, and must stimulate further search. American citizens have had an honorable share in these in the past, and it is to be hoped that they will continue to attack the problem until it is solved.

RECENT LITERATURE.

The Earth and Its Story by A. Heilprin[1] fills a want long felt by teachers of elementary geology. It is a well illustrated little volume which presents "briefly, forcibly and possibly in a more popular form than in most books of a similar nature, the general facts of geology." It covers the field that it is intended to cover in a remarkably satisfactory manner. The facts of the science are given in sufficient detail to impress the student with the notion that the generalizations based upon them are built upon a secure foundation. Comparatively slight stress is laid upon these facts, the greater emphasis being placed on the general truths to which they lead. The book is interesting. It is well written; the language is simple and the thoughts are very clearly expressed. Only the most important conclusions of geology are mentioned, and where the views expressed are not accepted by all geologists, the author does not hesitate to mention the fact.

A prominent feature of the book are the illustrations. These are mainly reproductions of photographs, many of them entirely new. A

[1] Angelo Heilprin: The Earth and Its Story, a First Book of Geology. Boston, Silver, Burdett and Co., 1896. Pp. 267 and Plates 64.

few are blurred, but the majority are sufficiently full of detail to be of great aid to the reader. Two might well have been spared without injuring the value of the volume in the least—the map of Mammoth Cave (Plate 22, Fig. 2) on which the lettering is so small as to be read with difficulty, and the plate supposed to show the forms of crystals.

Criticism might well be urged against the table of geological "epochs and formations," since the terms 'primary' and 'secondary' are used in conjunction with Paleozoic and Mesozoic, as though they were in as frequent use as the latter, and the term 'tertiary' is used as synonymous with Cainozoic. 'Azoic' is also used as the time term corresponding to the formation term Archean, in spite of the fact that the presence of fossils in the Archean rocks (Huronian and Laurentian) is not positively denied. Finally the term Algonkian has no place in the table. While, of course, it is permitted to the author to decline to accept this term as having a definite significance, it is at the same time unfortunate for his readers that they are not made familiar with it, if only as an aid toward the understanding of the handsome geological maps of the U. S. Geological Survey.

There are 19 chapters in the book. The first three treat of rocks, their formation and decay, the fourth of mountains, the next two of glaciers, the seventh of underground waters, the eighth of the relation between sea and land, the ninth of the interior of the earth, the tenth and eleventh of volcanoes, the twelfth of coral islands, the next three of fossils—their organization and their teachings, the sixteenth of land surfaces, and the last three of metals, minerals, building stones, etc.

No one need hesitate for an instant in recommending this little volume for use in our high schools and academies. It is by far the best thing of its kind that has yet appeared upon the market.—W. S. B.

A Handbook of Rocks, for use without the Microscope by Dr. J. F. Kemp[2] is a very welcome visitor to the desk of the teacher of geology. There has long been needed a little treatise on lithology which might be used as an introduction to the study of rocks and as a text-book for the use of those students in geology who have no intention of taking up the subject as a specialty. The volume before us fills this need completely. It is an excellent little book, as full of detail as is desirable for a book of its character and as accurate as is possible in one of its size. Each of the main families of rocks is well characterized

[2] J. F. Kemp: A Handbook of Rocks, for use without the Microscope with a glossary of names of Rocks and other Lithological Terms. Printed for the author. New York, 1896, pp. vii, 176. Price in lots of ten copies $1.00 each.

in a few discriminating sentences, analyses of many varieties are given and the structures and textures of all are well described. One of the most commendable features of the volume is the use of only the more important rock-names in the body of the text—the less important ones being relegated to a very comprehensive glossary which forms a convenient appendix to the book. In this respect, as in some others, the volume under review is very much more satisfactory to the untechnical reader than the other volumes of similar character that have recently come under our notice.

The work opens with a description of the rock-forming minerals and a discussion of the principles of rock classification. Following this are the descriptions of the rocks. These are divided into Igneous, Aqueous (including Eolian) and Metamorphic rocks. Each class is divided into groups according to chemical composition, and each group is further subdivided according to texture. The classification is an eminently practical one, and at the same time it can give no offense to the micoscopical lithologist.

In the discussion of the rock-types each chapter begins with a list of analyses; this is followed by comments upon them. Then comes a description of varieties, a statement of relationships, a paragraph on geological occurrence, one on alterations and one on distribution. In that portion of the book that deals with the igneous rocks the glasses are first taken up, then the porphyritic varieties and, finally, the granitic ones. The aqueous rocks are grouped as mechanical sediments, limestones, organic remains and precipitates from solution. Of the metamorphic rocks two great classes are recognized, viz., those produced by contact action and those produced by regional metamorphism.

The above outline of the contents of the volume is very brief, but it is sufficiently full to indicate that the author has covered well the field that such a treatise as this one should cover. This book should find a wide sale among engineers as well as among all teachers who introduce into their courses on geology a description of rocks. It is a far more valuable synopsis of the characteristics of rock types to place in the hands of geological students than the synopses contained in the large text books on geology.—W. S. B.

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General Notes.

MINERALOGY AND CRYSTALLOGRAPHY.[1]

Etched Figures on Some Minerals.—Traube[2] brings into deserved prominence the value of the method of etching, and gives the results of an extended series of experiments on the etched figures of a number of minerals. He mentions especially those cases in which the etched figures indicate a higher symmetry than that occasionally shown by the geometrical development of the crystal form. He evidently lays more stress on the etched figures of crystals than on the occasional growth of planes corresponding with a lower symmetry. K F and K F, H F are mentioned as giving good results in many cases where the problem is to etch one of the more refractory silicates, and a caution is given that care must be taken in the use of such powerful reagents.

On cuprite etched figures were produced by H_2SO_4, HCl, HNO_3 and KOH, dilute HNO_3 giving the sharpest figures. The etching indicates a holohedral regular symmetry, notwithstanding that Miers has observed faces of the form (986) in a position suggesting gyroidal hemihedrism.

Phosgenite gives sharp figures with hydrochloric, sulphuric, nitric and acetic acids, also with the caustic alkalies, all pointing toward holo-

[1] Edited by Prof. A. C. Gill, Cornell University, Ithaca, N. Y.

[2] Neues Jahrb. B. B. X, pp. 454–469, 1896.

hedral symmetry. The forms developed on some crystals from Monte Poni had suggested trapezohedral hemihedrism in the tetragonal system.

Wulfenite from several localities has been reported to be hemimorphic, on the strength of the polar development of the crystal form, but neither the etched figures nor the pyroelectric behavior of the crystals bears this out. Both wulfenite and scheelite act alike in these latter respects and appear to be pyramidal hemihedral, without difference in the two directions of the vertical axis.

Chalcolite, disthene, tourmaline, vesuvianite, dioptase, willemite, nepheline, beryl, adularia and some of the triclinic feldspars were also etched, with the result of confirming the higher symmetry in each case where doubt could exist. Nepheline, as already established by Baumhauer, belongs to the pyramidal hexagonal class of Groth (1st hemimorphic tetartohedral division of the hexagonal system, Liebisch).

Pollucite, Mangano-columbite and Microlite from Rumford, Maine.—These minerals were discovered in pegmatite associated with quartz, feldspar, muscovite, tourmaline, lepidolite, spodumene, amblygonite, beryl, cassiterite and columbite. They are described by H. W. Foote.[3] The pollucite, though rare, occurs in rather large masses difficultly distinguishable from white quartz. The analysis proves the mineral to be chemically identical with that from Hebron, Maine, and seems to sustain the view of Wells that the formula is $H_2Cs_4\ Al_4(SiO_3)_9$.

The Mangano-tantalite is in the form of dark reddish-brown crystals resembling rutile. A qualitative analysis revealed the presence of Mn, Ta and Ni. The specific gravity, 6.44, would indicate that the last two elements are present in about equal proportions. The form differs somewhat from columbite, as shown among other facts adduced, by the axial ratios.

	Columbite.	Mangano-columbite.
a : b : c =	.8285 : 1 : .8898	.8359 : 1 : .8817

Microlite in beautiful honey-yellow crystals 2 mm. in diameter have a specific gravity of 5.17. The prevailing form is the octahedron, modified by the dodecahedron and sometimes by (113).

Epidote and its Optical Properties.—The peculiar appearance of a gray epidote from Huntington, Mass., led to its detailed investigation by Forbes.[4] The light color is evidently due to the low percent-

[3] Am. Jour. Sci., CLI, pp. 457-461, June, 1896.
[4] Am. Jour. Sci., CLI, pp. 26-30, 1896.

age of iron, as shown by the subjoined mean of two closely agreeing analyses.

SiO_2	37.99
Al_2O_3	29.53
Fe_2O_3	5.67
FeO	.53
MnO	.21
CaO	23.85
H_2O	2.04
	99.82

This corresponds with the accepted epidote formula. Some of the angles vary quite considerably from those given by Kokscharow—possibly due in one or two cases to the striated character of the faces.

The optical properties are unusual. The axis of greatest optical elasticity lies in the *obtuse* angle β, making an angle of $1° 51'$ to $2° 47'$ with the vertical axis, according to the nature of the light used. The optical sign is positive—an unusual thing for epidote. The indices are $\alpha = 1.714$, $\beta = 1.716$, and $\gamma = 1.724$. The double refraction is thus .010, the least value known for the mineral. The optical angle over a, $90° 32'$, is exceptionally large. A comparison of the data at hand seems to show that with increasing percentage of iron the double refraction becomes stronger, the index of refraction increases, while the optical angle (over a) grows larger, and when it passes 90° the crystals become optically negative.

Miscellaneous Notes.—Leiss[5] gives details concerning several new models of optical instruments as manufactured by Fuess of Steglitz, near Berlin. The most important of these are a petrographical microscope, a theodolite-goniometer, an optical angle instrument, and a number of devices for universal motion.—Viola[6] shows the application of the quaternion method to the discussion of crystal symmetry, and arrives at results concordant with those of Fedorow, Schönflies and others.—Schwarzmann[7] describes a scale for reading directly with approximate accuracy the apparent optical angle 2E, without the labor of calculating it by Mallard's formula.—Crystallographers will be much interested in the results obtained by Rinne[8] in certain experi-

[5] Neues Jahrb., B.B. X, pp. 179–195; also pp. 412–439, 1895.

[6] Neues Jahrb., B.B. X, pp. 495–532, 1896.

[7] Neues Jahrb., 1896, Vol. I, pp. 52–56.

[8] Neues Jahrb., 1896, Vol. I, pp. 139–148.

ments on heulandite. Anhydrous H_2SO_4 abstracts 2 H_2O from the molecule Ca Al_2 Si_6O_{16} + 6 H_2O leaving Ca Al_2 Si_6O_{16} + 4 H_2O. The latter compound is optically quite different from the original heulandite, having, for example, a much higher double refraction and a different plane of the optical axes. The change may be watched under a microscope, and takes place faster in some crystallographic directions than in others. Dilute sulphuric or hydrochloric acid gives a pseudomorph, which, after heating, is composed of almost pure SiO_2 (only 1.33 per cent bases). It has a specific gravity of 2.143, is optically biaxial with a small angle, and has weak double refraction. It is regarded as a new modification of SiO_2, probably like Scacchi's "granulin."

In continuation of his studies on Algerian minerals, Gentil[9] mentions with more or less detail calamine, smithsonite, sphalerite, calcite, galena, cerussite, limonite and barite from a number of zinc mines. Ilvaite and bustamite from Cape Boujaroun are also studied somewhat at length.—Dufet[10] publishes the results of a crystallographic study of four modifications of indophenol, also of several complicated organic and inorganic compounds which are not at all related to one another. —Lacroix[11] describes the microscopical characters of a number of compact or earthy minerals. They are not amorphous, as they appear to the naked eye, but are all micaceous and crystalline in ultimate structure.—Termier[12] reports seven new forms, and a large number of rare ones, on a quartz crystal discovered on a block of gneiss in the lateral moraine of the lower Grindelwald glacier. The new forms are

$$\bar{7}.7.0.4$$
$$32.\overline{15}.\overline{17}.62$$
$$\overline{11}.\overline{25}.36.0$$
$$10.\overline{35}.25.20$$
$$\bar{1}.4.5.\bar{2}$$
$$\overline{17}.\bar{4}.21.\bar{9}$$
$$\bar{3}.\bar{4}.7.\bar{7}$$

The explanation of these rare faces is sought in the deposition of calcite on the quartz, followed by the formation of "temporary limiting" faces as the crystal again grows, and, finally, the solution of the out-

[9] Bull. Soc. Fr. Min., XVIII, pp. 399–414, 1895.
[10] Bull. Soc. Fr. Min., XVIII, pp. 414–426, 1895.
[11] Bull. Soc. Fr. Min., XVIII, pp. 426–430, 1895.
[12] Bull. Soc. Fr. Min., XVIII, pp. 443–457, 1895.

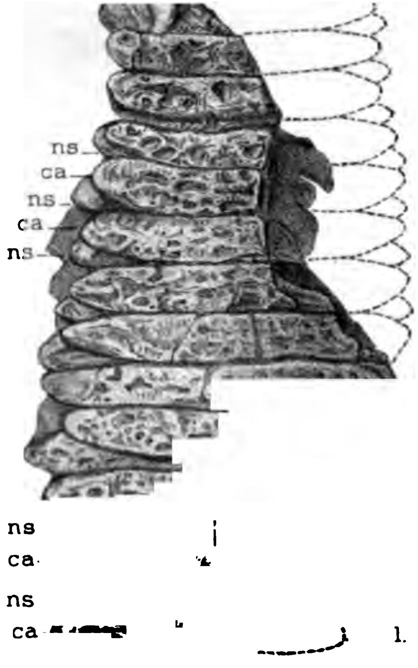
side of the quartz, thus exposing again the unusual faces. Some of the calcite layers are still present in the specimen.

The new mineral lawsonite is more fully described by Ransome and Palache[13] than was the case in the original paper by Ransome. The formula is H_4 Ca Al_2 Si_2O_{10}.—Walker[14] finds that the sperrylite from the Sudbury district probably occurs originally included in chalcopyrite. The new face π (10.5.2) was observed. The suggestion is made that Os and Ir occur replacing Pt in sperrylite, and an analysis of the products of the Murray mine, showing the presence of these elements, is given. (If, as this analysis would indicate, the two elements osmium and iridium are present in an amount equal to one quarter that of the platinum, it is difficult to suppose that they exist in the sperrylite, since Wells states specifically that he found no iridium in the sperrylite analyzed by him).—Adams and Harrington[15] describe a new alkali-hornblende chemically near an orthosilicate, and a titaniferous andradite from the nepheline-syenite from Dungannon, Hastings Co., Ontario.—Merrill[16] notes an occurrence of free gold in a black mica granite from Sonora, Mexico, apparently as an original constituent of the rock.—Crocoite crystals from Mt. Dundas, on the west coast of Tasmania, measured and figured by Palache[17] present, in addition to the twelve known forms the new, though doubtful, prism (10.3.0).

GEOLOGY AND PALEONTOLOGY.

Permian Land Vertebrata with Carapaces.—In the NATURALIST for 1895 (November) I described under the name of Dissorhophus a new genus of probably Ganocephalous Stegocephalia with an armadillo-like carapace. In the Proceedings of the American Philosoical Society for the same year and month I described a new family of Cotylosaurian Reptiles protected by a similar structure. These constitute the only forms of land vertebrates so constructed known from the paleozoic formations. The nearest approach to it previously known from the Permian is seen in the genus Zatrachys, where the

[13] Zeitschr. f. Kryst., XXV, pp. 531-537, 1895.
[14] Zeitschr. f. Kryst., XXV, pp. 561-564, 1895.
[15] Amer. Jour. Sci., CLI, pp. 210-218, 1896.
[16] Am. Jour. Sci., CLI, pp. 309-311, 1896.
[17] Am. Jour. Sci., CLI, pp. 389-390, 1896.



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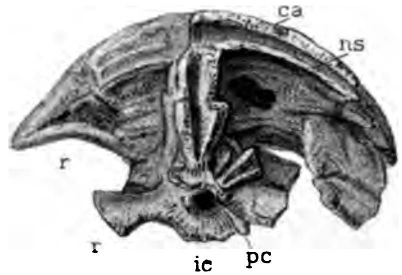

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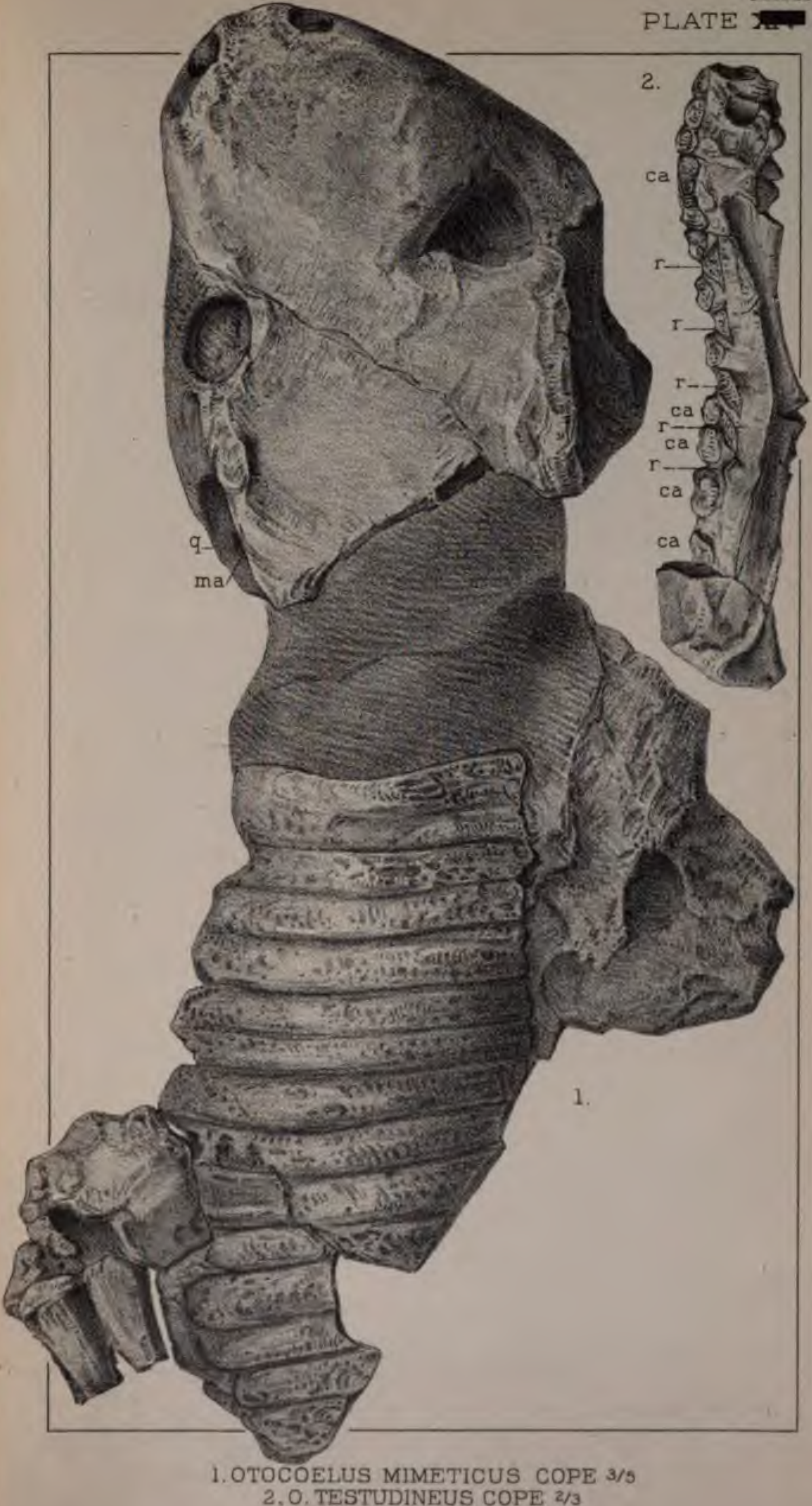
DISSORHOPHUS ARTICULATUS COPE 5/6

XXII
PLATE [redacted]



1. OTOCOELUS MIMETICUS COPE 3/5
2. O. TESTUDINEUS COPE 2/3

species *Z. apicalis* has the apex of the spines dilated and sculptured on the superior or external surface, indicating the presence of a row of osseous shields covered by epidermis only, extending along the middle dorsal line. In the Trias, two such types have been previously known; viz., the genus Typothorax Cope, from New Mexico, and Aëtosaurus Fraas from Würtemberg.

The discovery of the Permian form in question is important from various points of view. The discovery confirms again a hypothesis proposed by me, several years previously (NATURALIST, 1885, p. 247, Transac. Amer. Philos. Soc., 1892, p. 24). It presents us with what had been previously wanting, forms ancestral not only to the Triassic Reptilia above referred to, but also ancestral to the order of the Testudinata, which according to Quenstadt and Baur appears first in the Trias. The discovery also brings to light an interesting case of homoplassy, since we have two families in no way allied to each other, the one a Batrachian, and the other a reptile, presenting an identical character, and which is so closely similar in the two, that the carapaces cannot be well distinguished on an external view. Internally, however, the characters differ widely. In the case of the Reptilian family (Otocœlidæ) the structure is what one finds in the Testudinata and Pseudosuchia (Typothorax); while in the Batrachian it is constructed by an expansion of characters already known in other Stegocephalia.

For the accompanying illustrations I am indebted to the American Philosophical Society.

PLATE XXI.

Otocœlus testudineus Cope, From above x.66.

PLATE XXII.

Dissorhophus articulatus Cope, x.82; 1 above; 2 below; 3 anterior view.—E. D. COPE.

Ameghino on the Evolution of Mammalian Teeth.[1]—The discoveries of M. Ameghino in Argentina have put him in a position to throw a great deal of light on the evolution of the Mammalia. Several problems which are presented by general Mammalian dentition should be greatly elucidated by his material, and some of those suggested by the Toxodont and Edentate types are within his reach almost to the exclusion of other investigators. He has already made important con-

[1] See l'Evolution des Dents des Mammiferes par Florentino Ameghino. From the Bull. Acad de Ciencias de Cordoba, XIV, p. 381; Buenos Ayres, 1896.

tributions to the histories of both these orders, while other problems remain open.

In the paper of about 1060 pages now before us, M. Ameghino gives his views on the general subject. It seems that in his work Filogenia, published in 1884, he adopted the view of Gaudry of 1878, (previously barely suggested by others), that complex teeth of Mammalia are produced by the fusion of a number of originally distinct simple teeth; a view which has been supported by Kükenthal and Röse on embryologic grounds. It had been previously believed that additional cusps are the product of plications of the dental crowns of simple teeth, and in 1873 and later I had constructed on that basis a phylogenetic system of dentition. This, as is well-known, proceeds from the simple to the complex, without the element of fusion entering at any point. The series is, for the upper jaw; the haplodont, triconodont, tritubercular,[2] (sectorial) quadritubercular, quinque- and sextubercular, and the various lophodont forms; for the lower jaw; haplodont, triconodont, tritubercular, tuberculosectorial, (sectorial), quadritubercular, and the various lophodonts. This succession corresponds with the time order both in North America and Europe, and it is to be supposed that it must, therefore, do so in other parts of the earth, wherever the Mammalia have developed a dentition beyond primitive types.

I have never attempted to bring into this system the Monotrematous Prototheria, and have maintained that they constitute a distinct phylum.[3] My discovery that the dentition of the Permian Cotylosaurian family of the Pariotichidæ consists of simple teeth arranged in transverse series,[4] induced me to remark[5] "that the only question that could arise" as to the hypothesis of dental fusion "is with regard to the Multituberculata." A fusion of the teeth of the Pariotichidæ could produce molars like those of the Multituberculata; but there is no evidence that such a fusion has ever occurred.

Returning to the Eutherian Mammalia, we observe that Ameghino believes that the complex molars have preceded the simple ones in the order of time, and that the tritubercular molar is the result of a loss of a tubercle of the quadritubercular; the quadritubercular the result of

[2] Rütimeyer used the term trigonodont for triangular molars, without specification of the number of tubercles. This word cannot take the place of tritubercular, since the evolution is a question of tubercles, and not of shape. Some tritubercular teeth are quadrangular (Periptychus) and vice versa.

[3] See Amer. Journal of Morphology, 1889, p. 146.

[4] Proceeds. Amer. Philos. Society, 1895, 439–444.

[5] Primary Factors of Organic Evolution, 1896, p. 334. AMER. NATURALIST, 1896, Plate VIIa, p. 801.

reduction of a still more complex molar. Most of the evidence for this conclusion is derived from the fact, as he believes, that the Mammalia of Eocene and possibly earlier age, which are found in Argentina generally, have quadritubercular molars. In accordance with this view Cetacea and Edentata with numerous teeth, present a primitive type of dentition which has survived.

The reply which can be made to this fundamental proposition as to time-order, is, that M. Ameghino has probably affixed too great an age to his earlier beds. This is the opinion of Lydekker, and such extinct types as occur in those beds which occur elsewhere confirm this conclusion. Thus the Patagonian, which Ameghino regards as an Eocene formation, containing the Pyrotherium, contains also the primitive monkeys Anthropops, and the cetaceous Prosqualodon, Argyrocetus and Diaphorocetus. Now Diaphorocetus and forms closely allied to Arygocetus and Prosqualodon are characteristic of the middle Miocene in North America and Europe. It is highly improbable that the quadrumanous genera discovered by Ameghino are of Eocene age, since nothing of the kind occurs in Eocene beds in the Northern Hemisphere, where more primitive and ancestral lemuroid families represent them. The presence of supposed Condylarthra (not yet described) however, gives an Eocene character, and if the forms described by Ameghino as Multituberculata are really such, this character would be difficult to deny. However, recently Ameghino has recognized that these forms do do not belong to that order, but are true Marsupialia, and Lydekker assert that they do not belong to the Patagonian formation, but to the overlying Santa Cruz beds.[6] But supposing that the Patagonian formation is upper Eocene, it does not furnish the material for an elucidation of the dental characters of the primitive Mammalia. These are only partly displayed in the lower Eocene, for it is in the Postcretaceous (Puerco and Laramie) that the true ancestral relation of the tritubercular molar is fully seen. These formations may be represented by the lower or dinosaurian beds which lie below the Patagonian formation in Argentina, but no Mammalian remains have been found there thus far by Ameghino. The oldest Mammal is said to be the Pyrotherium of the Patagonian formation, but it has an aspect more modern than Eocene. It is suspected by Ameghino to be a proboscidian, but it has not yet been shown that it is not a marsupial.

Dr. Ameghino misinterprets North American fossils in more than one instance. He cites the Amblypoda in evidence of the proposition that the tritubercular molar is the result of a reduction of the quadri-

[6] Geographical History of Mammals, 1896, 115. Ameghino makes the same statement in Enum. Synopt. Mamm. Foss. Eocene de Patagonie, 1894, p. 10.

tubercular. His series is Uintatherium, Coryphodon, Pantolambda; the last the most completely tritubercular. The time order is, however, the reverse, viz.: Pantolambda (Puerco), Coryphodon (Wasatch, and Uintatherium (Bridger); the first the most unmodified tritubercular.

In accordance with his general position Dr. Ameghino believes (p. 72) that the quadritubercular genus Procyon is of great antiquity and prior to tritubercular types. This, however, cannot be believed. It has descended from a primitive plantigrade tritubercular, canine type, as have their allies the bears. The same modification is seen in the Mustelidæ in the badgers; and all such are modern forms. He states (p. 26) that in Periptychus and Mioclænus, Phenacodus and Achænodon, the teeth are quadritubercular. The first two genera have tritubercular molars with insignificant rudiments of others both before and behind the protocone (Periptychus) or behind only (Mioclænus), and they belong to the primitive Puerco period. The other two genera are quadritubercular, but belong to later beds, Phenacodus being Wasatch, and Achænodon, Bridger, neither of which formations has any genus in common with the Puerco (except Didymictis of Puerco and Wasatch ages).

Dr. Ameghino believes the Typotherian suborder of the Toxodontia to be related to the Quadrumana. The digits resemble decidedly those of that suborder, but one important difference is overlooked by him. He has pointed out the striking alternation of the two rows of carpal bones in the Typotheria, in which they agree with the Toxodontia proper, and with the Amblypoda. Now in primitive Quadrumana this alternation does not exist, but the bones of the two carpal rows, like those of the tarsus, are directly juxtaposed, or taxeopodous. This characterizes the Condylarthra, which furnish the exact foot characters of the lemuroids, or ancestral Quadrumana.

Finally as to Dr. Ameghinos' views of the origin of the Cetacea, he again inverts the order of succession. He does this by assuming that the Archæoceti are not related to the Cetacea proper, and cannot be ancestral to them. He does not regard the presence of two rooted molars in the fœtal Balæna as significant in this direction. The opinion of zoologists and paleontologists has been different from this, and I have confirmed the general view in my recent researches on the extinct Balænidæ of the Eastern United States.[7] I have shown that a decided sagittal crest like that of the Zeuglodontidæ exists in some of the Miocene whalebone whales. In my estimation the simple teeth of many Cetacea are the result of a process of dental degeneration.

[7] Proceeds. Amer. Philos. Soc., 1895, p. 139; 1896, p 141.

Their increase in number has not been due to subdivision of primitive teeth as supposed by Kükenthal, nor is it a survival of primitive conditions, as supposed by Ameghino, but it is probably a repetition of similar structures due to an extension of the dental groove and dental lamina, following the gradual elongation of the maxillary and premaxillary bones, proceeding contemporaneously with degeneracy of the teeth themselves.—E. D. Cope.

Eozoon canadense.—In recent numbers of the *Geological Magazine*, Dr. Dawson cites the evidence to date for the animal nature of Eozoon. Briefly summarized, the facts are these: (1) The rocks of the Grenville series, where the fossil in question occurs most abundantly, belong to a sedimentary formation. (2) They form a great calcareous system comparable with the metamorphosed Paleozoic calcareous beds of organic origin in petrological and chemical character. (3) New material showing more plainly the structure of the canal systems and tubes, evidences a definite plan of macroscopical structure. (4) Late discoveries of Archaeospherinae and other objects supposed to be organic in pre-Cambrian rocks in Canada and in Europe afford, to some extent, corroborative evidence in favor of Eozoon. (Geol. Mag. 1895.)

Thickness of the Coal Measures.—According to Mr. J. C. Branner, the total thickness of the Coal Measures (Pennsylvanian) sediments in Arkansas is greater than that of the sediments of the same age in other parts of the country or of the world. He gives the following table of comparison: Arkansas, 23,780 feet; Nova Scotia, 16,000 feet; Utah and Nevada, 16,650 (?) feet: Indian Territory, 10,000 feet. Mr. Branner finds a reason for the great thickness in the drainage of the Continent during Carboniferous times. "The rocks of this series in Arkansas afford fossil evidence that this part of the Continent was probably not much above tide level. The drainage from near the Catskill Mountains in New York flowed south and west. The eastern limit of the basin was somewhere near the Archean belt from New England to central Alabama. This Appalachian water shed crossed the present channel of the Missouri from Central Alabama to the Ouachita uplift, and the drainage flowed westward through what is now the Arkansas Valley, between the Ozark Island on the north and the Arkansas Island on the south." (Amer. Journ. Sci., Sept., 1896.)

Geological News.—Paleozoic.—An accumulation of fresh material from the Ichthyologic fauna of the Cleveland Shales, Loraine County, Ohio, has enabled Mr. C. R. Eastman to determine the rela-

tions of certain body-plates in the Dinichthyids. These are the median ventral plates of Titanichthys, and a postero-dorso-lateral of Dinichthys. The author further states that "every plate present in the body armor of Coccosteus has its representative in Dinichthys, and that the conditions of overlap and underlap are the same in both forms. (Amer. Journ. Sci., July, 1896.)

CENOZOIC.—Mr. H. W. Fairbanks states that the Lower Cretaceous age is represented in Santa Barbara County, California, by the Knoxville beds, containing the characteristic Aucella fossils. This is the southern point at which the genus Aucella has been found in California. (Bull. Dept. Geol. Univ. Calif., Vol. II, 1896.)

The skull of *Orycteropus gaudryi* (Ant-Bear or Aard-Vark) from the Lower Pliocene of Samos, described by C. W. Andrews, indicates an animal about one-fifth less than the living species. The close resemblance between the fossil and recent forms is remarkable. Dr. Forsyth Major has pointed out that the former distribution of the genus indicates its northern origin, and that it spread into Africa along with the rest of the Pliocene Mammalia with which it has been found. (Proceeds. Zool. Soc. London, 1896.)

In discussing the geographical distribution of the known Castoroid species, Mr. Merriam notes that the American Castorinae seem to reach their maximum development at or before the beginning of Pliocene time, while the culmination of the Eurasia group appears to occur in the Pliocene. This apparent earlier culmination of the American Castorinae, together with the earlier extinction of certain forms in this country, seem to point to an American rather than to the European origin of the family. (Bull. Dept. Geol. Univ. Calif., 1896.)

According to Dr. Shufeldt, Harpagornis, the fossil bird recently found in New Zealand, represents a more or less aquiline type, that might easily have been the common ancestor to a number of genera of existing modern eagles, as, for example, Haliaetus, Aquila and Thalassaetus. A natural scheme of classification would place it between the genera Aquila and Thalassaetus. (Trans. New Zeal. Inst. [1895], 1896.)

ZOOLOGY.

Fishes in isolated pools.—The occurrence of fishes in pools which have no communication with running streams or large bodies of water has been often noticed, and the explanation of their origin and persistence in such places is in some cases not satisfactory.

In collecting during this month (September) in Camden county, New Jersey, I made the following observations. I fished near Winslow, a pool of about twenty-five feet in diameter, and two feet in depth, with a muddy bottom and a few Nymphæas growing in it. It is distant about a quarter of a mile from an insignificant ditch with a little running water, and is surrounded by higher and sandy ground, offering no possible communication with the ditch. A half mile distant and still more inaccessible is a running stream. From this pool I caught large numbers of the following fishes. *Umbra pygmæa*, *Apomotis obesus* and *Acantharchus pomotis*. The Acantharchi were small, while the others were fully grown.

A quarter of a mile distant from this pool, and at an equal distance from the ditch above mentioned, and not connected with it by any depression of the surface, is another pool of about thirty feet in diameter. The water reaches a depth of three feet in a limited portion of it, and Nymphæas are more numerous, together with Utricularia, etc. Here I obtained the following fishes in considerable numbers. *Umbra pygmæa*, *Amiurus prosthistius*, *Esox vermiculatus*, *Aphododerus sayanus*, *Apomotis obesus*, *Mesogonistius chaetodon*, *Acantharchus pomotis*. Many of these were fully grown. The turtle *Chrysemys picta* was also abundant.

The mud in the first mentioned pool was light colored, and all the fishes were remarkable for the extreme paleness of their tints. The second pool is situated in better soil and its mud contains much decomposing vegetable matter, and is consequently black. The fishes were all deeply pigmented, including the three species found in the other pool, from which they could be distinguished at a glance. The smaller pool was said to have been dried up during the past summer.

Seven additional specimens of the *Amiurus prosthistius* Cope enable me to verify the characters already given (Proceeds. Acad. Nat. Sci., Philada., 1883, 133), from an examination of four from the Batstow River, New Jersey. In five of the new specimens where I counted the anal rays, they number 26. Prof. Jordan has recently attempted to

identify the species with his *A. erebennus* in The Fishes of North and Middle America, 1896, p. 139. From Jordan's descriptions it is evident that the *A. prosthistius* is nearer to the *A. natalis* than to the *A. erebennus*. The spines are not elongated as in the former, nor is the head long and narrowed forward, but it is short and wide; it enters the length (without caudal) 3½ times and not four times. The mouth is relatively wider in the *A. prosthistius*, being .66 of the head-length, and not .5 of it as in *A. erebennus*. The inferior barbels are white in the former, while one may suppose they are black in the *A. erebennus* from Jordan's description. The supraoccipital spine is widely separated from the dorsal spine. In the specimens from Winslow the anal fin is relatively longer than in those from Batstow; in the former it enters the length (without caudal fin) very little over three times (3.2), while in the latter it enters from 3.5 times in one, to 3.66 in two, and 3.85 in another. The length of the anal rays is .66 of that of the head in the Winslow specimens, and .5 of the head in the Batstow specimens. The latter are of larger size.—E. D. COPE.

On the Mud Minnow (Umbra pygmaea) as an air breather.—In the autumn of 1895, I tried to keep a few fishes alive in a small aquarium, viz., a glass jar holding about a gallon. This was filled with well water and some water plants placed in it which grew well. Various fish were placed in it from time to time but all without exception died in less than six hours except a Mud Minnow (*Umbra pygmæa*) This came to the top at frequent intervals, on each occasion emitting bubbles of air and presumably gulping more down, making considerable noise in so doing. On being placed in well aerated water six weeks or more later, this habit ceased.

The other fishes which were placed in the jar, Catfishes, Minnows, Sunfish, and Suckers would come at once to the top gasping for air, and died in an hour or two.

I have placed other of these fish—*Umbra pygmæa*, in well water and they acted the same way, coming to the top at frequent intervals and "bubbling" each time.

I have never found any of these fish dead in dried up pools, though I have carefully looked for them, presumably their ability to use air for respiration saves them.—C. S. BRIMLEY.

The Peritoneal Epithelium in Amphibia.—In a recent study of the peritoneal ephithelium in Amphibia the following points were noted. The species examined were *Necturus maculatus*, *Amblystoma punctatum*, *Desmognathus fusca* and *Diemyctylus viridescens*. All the

specimens of *Necturus*, *Desmognathus* and *Diemyctylus* were taken from January to April and none were examined after spawning. Specimens of *Amblystoma* were studied shortly before and immediately after ovulation, and in August and December. In all the species cilia were found only in the adult female. They occurred constantly upon the hepatic ligament, the ventral wall of the body cavity, the membranes near the mouths of the oviducts and upon the serosa of the liver. In *Amblystoma* cilia were also found upon the mesoarium and the membranes supporting the oviducts. Some of the adult female *Necturi* possessed cilia also upon the cephalic part of the dorsal wall of the body cavity. The ciliated cells occured either singly or in groups. They were most numerous near the mouths of the oviducts. It was found that the direction of the current produced by the cilia was towards and into the mouths of the oviducts. This and the fact that cilia are present upon the peritoneum of the adult female only would seem to strengthen the theory that the ova when set free in the body cavity are propelled by means of cilia into the oviducts.—ISABELLA M. GREEN.

The Penial Structure of the Sauria.—In the Proceedings of the Philadelphia Academy for August and September I have published a paper on this subject, which gives the results of an investigation into the anatomy of the hemipenes of lizards. Very little attention has been given to the subject hitherto, and our knowledge up to 1856[1] is thus summarized by Stannius: " A duplication or bifurcation of each organ is present in *Lacerta* and in *Platydactylus guttatus*. The copulatory organs of the Chamæleonidæ are distinguished by their shortness. In various Varanidæ which have been investigated the internal cavity (external when protruded) has transverse concentric folds. A fissure interrupts these folds so that they are not complete annuli. The extremity is acuminate and expands at the base, forming a kind of glans."

In 1870[2] J. E. Gray describes and figure this organ of *Varanus heraldicus*, giving the best illustration that I know of. In 1886 Wiedersheim (Lehrbuch der Vergl Anat. Wirbelth.) describes and figures this organ in Lacerta. Besides these references I know of nothing later.

As was to have been anticipated, I have found these organs to correspond with the rest of the structure, and to furnish invaluable aids to the determination of affinities among the Sauria. Reference to them

[1] Zootomie der Amphibien, p. 266.

[2] Annals Magaz. Nat. Hist., 1870, VII, p. 283.

cannot be omitted henceforth in cases where the other characters render the question of affinity uncertain.

In the Sauria the male intromittent organ or hemipenis, presents much variety of structure, showing some parallels to the corresponding part in the snakes. It is, however, rarely spinous, as is so generally the case in the Ophidia, the only spinous forms being, so far as I have examined, the American Diploglossinæ and genera allied to *Cophias*. The higher Sauria have the apical parts modified, as in the Ophidia, by the presence of calyculi. Such are characteristic of the Rhiptoglossa and Pachyglossa. The Nyctisaura possess the same feature. The Diploglossa, Helodermatoidea and Thecaglossa have the organ flounced, the flounces often pocketed or repand on the margin. In the Leptoglossa we have laminæ only; in the Tiidæ mostly transverse, and in the Scincidæ mostly longitudinal. In various genera terminal papillæ are present. The organ may be simple or bifurcate or merely bilobate. I have not met with the case so common in the Ophidia, where the sulcus spermaticus is bifurcate and the organ undivided.

The structures of the hemipenis have a constant systematic value. As in the Ophidia, the value differs with the character, but it varies from generic to superfamily in rank.—E. D. Cope.

Food habits of Woodpeckers.—A preliminary report on the food habits of Woodpeckers has been published by F. E. L. Beal, the assistant ornithologist in the U. S. Dept. of Agriculture. The paper is based on the examinations of 679 stomachs of Woodpeckers, representing 7 species—all from the eastern United States. The results of the author's investigations are summarized as follows:

"In reviewing the results of these investigations and comparing one species with another, without losing sight of the fact that comparative good is not necessarily positive good, it appears that of 7 species considered the Downy Woodpecker is the most beneficial. This is due in part to the great number of insects it eats, and in part to the nature of its vegetable food, which is of little value to man. Three-fourths of its food consists of insects, and few of these are useful kinds. Of grain, it eats practically none. The greatest sin we can lay at its door is the dissemination of poison ivy."

"The Hairy Woodpecker probably ranks next to the Downy in point of usefulness. It eats fewer ants, but a relatively larger percentage of beetles and caterpillars. Its grain eating record is trifling; two stomachs taken in September and October contained corn. For fruit, it seeks the forests and swamps, where it finds wild cherries, grapes, and

the berries of dogwood and Virginia Creeper. It eats fewer seeds of the poison ivy and poison sumac than the Downy."

"The Flicker eats a smaller percentage of insects than either the Downy or the Hairy Woodpecker, but if eating ants is to be considered a virtue, then surely this bird must be exalted, for three-fourths of all the insects it eats, comprising nearly half of its whole food, are ants. It is accused of eating corn, but its stomach yielded only a little. Fruit constitutes about one-fourth of its whole fare, but the bird depends upon nature and not upon man to furnish the supply."

"Judged by the results of the stomach examinations of the Downy and Hairy Woodpecker and Flicker it would be hard to find three other species of our common birds with fewer harmful qualities.

The Ectal Relations of the Right and Left Parietal and Paroccipital Fissures.—A preliminary communication upon this subject was made by Dr. B. G. Wilder at the last session of the American Neurological Association in Philadelphia.

The following abstract presents the salient points of the paper:

"The parietal and paroccipital fissures may be either completely separated by an isthmus, or apparently continuous. When so continuous ectally there may still be an ental and concealed vadum or shallow. Disregarding the vadum on the present occasion, the ectal relations of the two fissures may be designated as either *continuity* or *separation*. That continuity occurs more frequently on the left side has been noticed by Ecker, Cunningham and the writer. Hitherto, however, statistics have included unmated cerebrums as well as mates from the same individuals. The following statement is based upon the cerebrums of 58 adults of both sexes and various nationalities and characters. The speaker has examined 48; the other ten have been accurately recorded by Bischoff, Dana, Jensen and Mills."

"The four possible combinations of right and left continuity and separation occurred as follows."

"I. Left continuity and right separation in 27; 46.5 per cent.

II. Right and left continuity in 22; 38 per cent.

III. Right and left separation in 8; 13.8 per cent.

IV. Left separation and right continuity in 1; 1.7 per cent."

"When five groups of persons are recognized the combinations are as follows:

A. In 8 moral and educated persons, combination I, 62.5; II, 25; III, 12.5.

B. In 23 ignorant or unknown I, 56.5; II, 34.8; III, 8.7.

C. In 20 insane, I, 40; II, 35; III, 20; IV, 5.

D. In four murderers, I, 0; II, 75; III, 25.

E. In three negroes, I, 33; II, 67.

So far as these 58 individuals are concerned, the most common combination, viz., left continuity and right separation, is decidedly the rule with the moral and educated, less frequent with the ignorant and unknown, the insane and negroes, and does not occur at all in the murderers. The only instance of the reverse combination (left separation and right continuity) is an insane Swiss woman. The only two known to be left-handed presented the more frequent combination I. (Journ. Comp. Neurol. Cincinnati, Vol. VI, 1896.)

PSYCHOLOGY.[1]

The Nature of Feeling.—A cardinal point of dispute in current psychology is the nature of feeling. The division of simple feeling into pleasure and pain is generally accepted; the question that remains unsettled is the relation of these latter to sensation. Wundt, Lehmann, Marshall and other recent writers, whose views differ in important respects, agree in regarding pleasure-and-pain as a characteristic of sensation (its *Gefühlston*) like quality or intensity. On the other hand there are those who claim that pain (at least) is a separate species of sensation, with a distinct set of nerves and end-organs. Goldscheider at one time believed that he had discovered these pain nerves, but he has recently retracted this claim. Others, again, regard pain as an extreme form or quality of sensation common to the touch, heat and cold senses.

The problem is somewhat complicated by the ambiguity of the word *pain*. In the sense of "physical pain" (*Schmerz*) it may be a species of sensation; while at the same time in the sense of "displeasure" (*Unlust*) it may be regarded as either an "attribute" of sensation or a second element of consciousness. This distinction is maintained by Münsterberg and Baldwin, among others.[2] The ordinary associations of the word *pain* have undoubtedly biased many writers and helped to keep alive the confusion between its two meanings.

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

[2] Dr. Nichols in his criticism of Baldwin in the September number of this magazine certainly misapprehends the latter's view on this point. Cf. Mental Development, pp. 483, f.

Prof. Titchener in treating of the subject in his Psychology[3] endeavors to avoid this ambiguity by discarding the terms pleasure and pain, and using *pleasantness* and *unpleasantness* instead. Apart from his terminology, Prof. Titchener's discussion is of special interest from the fact that, although an earnest follower of Wundt in most respects, he recognizes feeling or affection, as a distinct element of consciousness. Wundt reduces all consciousness (aside from the active) to a single element, sensation; Prof. Titchener restricts sensation to the cognitive side of consciousness, and makes affection a distinct and co-ordinate term.

The mind, or consciousness, he says, "not only senses: it *feels*. It not only receives impressions and has sensations: it receives impressions in a certain *way*. . . . Life means the balance of power (more or less effective) in the perpetual conflict of two opposing forces—growth and decay. No impression can be made upon the living body that does not tend in some way to change this balance. . . . It must help either to build up nervous substance or to break it down. The organism is a whole: and what effects it in either of these ways at one part, must affect it as a whole, in all. The conscious processes corresponding to the general bodily processes thus set up by stimuli—processes not confined to definite bodily organs—are termed *affections*. . . . There are only two bodily processes to give rise to affective processes: the building-up process (anabolism) and the breaking-down process (catabolism). We should expect, then, to find no more than two qualities of affection; and introspection tells us that expectation is correct. The anabolic bodily processes correspond to the conscious quality of *pleasantness*, catabolic processes to that of *unpleasantness*."

Prof. Titchener then examines the relation of affection to sensation. "The processes of pleasantness and unpleasantness seem, at least in many cases, to bear a strong resemblance to certain concrete experiences which we have analyzed, provisionally, as complexes of sensations. Thus pleasantness may suggest health, drowsiness, bodily comfort; and unpleasantness pain, discomfort, overtiredness, etc. . . . Now there can be no doubt of the resemblance in the instances cited. But the reason of it is simply this, that health, drowsiness and bodily comfort *are* pleasant, *i. e.*, that pleasantness is one of the constituent processes, running alongside of various sensation processes, in the total conscious experience which we call 'health,' etc.; and that pain, bodily discomfort and overtiredness *are* unpleasant, *i. e.*, that unpleasantness is one of the processes contained in each of these complex experiences.

[3] An Outline of Psychology, by E. B. Titchener, Chap. V.

Beyond this there is no resemblance: a sensation process is radically different from a pleasantness or an unpleasantness." This difference appears in several ways:

(1). The sensation is looked upon as belonging to the object which gives rise to it, while the affection is regarded as belonging to the subject or conscious self. "Blue seems to belong to the sky; but the pleasantness of the blue is in me. Warmth seems to belong to the burning coals; but the pleasantness of warmth is in me. . . . The distinction is unhesitatingly drawn in popular thought, and clearly shown in language. It points to a real difference between sensation and affection as factors in mental experience—a difference which the psychologist must make explicit in his definition of the two processes. The same difference is observed even when we compare the affective processes with those sensations which are occasioned from within, by a change in the state of the bodily organ. The unpleasantness of toothache is far more personal to me than the pain of it. The pain is 'in the tooth;' the unpleasantness is as wide as consciousness."

(2). If a stimulus be long continued, the affection, if it is not of such a character as to pass over into pain, in the end becomes indifferent, while the sensation remains as strong and clear as ever, when the attention is directed to it. "Nervous substance, at the same time that it is very impressionable, is eminently adaptable. The organism adjusts itself to its circumstances—resigns itself, so to say, to their inevitableness. When once adaptation or adjustment to surroundings is complete, the surroundings cease to be taken either pleasantly or unpleasantly; their impressions are simply received, passively and unfeelingly."

(3). "The more closely we attend to a sensation, the clearer does it become, and the longer and more accurately do we remember it. We cannot attend to an affection at all. If we attempt to do so, the pleasantness or unpleasantness at once eludes us and disappears, and we find ourselves attending to some obtrusive sensation or idea which we had no desire to observe."

(4). "As a general rule, 'central' sensations are much fainter and weaker than 'peripheral.' A remembered noise has hardly anything of the intensity of the noise as heard. Affection can originate in the same two ways. But 'central' pleasantness and unpleasantness are not only as strong as—they are in very many cases stronger than —'peripheral.'"

"We see, then," concludes Prof. Titchener, "that there are strong reasons for regarding affection as different from sensation. It must be

carefully noted that the statements just given of these reasons do not tell us *how* 'red,' a sensation, differs from 'pleasantness,' an affection, in mental experience. They are sufficient indication that a real difference exists; but the difference itself cannot be described—it must be experienced."

It remains to be seen how this theory, or rather Prof. Titchener's restatement of it, will be met by the adherents of the Wundtian view. As to the verbal innovation, the terms *pleasantness* and *unpleasantness* would be more welcome if the proposed meanings accorded better with ordinary usage. Both words, especially the second, are suggestive of a very mild form of feeling; and until we became accustomed to the change it would excite our sense of the ludicrous to call the feeling connected with a violent toothache or an intense abdominal pain *unpleasant.*—H. C. WARREN.

Further Comments on Prof. Baldwin's "New Factor in Evolution."—In a "Note" in THE AMERICAN NATURALIST, October, 1896, Prof. Baldwin declares that I have grossly misunderstood his views, and that, to quote his words, "Dr. Nichols' home thrusts are all directed at my view of pleasure and pain, which he considers, quite mistakenly, the point of my paper. On the contrary, the 'factor' is entirely the influence of the individuals adaptation on the course of evolution; not at all the particular way in which the individual makes its adaptation."

This quotation is typical of the author's style of thinking and writing; of which his critics unanimously complain. The word "influence" is frequently misused by careless writers, as in the above, to denote the *results* of a factor, rather than the factor itself. A "factor" is a set of influences or circumstances contributing *to produce* a result. It is true that an author, if of expansive mind, may run ahead of his subject. It is true, as Prof. Baldwin above declares, that his mind was chiefly on the results supposed by him to be worked by his factor. But he should not forget that he declared himself, in his title, to be writing about his "new factor"; and it was quite correct that he should write about it, since one ought, in Science, to establish the existence of a thing before discussing its effects. It was this last I had in view when, in my paper, I directed my discussion toward demonstrating that his new factor, as specificly described by Prof. Baldwin, was a myth.

I directed my discussion against Prof. Baldwin's views of pleasure and pain because he completely identified his "factor" with his particular and all-expansive views of pleasure and pain. On p. 451 of his

pamphlet, he sums up his June paper in these words: "It seems proper, therefore, to call the influence of Organic Selection "a new factor; . . . . *The ontogenetic adaptations are really new, not performed; and they are really reproduced in succeeding generations, although not physically inherited.*" Here the author correctly, though in flat contradiction to his Note, declares in so many words his factor *to be* Organic Selection, and "ontogenetic adaptations" is for it but another name. Of this fact the words which he italicized leave no doubt. Naturally, to find out most accurately what Prof. Baldwin means by Organic Selection, we go to that part of his writing which most professedly expounds it. This is done in Part IV, p. 541, under the caption: "The Process of Organic Selection." After preliminary remarks, which I shall speak of later, Prof. Baldwin's exposition is in the following words:

"There is a fact of physiology which, taken together with the facts of psychology, serves to indicate the method of adaptations or accommodations of the individual organism. The general fact is that the organism concentrates its energies upon the locality stimulated, for the continuation of the conditions, movements, stimulations, which are vitally beneficial, and for the cessation of the conditions, movements, stimulations, which are vitally depressing and harmful. In the case of beneficial conditions we find a general *increase of movements, an excess discharge of the energies of movement in the channels already open and habitual; and with this as the psychological side, pleasurable consciousness and attention.* . . . This form of concentration of energy . . . is called the "circular reaction." It is the selective property which Romanes pointed out as characterizing and differentiating life. It characterizes the responses of the organism, however low in the scale, to all stimulations—even those of a mechanical and chemical nature— . . . Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult, *i. e.*, by the selection of fit movements from excessively produced movements, that is, from *movement variations.* So, granting this, we now have the further question: How do these movement variations come to be produced when and where they are needed?"

Having reduced his problem of "the selection of fit movements," *i. e.*, of Organic Selection, to this pointed inquiry, Prof. Baldwin then proceeds to state his still more explicit exposition of his selective "factor" in full, as follows:

"But, as soon as we inquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested [an answer to the last above quoted question]. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of a movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose; this

is disputed no less by the infants movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. There is a characteristic antithesis in vital movements always. Healthy, overflowing, overstretching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which the theory of the selection of movements from over-produced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give the excess movements, from which new adaptations are selected; and that decreased vitality, represented by pain, should do the reverse, *i. e.*, draw off energy and suppress movement.

"If, therefore, we say that here is a type of reaction which all vitality shows we may give it a general descriptive name, *i. e.*, the "Circular Reaction," in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that (it distinguishes etc.) it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. . . . *This kind of selection, since it requires the direct co-operation of the organism itself, I have called Organic Selection.*"

"This" (note the last sentence), then, is the "Organic Selection" which Prof. Baldwin himself specifically declares (p. 451) he names a "new factor." As the reader must see for himself, the author's description of it is a description of pleasure-pain functions pure and simple and nothing more. It is not *merely* the old pleasure-pain tradition, for nothing remains inexpansive in this vigorous author's hands. But it is the orthodox tradition unfolded to "a type of reaction which all vitality shows;" which "distinguishes in form and amount between stimulations which are vitally good and those which are vitally bad;" "which is a characteristic antithesis in vital movements always;" which "is the selective property which Romanes pointed out as characterizing and differentiating life;" and which performs its task of the "*selection of fit movements*" generally, by its universal exercise in all creatures from first to last and at all times.

It is dangerous to grapple with an author who is so macrocosmic in his thought, and so amorphous in his diction. But I discussed Mr. Baldwin's "New Factor" from the point of view of his "expanded" pleasure-pain functions because he so completely *identified* it with them. I cannot conceive this to have been done more explicitly and completely than in the author's specific exposition of Organic Selection in his Part IV. Under this situation it was surely "to the point" to prove Mr. Baldwin's New Factor a myth. The tone of Mr. Baldwin's "Note" seems to indicate that this was done with peculiarly exhaustive effect.

A word remains to be said about Mr. Baldwin's complaint that his pamphlet distinctly insisted on *the fact* of Organic Selection, without regard to any "particular way" it may be accomplished. Prof. Baldwin did file such a caveat upon all possible ways which man may ever invent for proving that Organic Selection *may be* a fact. But this is not the method of Science. She does not feel called upon *to invent* all possible ways before she rejects the sole one offered. When Prof. Baldwin does give us some other "particular way" than the one he did give for the operation of his factor, I will, perhaps, then be able to show him it cannot be called "new" with any sort of justice to Darwin and to biologists commonly.

Of the personal tone of Mr. Baldwin's "Note" I have nothing to remark, save by way of gratification, that it is unmatched in American Science. HERBERT NICHOLS.

Boston, Oct. 14, 1896.

ANTHROPOLOGY.[1]

Pictured Caves in Australia.—In West Australia, New South Wales, Queensland, and doubtless in other parts of Australia, where the geology is favorable, rock shelters and caves have been recently noticed, whose walls are decorated with native allegorical designs and figures of men, birds and animals outlined in colour. Mr. T. Wornsop addressing the Australasian Association for the Advancement of Science at Brisbane in January, 1895 refers to a great number of rock paintings of Kangaroos, Lizards, Emus, Flying birds, Snakes and other forms. Refering to discoveries of these strange and impressively decorated shelters by Sir George R. Grey, Mr. Stockdale, Mr. O. Donnell and others, he states that a general similarity characterizes the designs wherever found, and describes further the curious method of painting generally noted, which appears to consist in smearing the rock surfaces with animal fat, pressing the object to be represented against the greased rock, and then blowing dry color against it so as to thus stencil the outlined form by a surrounding area of contrasting tint. When wet color was splashed on, no grease would have been needed. Mr. W. J. Enright, the discoverer of numerous painted caves and Mr. R. H. Matthews describe in particular the abundant figures of human hands with

[1] This department is edited by H. C. Mercer, University of Pennsylvania.

out stretched fingers apparently painted and stenciled in this manner, often in red, in nearly all the caves. Along the Glen Lake river valley near Kimberly, West Australia and on Bulgar Creek, New South Wales the caves display hearts, white human figures on black backgrounds, staring faces outlined in red, with yellow lines, figures of the rising-sun, and Phallic symbols, where the stenciling according to Mr. Enright has often been done by blowing powered pipe clay from a deposit near at hand (sometimes white and sometimes stained yellow by oxides) upon the greased rock. Strangest sight of all must be the weird shelter on Nardo Creek in Central Queensland where a diabolical picture 70 feet long seems to represent a lake out of which are stretched hundreds of brown human arms pointing, grasping and knotted in many positions as if writhing in torture.

Mr. Wornsop and others looking in vain for a clue to the meaning of the rock paintings, have set in evidence the refusal of neighboring natives to account for them, just as earlier observers in America, were wont to quote indian ignorance of mounds, and earthworks. But on the other hand Mr. Enright noting the fresh appearance of many of the designs, speaks of one of the decorated caves recently inhabited by a native named Cutta Muttan, without doubting that the later had done the painting. No doubt he did, and small question that natives now living in Australia could if sympathetically approached by Ethnologists (who living with them had gained their confidence), explain all the designs. —H. C. Mercer.

Man and the Fossil Horse in Central France.—Not many hundred yards from the classic rock shelters of Laugerie Haute and Laugerie Basse (which contain according to the French classification *Magdalenian* and *Solutrean* culture layers) a recently exposed talus, along the Manaurie brook an affluent of the Vezere (department Dordogne Commune Tayac, France) has revealed an interesting and surprising deposit of human remains associated with bones of the fossil horse. M. M. Chauvet and Riviere digging a trench 17 meters long, 1.80 meters broad and 3 meters deep, found in one day, three hundred and more horse-teeth together with other horse bones generally broken by human hands, besides the remains of the badger (*Meles taxus*) and the canine tooth of a large carnivore. No fresh water or marine shells were found but with the bones about two hundred chipped flint axes ("Turtlebacks") of so-called Chellean type, or of similar ovate form worked only on one side, were unearthed in a few days, with three Mousterian *racloirs*, four discoidal flints, two Magdalanian flakes, two scrapers,

and some nuclei. But few details as to the stratification or formation of the deposit are given in the account published in Cosmos (Sept. 12, 1896, P. 211) and as nothing is said about hammer-stones, and flint chips, we are left to wonder whether the place represents a palæolithic workshop such as Messrs. Spurrel and W. G. Smith found at Crayford and Caddington, England or not. Meanwhile the excavation which we are told is to be continued, if studied with care and without bias may affect the validity of the French subdivisions (*Chellean Moustierian, Solutrean Magdalenian*) of the Palæolithic period in Central France. Judged by the shape of the flint blades found with the horse bones, M. M. Chauvet and Riviere call the deposit *Chello-Moustierian* while hardly a mile away, we have Laugiere Haute classified as showing *Magdalenian* above *Solutrean* cülture layers, with Laugiere Basse, Cro-Magnon and Gorge d'Enfer floored with *Solutrean* only. The rock shelters of Le Moustier (*Moustierian*) and La Madeleine (*Magdalenian*) are not far distant and the question is whether all these different geological epochs supposed to indicate intervals of thousands of years, varying stages of human culture and changes in animal life can be justly established at this remarkable neucleus of ages where one more subdivision is proposed to be added to the list of culture layers represented in an area of a few square miles and based on differences in flint chipping, and variations not universally agreed to, in the sequence of animal life.

Chipped Flint blades from Somali Land.—Mr. H. W. Seton-Karr who presented to the British Association for the Advancement of Science at Ipswich, England in 1895 several heavy ovate blades of chipped flint from Somali Land, has brought more recently from the same region others (referred to in Proceedings of Royal Society, Vol. LX, no. 359, p. 19). Often well worked, considerably patenated, and resembling in shape and make, the drift blades of England and France they appear to have been found not in situ but on ths surface, mostly along water courses where rain or wind had bared them of surrounding earth. No excavations were made to ascertain their position with reference to the surrounding geological strata, and no association appears to have been established with the remains of animals living or extinct. Nothing is said of Hammer-stones or chips that might have testified to the existence of blade workshops at the sites, and nothing as yet save the appearance of the blades (some of which are worked only on one side after the French *Moustier* pattern) has been presented to warrant us in setting back the date of these relics to the date of the similar shapes associated with the Mammoth and Rhinocerus in the Somme Valley.

Cave Hunting in Scotland.—If as we understand no chipped blades of the "Turtleback" or drift character have been gathered in Scotland or northern Europe, if no traces of (Paleolithic) man in Association with the Cave Bear, Woolly Rhinocerus and Mammoth have been discovered in caves or quarries anywhere to the northward of middle England or in Scandinavia North Germany and Russia, if in a word it can be proved that snow and ice precluded human presence or obliterated man's foot-prints in northern Europe at the time when drift men were chipping flint on the banks of the Thames and Somme, then the exploration of caves in any part of this colder European region is of particular scientific interest. Near Oban in Scotland the Mackay, Gas works, Distillery, and MacArthur caves recently explored by Mr. J. Anderson for the Society of Antiquaries of Scotland (see proceedings of the Society, vol. XXIX, 1895, p. 211) showed human rubbish deposits consisting largely of the shells of edible mollusks (Ostrea, Patella, Pecten, Solen, etc.), interbedded in one instance (the Mackay Cave) with a gravel layer apparently caused by a marine inundation. In the latter cave, fairly representing the others, Mr. Anderson found in in the shell rubbish about 150 bone needles and points, seven numerously barbed bone harpoons, sometimes with pierced bases, three pebble hammerstones, a few flint nodules, and several flakes and scrapers together with numerous fish bones and the remains of the common deer, the *Bos longifrons*, boar, the dog and the cat; in other words, the recent fauna of the region. The bones of fifteen human skeletons found apparently near the surface and above the shell and bone refuse in the various caves, according to Mr. Anderson and Sir William Turner, represent a people of the Neolithic or late stone age in Europe, while on the other hand M. Boule (see L. Anthropologie, May and June, 1896, p. 321) citing the gravel bed as evidence of an early flood and comparing the barbed and pierced harpoons with similar harpoons supposed to be of an intermediate age (between Paleolithic and Neolithic) from certain French caves, suggests that the Oban remains form a connecting link between the Paleolithic (Mammoth, Rhinoceros and Reindeer time) and the Neolithic (recent fauna time) of western Europe. When all the results of European archæology are summed up it has been supposed that a hiatus in time unbridged by any intermediate human or animal presence, existed between the earlier and later of these periods, and a link will be added to the archæological chain, if discoveries in French caves or elsewhere satisfactorily fill the supposed gap. But whether the remains from Oban can or cannot be assigned this important intermediate position, further investigation will show. For a time the cave explorer might leave

the southern fields where much collaboration has perplexed the subject, and turn northward. There the coast is clear. There evidence broadening the perspective of the European student, and setting a wide geographical limit to the ancient human record, can be established in unexplored caves, where in a new way the unearthed testimony should show the relation of fossil man to glacial ice and cold.—H. C. MERCER.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

New York Academy of Sciences.—Biological Section, October 12, 1896.—Dr. Bashford Dean and Mr. G. N. Calkins presented preliminary reports upon the results attained at the Columbia University Zoological Laboratory at Port Townsend, Washington. The expedition spent about six weeks in exploring and collecting, and brought home large collections from exceptionally favorable collecting grounds. Dr. Dean spent some time in Monterey, Cal., and brought home collections of eggs and embryos of Chimæra and Bdellostoma.

Dr. J. L. Wortman made a preliminary report upon the American Museum Expedition to the Puerco and Wasatch Beds. He reported finding a connecting link between the close of the Cretaceous and the beginning of the Tertiary. He gave an interesting account of the massive ruins of the so-called cliff-dwellers in the region visited by him. In the Big Horn basin the expedition had remarkable success as well as in the Wind River basin.

Prof. Osborn stated that with the collections made this summer the American Museum could now announce that their Eocene collection was complete, containing all mammals now known in the Eocene; that their collection from the Wasatch bed was the finest in existence, and that from the Wind River basin was complete; the Bridger was represented by all but two or three types; and fine collections have been made in the Uintah.

Mr. W. J. Hornaday made a report of a tour of inspection of foreign zoological gardens, made under the auspices of the New York Zoological Society. He visited fifteen gardens in England and on the continent, studying the features of excellence in each.

Prof. Bristol gave a brief account of the progress at the Marine Biological Laboratory at Wood's Hole, Mass., during the past summer.

Prof. Osborn offered the following resolution on the death of Professor G. Brown Goode, after paying a tribute to his memory:

Resolved, That the members of the Biological Section of the New York Academy of Sciences desire to express their deep sense of loss in the death of Professor G. Brown Goode, of the U. S. National Museum. In common with all naturalists in this country, we have admired his intelligence and highly successful administration of the National Musuem as well as his prompt and ready response to the requests and needs of similar institutions throughout the country.

In face of the arduous and exacting duties of his directorship he has held a leading position among American zoologists, and we are indebted to him for a series of invaluable investigations, especially upon the fishes.

Those of us who had the good fortune to know Professor Goode personally, recall his singular charm of character, his genial interest in the work of others, his true scientific spirit. We have thus lost one of our ablest fellow-workers and one of the truest and best of men.

The resolution was adopted unanimously by a rising vote.

CHARLES L. BRISTOL, *Secretary.*

The Academy of Science of St. Louis.—At a meeting of the Academy of Science of St. Louis, held October 19, 1896, Mr. Trelease exhibited living flowers of *Catasetum gnomus*, demonstrating the extreme irritability of their tentacles and the precision with which the pollinia become attached to any object touching either tentacle. Mr. J. B. S. Norton presented a list of the Ustilagineæ of Kansas, together with the result of germinations of about one-half of the entire number. Three persons were elected to active membership.

WILLIAM TRELEASE, *Recording Secretary.*

The Biological Society of Washington.—The following communications were made: C. Hart Merriam, "A New Fir from Arizona;" Frederick V. Coville, "Notice of Britton and Brown's Illustrated Flora of the Northern United States and Canada;" Erwin F. Smith, "A Bacterial Disease of Potatoes, Tomatoes and Eggplants;" B. E. Fernow, "Timber line: Its Aspects and Causes."

FREDERICK A. LUCAS, *Secretary.*

SCIENTIFIC NEWS.

A course of eight free lectures mainly upon Science and Travel has been arranged by the Field Columbian Museum for Saturday afternoons in October and November at the usual hour, 3 o'clock. Most of these lectures will be illustrated by steropticon views. Subjects, Dates and Lecturers: Oct. 3.—"Archeological Explorations in Peru," Dr. G. A. Dorsey, Assistant Curator of Anthropology, Field Columbian Museum. Oct. 10.—"A Trip to Popocatapetal and Ixtaccihuatl," Prof. O. C. Farrington, Curator of Geology, Field Columbian Museum. Oct. 17. —"San Domingo," Mr. G. K. Cherrie, Assistant Curator of Ornithology, Field Columbian Museum. Oct. 24.—"Egypt and what we know of her," Dr. J. H. Breasted, Instruction in Egyptology and Semitics, University of Chicago. Oct. 31.—"The Petroleum Industry," Dr. D. T. Day, Chief of Division of Mineral Resources, U. S. Geological Survey. Nov. 7.—"Alaska and its Inhabitants," Prof. George L. Collie, Beloit College, Wis. Nov. 14.—"The Economic Geology of the Sea," Mr. H. W. Nichols, Curator of Economic Geology, Field Columbian Museum. Nov. 21.—"The Physical Geography of New England," Dr. H. B. Kümmel, Assistant Professor of Physiography, Lewis Institute.

Dr. Ludwig Reh, formerly assistant in the Museum at Saõ Paulo, Brazil, has been appointed assistant in the Concilium Bibliographicum at Zürich. With this addition to the working force the Bureau will soon bring its work up to date; and its cards will be sent out more frequently than before.

The annual meeting of the American Psychological Association will be held at Boston, December 29th and 30th, 1896, that place and time having been chosen by the American Society of Naturalists and ratified by the President of the Association.

The Executive Committee of the American Society of Naturalists have decided to hold the next meeting of the Naturalists at Boston and have chosen the Inheritance of Acquired Characters for the theme of discussion.

The next session of the Association of American Anatomists shall be held in Washington City, May, 1897, in conjunction with the other societies of the Congress of American Physicians and Surgeons.

J. H. Maiden has been appointed government botanist and director of the botanical gardens of New South Wales, succeeding Charles Moore who held the position for nearly fifty years.

Mr. F. F. Blackman has been appointed assistant in botany in the University of Cambridge, and Dr. E. Albrecht assistant in the Anatomical Institute of the University of Munich.

The Academy of Natural Sciences of Philadelphia has conferred the Hayden Memorial Geological Award for 1896 on Prof. Giovanni Capellini of the University of Bologna.

Prof. A. N. Kuznetzow has been advanced to the position of ordinary professor of botany and director of the botanical gardens in the University of Dorpat.

The Ninth Annual Winter Meeting of the Geological Society of America will be held in the city of Washington, D. C., on December 29, 30, 31, 1896.

Dr. H. Hanns, Th. Loesener and P. Gräbner have been called as scientific assistants to the botanical museum of the University of Berlin.

Dr. L. Kathariner, of Würzburg, goes to the professorship of zoology and comparative anatomy in the University of Freiburg, Switzerland.

Dr. V. Schiffner has been advanced to the position of professor extraordinarius of botany in the German University of Prague.

Dr. A. Möller, of Idstein, well-known for his studies of South American botany has gone to the Forestry Academy at Eberswald.

The Ministry of Education has conferred the title of professor upon the botanist Dr. Kienitz-Gerloff of Weilberg on the Lahn.

Dr. B. Hofer, of the University of Munich, has been appointed professor of fish culture in the Veterinary school at Munich.

Dr. K. Busz, formerly of Marburg, has gone to the University of Munich as extraordinary professor of mineralogy.

Dr. F. W. K. Müller has been advanced to the position of directors assistant in the Museum of Ethnology in Berlin.

Dr. H. F. Reid, of Johns Hopkins University has been advanced to the position of assistant professor of geophysics.

Prof. K. von Kupffer, of Munich, has been elected corresponding member of the Prussian Academy of Sciences.

Dr. V. A. H. Horsley, professor of histology in University College, London, has been made professor emeritus.

Dr. Standenmaier, of Munich, goes to the Lyceum at Friesing as Professor of Chemistry and Mineralogy.

Dr. J. Lerch, well-known for his studies of the Swiss flora, died at Couvet, March 13th of this year.

Canon A. M. Norman is hereafter to be addressed at Houghton-le-Spring, Co. Durham, England.

Dr. F. Kohl has been advanced to the position of ordinary professor in the University of Marburg.

Dr. A. Hosius, professor of mineralogy in the Academy of Münster, died May 11, aged 71 years.

Dr. R. Zuber is now professor extraordinarius of geology in the University of Lemberg.

Dr. A. Zimmermann, of Berlin, goes to the Botanical Gardens at Buitenzorg, Java.

Dr. H. Henking is now professor of zoology in the University of Göttingen.

Prof. W. Tief, of Villach, Carniola, a student of the Diptera, is dead.

THE

AMERICAN NATURALIST

VOL. XXX. December, 1896. 360

THE BIOLOGIC ORIGIN OF MENTAL VARIETY, OR HOW WE CAME TO HAVE MINDS.[1]

BY HERBERT NICHOLS.

It is not an infrequent combination that the most familiar things neither excite curiosity nor are understood. Our subtitle suggests an instance of this kind. The naive man commonly takes for granted that he sees the landscape, and hears the orchestra, for no further reason than that they are there before him to be seen and heard. A man a degree wiser gets so far as to recognize that eyes, ears and a brain are necessary. If a biologist be asked, to-day, how we came by this apparatus, he will answer, "through evolution." This is the maximum reach of Science at present. Yet it is nearly as naive to conceive that we have minds, such as ours, merely because we have eyes, ears and a brain, as for one to imagine that he sees just because he has his eyes open. This becomes apparent if we consider the widely accepted doctrine that all the sensory currents running through the nerves to the brain are of the same general sort, as much so as those in electric wires

[1] This paper, under the title "Psychology and Biology" and now somewhat altered from the original, was one of six lectures on "Modern Psychology and its Bearings," delivered, by the author, at Johns Hopkins University in March, 1896.

some of which ring bells while others blow whistles. For if it be asked why our sensory currents 'ring up' such different results as sight from the optic nerve, and hearing from the auditory nerve, it is plainly not satisfactory to answer, "because we have eyes and ears," if, as this doctrine asserts, the eye and ear nerve currents are alike. Nor is it much more enlightening to be told that "it is the *place* in the cortex to which the different nerves run that makes the difference in the sensation resulting from them;" not unless we are in some way told wherein and why these "places" differ. It is just in the fact of never having even inquired how these "places" came to differ, that our evolutionary science falls short in one of the most curiously interesting and important questions that can arise either in biology or in psychology.

Of course, it is a fundamental assumption of both these sciences that all our mental differences are paralleled by molecular differences among the neural activities that underlie them. But this still avoids the question why these last are different, and how they came to be so. And until some answer shall be found that shall logically connect these ultimate neural peculiarities with those peculiarities in outer objects which the world commonly conceives to correspond to our various sights and sounds, it can scarcely be boasted that we are much less naive than the ancients who thought that the objects gave off films that floated into our minds bodily. I by no means imply that this doctrine of all sensory nerve currents being of the same sort is universally accepted. But where any other hypothesis has been offered in its place, the relationship between inner sense and outer stimulus has been left as barren of explanation as even in this doctrine, where, apparently, the possibility of explanation is cut off altogether. But all these matters we are to examine categorically further on. Sufficient has now been said, by way of introduction, to make clear that it is the *variety* of our sensory responses (without which our minds would not be minds), and of their connection with the sorts of stimuli with which they are now connected, that we are, in this paper, to subject to careful investigation. It should be obvious that this inquiry must in-

volve, fundamentally, the evolutionary relation between biology and psychology; and it is for this reason that I have selected it as worthy of the present occasion.

Plunging at once to the heart of our problem, I may state that there are two possible propositions regarding the fundamental relation of our senses to their respective sense organs; which propositions are mutually contradictory and exclusive of each other; which, being fundamental and contradictory, it is necessary to decide between, as a first step toward any permanent insight into the evolutionary relation between body and mind; yet regarding which neither science nor philosophy, up to the present moment, has given any least intimation. It will be the main purpose of this paper to set forth these alternative postulates as completely as I may, within the limit of an hour; and if within that space we do not arrive at any vantage ground, where we may venture a guess at the proper decision between them, I trust that this will but the more emphasize their vast and crucial significance. To venture a prophecy, I may state that the indispensible solution of these two postulates is not likely to be reached for many years to come, nor until wider discussions and further reaching investigations shall have been ploughed under them, than now cover the fields of the great Weissman-Lamarck controversy.

The first of these postulates may be stated as follows: In the light of the little knowledge we as yet possess, it is open to conceive that, in the beginning of the present epoch of animal evolution, crude or primary protoplasm was sensitive not only to all the forms of physical stimulation which now produce sensory responses in us (i. e., sight, sound, taste, smell, touch, temperature, muscle, and other sensations), but was also capable, in response to appropriate stimuli, of an infinite, or x number of other forms of sensation which we know nothing about. In accord, and in illustration of this possibility, we may conceive that the simplest amorphous creatures now actually experience an infinite variety of transient and elementary sensations, including the few we have and a multitude of others that we never have.

Under this conception, we may look upon the rise and development of sense organs generally to mean the slow differentiation of protoplasm to the exclusive use of certain specific forms of stimulation. Thus we may interpret the appearance of eyes to mean the production of an apparatus peculiarly adapted for light waves to the exclusion of all other forms of stimuli. Through the appropriation of the entire fixed surface of our bodies to the particular sense organs which developed in our ancestry, we see how, under this proposition, our few kinds of sense should have been *preserved* to us; and how the infinite number of others with which it endows primitive protoplasm, *should be lost to us through the required forms of stimulation being shut out.*

The alternative of this fundamental formula is that we may conceive, quite oppositely, that protoplasm was capable at first of only one form of sensory response; and of but one mode of neuro-sensory activity correspondent therewith. What this form of sense was we need not consider, at present, further than to suspect that it may have been far different from anything we experience.

Under this proposition we should attribute the rise of various new senses to the development of new kinds of protoplasm, capable of correspondently new forms of sensation. Thus the advent of sight and of sight organs, here, would mean the development of a new basis of physical activities, peculiarly susceptible to light stimulation, and the psychic counterpart of which would be a new kind of sense.

These, then, are our opposing hypotheses. According to one, *Life began with many fleeting, transitory senses, and we have become shut in to a few permanent and highly developed ones.* According to the other, *Life began with one simple sense, and has opened outward with the development of our various and complicated senses.*

It will now be proper to bring forward the implications of these great rival theories in a way to justify the lofty prospectus which we have announced for them.

First we should note, as already has been intimated, that both propositions, alike fundamentally, assume different phases

of neural activity for every psychic happening or sensation, and for every sense quality and shade of quality. That is, one sort for red, another for blue, and others for every sort of taste, smell, and so on.

Next we may note that both propositions equally involve the fact that physical activities immediately underlying our psychic states are enormously complex. Demonstrably, by modern experimentation, they rest upon a chemical basis, intricate beyond all comparison; each molecule comprising various atomic components which, in number, according to highest authority, mount among the billions, and out-run all adequate comprehension.[2]

Next we may observe that in proportion as these molecular activities are complex, so will the molecular differences be great which correspondently lie *between* our different elementary sensations, that is, which correspond to the psychic differences between sights, sounds, smells, tastes and our other major classifications of sensory elements; and between the reds, blues and greens, sweets, sours and bitters, and other minor differences observable in each subclassification, down to the limits of their infinity.

Of course, the old doctrine of specific energies, handed down to us by Johannes Müller, taught us to expect that every different quality of sense must be paralleled by a different form of neural activity. But by emphasizing the enormous complexity of these neural forms, and the vast molecular differences between them, which must be implied by the differences to be observed among our several senses, I wish to bring forward what appears to me to be one of the most important truths in mental science, and one which, in so far as I know, has never before been caught sight of, or taken into account in deciphering the great problem of m.nd and body.

For finally we may observe that, *Intrinsically and within themselves, these molecular differences, correspondent to the differences among our psychic elemeuts, must necessarily have constituted determining factors of animal evolution; and must have decided what peculiar psychic elements should be selected, and perman-*

[2] See "Man's Glassy Essence," by Charles Pearce, in *The Monist*, October, 1891.

ently incorporated into our psychic existence. They must, therefore, explain the variety of our psychic elements, and the origin of their connection with the central mechanisms on which they now depend, and with their respective sense organs. Consequently, also, they must furnish a key for deciding between our two rival theories of the origin of our psycho-physical organism. And altogether, a study of this subject must unlock to us wide and unforseen fields of scientific truths.

That the evolutionary values of his various "energies" should not have been perceived by Müller is not surprising, but that they remain unconsidered to-day can only be explained by the vagueness of current notions regarding them; and it must now be our task to come to a realization of the conditions which they involve.

Bearing in mind that, as is now demonstrable, the molecular basis underlying each one of the sense-elements of which our brain is capable comprises billions of variable physical constituents, we may first note that this truth must have its influence within the sphere of *Spontaneous Variation.* We do not yet fully know the laws which govern variation, and there is difference of opinion as to the role it plays in developing organisms. But at least the followers of Weissmann should appreciate that not all neural "energies" of such vast complexity could have equal chances of advent, and that this fact must have been a major condition in the origin of those different classes of sensation with which we are now equipped.

Secondly, we may appreciate that the variants in question must constitute determining factors of organization within the circle of *Nutrition.* Not only must our intricate brain components be born into our organism, but they must be maintained there in face of exhaustion and fatigue. Loss of the thyroid gland demonstrates to-day how special are the ingredients necessary for maintaining the general functions of intelligence; and there is ample room within the mysteries of Aphasia to suspect that the requirements are even yet more specific that must be provided within localized regions of the cortex for our different senses. Every notion of modern science suggests that these activities must be specifically com-

plex and vastly variable, and assuming them to be so, the conclusion is inevitable that they should neither be produced or maintained with equal ease; and that, therefore, within the course of adaptation and survival their specific characteristics must have determined, respectively, their own advent and perpetuation.

Third comes the vast region of fitness and selection, which must rise from the relative serviceableness of these several complex activities to *The Environmental Forces*, to whose stimulation they must join themselves for the creature's welfare and preservation. That this sphere is likely to prove of central interest in our problem should be obvious, and because we are to give it much attention further on, we may limit ourselves to the bare statement of it here.

A fourth region of evolutionary choice among possible sense energies must be found in the relative adaptiveness of their molecular complexities to the development and perperfection of such peripheral or end-apparatus processes as are requisite or of profit for mediating between them and the environmental forces. Within this field, more than anywhere else, perhaps, we are likely to discover the functions which most intimately determine the diverse forms of our perceptive organs, and that fix thereby the sorts of mental pictures dependent thereon. This, also, we are to discuss with some fulness presently.

Finally, the factors in question must have determinative bearings within our general physiology. These it will be necessary and important for Science in time to work out; but they are of a nature so remote from psychologic problems proper that they need not be intruded further upon the limited space of this present paper.

Their self-determinative fate within the realms of their Zoologic Genesis, their Physiologic Maintenance and Organization, and their Environmental Adaptation, these, then, are what we have chiefly to study, in our present quest. And these lines being laid down, the following considerations pertinently thrust themselves forward, for our further guidance. It should be obvious at the outset that the nervous currents or impulses

passing from the periphery to the cortex, and arousing there the activities lying nearest to the final sensory results, should be of crucial importance in our investigation of the molecular differences assumed to underly our different senses. Regarding these nerve currents two main opinions are held. Professor Wundt conceives that all the sensory cortical cells are equally potent of all the different sense-forms at our birth; and that the sort of response they actually give is dependent on the form of the impulse that reaches them through the peripheral nerves to which they happen anatomically to be joined.

According to an opposite view, advocated by Prof. James and many others, and to which in our introduction we have already made allusion, the currents in the different sensory nerves are all alike, and the sort of sensory responses they mediate are wholly dependent on the respective places in the cortex to which the different nerves run. The fundamental fact being, that each center is congenitally destined to its one specific form of activity, and the different centers to their permanently different forms.

We can get a sharp notion of these opposing views, as the text-books commonly point out, by imagining the visual and the auditory nerves to be cut somewhere in their course, and the cut ends to be crossed and joined together again so that thereafter the visual impulses will reach the auditory center, and the auditory impulses the visual center. Under this new condition, according to Prof. Wundt, we should both hear and see precisely as before; because both of these cortical centers are capable of both results; because what happens depends on the different forms of the currents that are determined in the peripheral sense organs; and because these run through unchanged, in spite of the crossing and that they are carried to new places. But, according to Prof. James' view, where the currents in the different nerves are all alike, and the results depend wholly on the place in the cortex in which they arrive—under the crossed conditions one should now "see the thunder and hear the lightning."

The chief facts on which the latter notion is founded is, that while the sensory nerves generally are sensitive to several

kinds of artificial stimulation, the sensation resulting thereby is always the same for each nerve. For example, the cut stump of the optic nerve will respond to pinching, pricking, burning, and to chemical and electric stimulation; but always with an indefinite visual flash, whatever the form of stimulus that is applied.

The chief facts upon which those who agree with Prof. Wundt base their opinion, are summed up within certain alleged phenomena of "Substitution," there being some reason to believe that when parts of the cortex are destroyed, either by disease or by experimentation upon animals, certain remaining parts take upon themselves the former functions of the lost parts, and change their former habits and modes of response in so doing.

Neither of these opposing theories are conclusively substantiated at present, since there are counter replies for each. Thus it is open for Prof. Wundt to explain the fact of the optic nerve replying invariably with sight sensations to every sort of artificial stimulation, by saying that this is only true in the adult, where the cells, by having only one form of stimulation brought to them by the nerve to which they are permanently fixed, have been educated persistently in one form of response past the age when they have lost the power of plasticity and of shaking off the old habit to take on a new one—a power which at birth they eminently possessed. And, on the other side, it is open for those who believe in fixed congenital responses to suspect that all the facts of Substitution are due to the lost function being taken up by remaining cells of the same kind, and especially by the correspondent cells of the other half of the body; *i. e.*, those of like kind in the opposite lobe of the brain.

Such is the state of this controversy up to date; and its confusion would be out of place in our present study if we were not now able both to bring this problem, by new considerations, to a solution, and also to demonstrate its cogent bearings upon our main subject. We speedily come to this by recalling that we have already determined that the molecular differences, corresponding to the differences between our several senses, are

certain to have been determining factors of the evolutionary relationship between our minds and bodies in any case, and by then passing on to observe that the sphere of this relationship would be vastly different respectively under our two rival theories regarding afferent nerve currents. Indeed, would be so different as to demand consequences, under one of these theories, so incompatible with existing facts that we shall be able to discard that theory altogether, thus reducing our difficulties, and giving us in the remaining theory an invaluable guide for our main investigation.

This difference in evolutionary sphere comes to view the moment we recognize that under the Wundtian notion (of the sensory currents being different all the way through the nerves to the periphery and to the different environmental forces which respectively stimulate them) the forces determining the selection and perpetuation of these currents in the organism would include all the five regions in which we discovered it was possible for our "molecular differences" (specific energies) to work with selective or evolutionary fitness—namely, the regions of Spontaneous Variation, of Nutrition, of Environmental Adaptiveness, of End-Organ Adaptiveness, and the vaguer sphere of our general physiology. On the other hand, and under the notion that the afferent currents are all alike, it should be plain that this likeness would cut off our central processes, regardless of their molecular differences, from all relative serviceableness either to the great world of environmental forces or to the intricate and indispensable mediating processes in the end-organs, and would thus reduce the sphere of their evolutionary reciprocity to the three remaining and apparently lesser fields.

The significance of this is so great that we shall do well to set forth the connection between our senses and their stimuli, under this point of view, by an illustration. We may do this by imagining two wires coming to this desk, one of which is attached to a bell that is rung in accord with the velocity of the wind outside by an electric current, brought through a wire from a proper apparatus on the roof—a heavy wind ringing the bell violently, and a calm giving no ring at all—and

the other wire we will imagine to be connected with a visible index, the rise and fall of which is determined by the rise and fall of a barometer and other electric apparatus, also situate on the roof. Under this illustration the ringing of the sonorous bell and the moving of the visible index are the analogues of our sensations, the electric wires correspond with our nerves, the wind-gauge and barometer with our end-organs, and the wind and temperature with their external stimuli. And since, under these conditions, by merely changing the wires here at the desk and connecting the barometer with the bell, and the wind-gauge with the index, the "sensory" results would be completely reversed from what they formerly were, so, therefore, we have here a perfect example of what Prof. James means, by saying that all depends on the *place* in the cortex with which the currents or wires are connected. And, going now a step further, these conditions also illustrate what Prof. James has wholly neglected to consider, namely, the evolutionary influences which *made* the "places" in our cortex different, and those which first connected them with the particular end-organs and stimuli with which they are now permanently connected; and these, we quickly perceive, are the important points in our great main problem. Precisely what we want to know is how we came to have the variety of senses that we do have, and how they came to be joined to the particular stimuli to which they are joined. From time out of mind mankind has naively taken for granted that the now existing relationship between sensations and their stimuli is an eternally permanent one of immediate cause and effect. But, as we have pointed out, this cannot be the case if the currents in all the sensory nerves are alike, and if, as Prof. James contends, it is alone the "place" in the cortex which determines the sort of sensation that shall respond to any sort of current or stimulus which may run to it. In this case it should be plain that it is in the characteristic differences of these "places" and in the evolutionary origin of the same, and of their permanent connection with their present peripheral organs, that the secrets must lie which we are in search of. Surely no one, under the conditions of our illustration, would

investigate alone the wind and temperature apparatus on the roof in order to discover why we hear the bell ring instead of see the index move. But, rather he would extend his investigations to discovering how the " nerve " connections originated that now exist, and how the internal apparatus, to which they run, came to be so different that in one case we " see " and in the other " hear " from the same sort of incoming current.

Enabled by this illustration to look with greater clearness into Prof. James' hypothesis, and into some of its implications, we may now go back to the assertions that vastly different spheres of evolutionary influence would be involved as between this theory that these currents are alike and the rival theory that they are different, and to the assertion that certain consequences are logically demanded by the " alike " theory which are so contrary to existing facts that it must be discarded.

What has been neglected by Prof. James is, as I have said, the *evolutionary or selective value* of the sensory currents. If these currents were all alike, then, manifestly the molecular differences which we are obliged to assume in the cortex as underlying our different sensations would be cut off from all diversity of influence either from the end-organ processes or from the environmental forces. *And this is the same as saying that they would be cut off from all selective relationship with these great spheres of influence, and that our end-organs and environment had nothing whatever to do with the origin of our different senses.*

Now, it must not be too quickly inferred from these words in italics, that it would be impossible to account for the evolutionary selection of our several senses within the narrowed sphere of influences remaining after cutting off the peripheral and outer forces. Such an inference would not only be wrong but also would confuse and obscure certain considerations that we are to come to further on, and in view of which it is imperative for me to stop long enough here to point out that the sphere of Spontaneous Variation alone *might* be sufficient to account for the variety of our senses and their present external connections, if *only their origin and not their preservation* needed to be accounted for. And this is done in pointing out that these connections might be originally due wholly to the

period at which a spontaneous variation made a new kind of "sense energy" possible. Thus, the present connection of cerebral sight with the optic nerve and with the eyes, and with the light that falls on them, might well, for all we know to the contrary, be entirely due to the chance appearance of a new form of "energy" or molecular possibility in the cortex just at a time when the development of optic end-organs made a connection with the new cerebral development available, and the exigences of the outer environment made the new linkage of processes of service. Such a connection of inner sense to outer stimulus would be as accidental as anything can be, yet it might be adequate for explaining the facts of our problem *were no influences to be considered that might disturb the permanency of such connections.* And this brings us finally to the influences which most surely would have disturbed the permanency which actually has been maintained, had this theory that the sensory nerve currents are alike been really in force.

These disturbing influences become apparent when we consider the uniformity of the functions that would be left to central nervous processes under the conditions of this theory. No one has ever contended that the outgoing currents of the motor nerves are of diverse kinds. If, therefore, the incoming currents were also all alike, there would then be left to the central processes the entirely homogeneous switch-board function of connecting like currents with like currents. And, under such conditions, and cut off from all diversity of external influences, it seems scarcely possible that some one form of molecular activity or "sense energy" out of the many that variation may have given birth to or protoplasm been originally capable of, would not prove most suitable to this one purpose, and as a consequence become perpetuated to the exclusion of all other less suitable kinds of neural sense-forms. Or, put again more simply, *since moleculur forms are sure to have been evolutionary determinants, therefore, if all the nerve currents were alike it seems certain that the cortical processes must also have become alike.* And since this manifestly is not the case, therefore we must abandon Prof. James' theory.

(*To be Continued.*)

PINEY BRANCH (D. C.) QUARRY WORKSHOP AND ITS IMPLEMENTS.[1]

By Thomas Wilson.[2]

(Continued from page 885.)

II.

Mr. Holmes' paper comprises 26 printed pages. The first part is occupied with a description and statement of facts; the second part is as I have shown made up of theory, assumption, opinion. I have examined them sufficiently to show their want of value. But the climax is reserved to the closing portion, for, commencing on page 19 and continuing 8 pages is a chapter relating to the age of the workshop and the race of the men who worked it. Mr. Holmes' conclusion is that though the quarry is prehistoric the age is not great and the race was the Modern Indian. This he argues with profundity, going into the racial question in detail and with great elaboration.

I decline to argue these propositions. I am appalled at the temerity as well as the dogmatism with which he decides these abstruse questions. He is a gentleman for whom I have the highest regard. I have known him well and favorably for many years. He has studied and written upon art products and art evolution and their relation with prehistoric man, in a philosophical and artistic strain which has done credit to his logic, and been as much benefit to art as to archæology. But Sir John Lubbock, Sir John Evans, Prof. Tylor, Sophus Müller, Hildebrand, Montellius, Naidallac, Hamy, de Mortillet and Cartailhac and the host of eminent Europeans, archæologists and anthropologists, of whom Keane is the latest author, who have spent their lives in the study of this science,

[1] Read before the Anthropological Society, Tuesday Evening, December 4, 1894.

[2] Curator of Prehistoric Anthropology, U. S. National Museum, Washington, D. C.

have not ventured upon the determination of these questions of prehistoric ages and races. with the confidence of Mr. Holmes, and certainly they do not decide these important questions with even a fraction of the satisfaction and certainty which seems to have inspired him.

Mr. Holmes did not content himself with the things of today which he saw in the quarry, but turned his mind's eye back when the quarry was being made and depicts it in the time of antiquity, with apparently as much certainty as if he had been then and there present. He not only describes the work with the detail and positiveness I have shown, telling the periods to which it belonged and the race and culture of the men who did the work, but he assumes to decide upon the objects *not* there. He determines not only upon what was left in the quarry, but he decides with equal positiveness upon the ultimate purpose and intention of the workman and the future use and destination of the implements which had been transported elsewhere.

He describes in several places the leaf-shaped blade—the "third stage" of his process—straight and symmetrical, with edges as slightly beveled as consistent with strength, less than half an inch in thickness and shown in *i* to *p*, Pl. IV (my Pl. XIX), and says "when they were realized, the work of this shop was ended" (XX), "they, and they only, were carried away to destinies we may yet reveal" (p. 13). "No examples of the successful quarry products were left upon the ground" (p. 15). "All forms available for further shaping or immediate use were carried away as being the entire product of the shop * * * for final finishing" (p. 15). "This was a stage of advancement which made them portable and placed them fully within reach of processes to be employed in finishing, and that they had been carried away to the villages and buried in damp earth (*cached*), that they might not become hard and (or) brittle before the time came for flaking them into the forms required in the arts. The history of the quarry forms is not completed, however, until we have noted their *final distribution among the individuals of the various tribes*, until we have witnessed the *final step* in the *shaping process*—the flaking out of specific

forms with a tool of bone—and their *final adaptation to use* and *dispersal over the country*," (p. 18).

"Having reached a definite conclusion that the blades were the *exclusively worked product* of the quarry," he "was led to investigate their subsequent history" (p. 18). The italics are mine. His investigation into the subsequent history of these objects led him to define a *cache*. "A 'cache' is a cluster or hoard of stone implements, numbering, perhaps, a score or more, secreted or deposited in the earth and never exhumed. Such hoards are frequently discovered by workmen in the fields," (p. 18).

Pursuing the "subsequent history" of these implements, I propose to go into the region round about Piney Branch, examine the aboriginal village sites of the District of Columbia, the fields containing these alleged secret hoards or caches, and the known places of aboriginal occupation within the neighborhood where these implements were said to have been carried, and see what have actually been found there, what of caches, what of leaf-shaped blades, and what of implements which had been subjected to the (fourth or other) "processes to be employed in finishing, when they were flaked into the final forms required in the arts" (p. 18), and I propose we compare the the objects actually found in these distant places, with what Mr. Holmes said would be found.

I look through my Department in the National Museum for the leaf-shaped implements which, according to the theory of Mr. Holmes, were made at Piney Branch and carried out to the homes of the Indians, their makers, in the District of Columbia, and I find the numbers insignificant; while, as to caches, the Bureau of Ethnology, through Prof. Cyrus Thomas, has lately made a catalogue of the "Known Prehistoric Works in the Eastern United States," among them deposits, hoards, or caches, and there is not a single cache reported from the District of Columbia, this, despite the statement of Mr. Holmes that "such hoards are frequently discovered by workmen in the field."

In the settlement of these questions, it is of high importance that so far as possible, facts and not guesses should be given.

I have taken the trouble to segregate the specimens in my Department in regard to material and locality and to ask a similar report from such private collectors as I could reach. The results I have given in the form of tables, and I have attempted in these to draw a sharp line between the implements which might, according to the Mr. Holmes' theory, have come from Piney Branch quarry, and those which did not.

No implement of *quartz*, found here or elsewhere came from the Piney Branch quarry, nor any of felsite or rhyolite, nor of argillite, shale or ferruginous sandstone, nor of flint, chert, or jasper; for Piney Branch was a quarry of quartzite only.

The following tables show the Aboriginal chipped stone implements from the District of Columbia and its neighborhood, divided according to material, form, locality, and mode of deposit, so as to show the number of quartzite leaf-shaped blades which might have come from Piney Branch quarry, according to Mr. Holmes' theory, and to compare them with those differing in these conditions, and thereby show what number did not come from Piney Branch.

TABLE I. CACHES, HOARDS OR DEPOSITS OF LEAF-SHAPED BLADES. ARRANGED ACCORDING TO LOCALITY AND MATERIAL.

Locality.	Quartz.		Quartzite.		Porphyritic felsite, Rhyolite.		Argillite shale, Fer. Sandstone.		Flint. Jasper. Chert.		[Totals.]	
	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.	Caches.	No. of Implts.
Bennings			1	7								
Red Bank			1	5							2	12
Jones Landing												
Piney Branch												
Little Falls					1	a8					1	8
Anacostia												
Pierce's Mills					1	a32					1	32
Piscataway, Md			1	b500							1	500
					1	25					1	25
					1	25					1	25
					1	26					1	26
South River, Ann Arundel Co., Maryland							1	114			1	114
South River, Ann Arundel Co., Md									1	7	1	7
South River, Ann Arundel Co., Md									1	4	1	4
Glenely												
Howard County					1	b100					1	100
Clarksville, Howard County					1	52					1	52
			3	512	7	268	1	114	2	11	13	905

a, not leaf-shaped; b, estimated.

There have been found in the District (Table I) but two caches of quartzite, containing together only 12 leaf-shaped blades. These are according to Mr. Holmes' theory, "the entire product of the shops" (p. 15), which "had been carried away to the villages and buried in the damp earth (cached) that they might not become too hard and (or) brittle." This was a sorry product of so extensive a quarry as Piney Branch with the "500,000 pieces of waste and failures" found therein by Mr. Holmes; and must have been a sore disappointment to even the cynical and thriftless Indian.

Plate XXIII represents 20 specimens of a cache of 32 arrow or spear heads and leaf-shaped implements found near Pierce's Mill, Rock Creek. Most of the specimens are broken. They are of porphyritic felsite and, therefore, never had any relation with the quartzite quarry at Piney Branch.

Should it be urged that some of the leaf-shaped blades may not have been cached or, if so, that the caches had been disturbed and the blades scattered over the surface, I have made a schedule of these, (table II), which shows a total of 1,948 leaf-shaped blades found on the surface, not cached, of which 1,065 were of quartz, felsite, argillite, etc., and but 883 of quartzite. It

TABLE II. LEAFED-SHAPED BLADES—*NOT* CACHED.

Locality.	Quartz.	Quartzite.	Porphyritic felsite, Rhyolite.	Argillite, Shale, Fer. Sandstone.	Flint. Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts,
Bennings	4	300	100	100	
	2	30	2	2	1
Red Bank		100	100	40	
Jones Landing		50	20	10	
Piney Branch		15			
		17			
Little Falls	1	215	308	202	
Anacostia	2				
Pierce's Mill					
Cabin John		22	6		1
Piscataway					
U. S. Natl. Mus. Miscl. Collections, from D. C., generally.	148	134	5	10	1
	157	883	541	364	3

Total implements, quartzite, ...883
Total implements not quartzite, ...1065

1,948

is a part of Mr. Holmes' theory that "the working of such a quarry led *inevitably* to the production of blades *in numbers* (meaning in great numbers), and it follows that they were removed "in numbers" (p. 18), but my examination demonstrates the error of this theory, for it shows the blades of quartzite (which alone could have been carried from Piney Branch Quarry) to be in the minority.

Again, Mr. Holmes theorizes (p. 18) that a "time came for flaking them (the blades) into the final forms, knife-blades, scrapers, perforators, and arrow and spear points required in the arts." Therefore, I made still another table (III) to show any of these final forms which might possibly have been made *from leaf-shaped blades;* and, again, we find the theory not

TABLE III. ARROW AND SPEAR HEADS WHICH MIGHT HAVE BEEN MADE FROM LEAF-SHAPED BLADES.

Locality.	Quartz.	Quartzite.	Porphyritic felsite, Rhyolite.	Argillite, Clay slate. Iron stone.	Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.
Bennings	15	15	7	2	
	300	100	50	25	
		2			
Red Bank	200	75	300	50	
Jones Landing	100	50	50	25	
Piney Branch					
Anacostia		2			
Little Falls	102	200	304	50	
Falls Church, Va.,		37	1	8	
		4	1		
Piscataway	3				
U. S. Natl. Mus. Mis. Collec-					8
tions, from D. C., generally.	149	209	15	73	2
	869	664	728	223	10

Total implements, quartzite, ...694
Total implements not quartzite, ...1840

2534

borne out by facts, for of all these leaf-shaped forms, numbering 2,634, only 694 were of quartzite and could have come from Piney Branch quarry. Thus, it appears that of the leaf-shaped blades found in the District and its environments, cached or not cached, the greater number have been of other material than quartzite, and must have come from other localities than Piney Branch quarry. Is not all this cumulative evidence of error somewhere in Mr. Holmes' theory?

There have been caches found adjacent in Maryland, and it may be suggested that these implements from Piney Branch might have been carried beyond the boundaries of the District of Columbia. But, unfortunately for this theory, the implements which have been found *en cache* in Maryland and adjacent to the District of Columbia are of porphyritic felsite, argillite, and other different material from those in the quarry at Piney Branch, and thus totally dissimilar from them. J. D. McGuire, Esq., of Ellicott City, Md., has furnished the best Maryland collection of these implements known (Table I) and he has kindly furnished me a sample series which have been photographed and are shown in Plate XXIV.

They show 8 caches—one of them 100 and one 114 specimens and a total of 365 specimens, not one of which could possibly have come from Piney Branch for one cache is of flint and jasper specimens, and one of argillite (similar to the leaf-shaped blades found by Dr. Abbot at Trenton), and six are porphyritic felsite or rhyolite.

The leaf-shaped implements found *en cache* in Maryland and some parts of Pennsylvania are, I believe, mostly either of argillite or porphyritic felsite. Several of these caches from the respective localities are to be seen in the Museum, and a single glance is sufficient to establish the absence of their relationship with the quartzite from Piney Branch.

We have now sought for the Piney Branch leaf-shaped quartzite blades at the home of the Indian, throughout the Districs of Columbia and the adjacent parts of Maryland where, according to Mr. Holmes, they were "buried in the damp earth;" andwe have sought in vain. Caches of such implements are not found within the District nor in its neighborhood. It may be hardy to declare a negative and to say that because these quartzite implements have not been found that they do not exist; but how much more hardy and, indeed, perilous must it be for Mr. Holmes to risk everything by declaring the existence of these caches when they have never been found.

The story told by the tables is not completed. Table IV tells of the "flaked implements, knife-blades, scrapers, arrow and spear points and perforators" (which Mr. Holmes says

were common to the region), which were not from the Piney Branch quarry because not made of quartzite. This table shows 21497 such specimens. Table III, showed 2,534 specimens which might have been made from leaf-shaped blades,

TABLE IV. OTHER IMPLEMENTS, SUCH AS KNIVES, SCRAPERS, PERFORATORS, ARROW AND SPEAR HEADS, ETC., APPARENTLY *NOT* MADE FROM LEAF-SHAPED BLADES

Locality.	Quartz.	Quartzite.	Porphritic Felsite, Rhyolite.	Argillite, Clay. slate, Iron stone.	Flint. Jasper. Chert.
	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.	No. of Implts.
Bennings	716	260	13	350	
	3000	4000	200	1500	
Red Bank	700	1200	1500	500	
Jones Landing	300	500	200	100	
Anacostia	35				
Piney Branch					
Little Falls	28	12	23	26	
	500	1000	2000	100	
Pierce's Mill			32		
Piscataway	25	75			
Falls Church	8	6		1	
U. S. Natl. Mus. Miscl. Collections. from D. C., generally.			3987		12
	1720	554	19	253	29
	7032	7607	3955	2830	41

Total implements *not* made from leaf-shaped blades.................. 21,497

RECAPITULATION.

Table I. Leaf-shaped blades, *cached*	865
Table II. Leaf-shaped blades, *not cached*,	1,948
Table III. Leaf-shaped blades, Impls. made from,	2,534
Table IV. Impls. *not* made from leaf-shaped blades,	21,497
	26,844

but of these, only 694 were of quartzite. The aggregate of these counts shows 24,031 (21,497 + 2, 534) specimens in these collections not made from Piney Branch quartzite leaf-shaped blades, against 694 which might have been.

Plate XXV shows how arrow and spear heads are, or may be, made from leaf-shaped implements. The five specimens at the top of the Plate are such. They were at one time leaf-shaped implements, and by the making of the notch and stem, they have been changed to arrow or spear heads, that is to say, they have been subjected to the second process which has changed them "into the final forms re-

quired by the arts" (p. 18). The four specimens at the bottom are leaf-shaped blades of quartzite found on the surface at Bennings, D. C., and might or might not have been the product of the quarry at Piney Branch. They form part of the 330 in Table II from that locality. Those in the middle are also leaf-shaped, found on the surface in the District or adjoining it in Maryland or Virginia, but are of quartz, argillite, shale, porphyritic felsite, all of them other material than quartzite, and so they could not have been the product of the quarry at Piney Branch. They form part of 541 in Table II and of the 1948 in Table V.

TABLE V. RECAPITULATION ACCORDING TO FORM.

	No. of Implts.	No. of Implts.	Implts. of quartzite, possibly from Piney Branch.
I.			
Caches, leaf-shaped,	865	865	a
Caches, not leaf-shaped,	40		12
II.			
Leaf-shaped blades. Surface, not cached,		1,948	883
III.			
Arrow and spear heads, etc., flaked implements which, from their form, might have been made from leaf-shaped blades,		2,534	694
IV.			1,589—Total which (according to Mr. Holmes' theory) might have been made in or come from the quarry at Piney Branch, out of a total 26,812.
Flaked implements, knife blades, scrapers, arrow and spear points, perforators which were judged, from their form and material, were not made from leaf-shaped blades of quartzite,		21,465	
	Total, all kinds,	26,812	

a. This omits the 500 from Piscataway because it is so far distant from Piney Branch and because we have no report of other implements from that locality. This cache was reported by Mr. Reynolds, who had but a single implement, given him by the finder, as a specimen.

In considering these tables and their bearing on the Piney Branch Quarry, we are to keep continually in mind that the sole and only material in that quarry was quartzite. There was no quartz pophyritic felsite, rhyolite, shale, ferruginous sandstone, flint, jasper or chert found in any of its deposits, and all implements made from any of these materials are to be excluded from consideration because impossible to have come from that quarry. Keeping this in view, these tables show

the following state of facts: Among all those implements from the District of Columbia, but two caches of quartzite were found containing together only 12 leaf-shaped blades of 1.948 leaf-shaped blades not cached, only 883 were of quartzite; of 2,534 common implements, such as arrow- and spear-heads etc., which from their form might have been made from leaf-shaped blades, only 694 were of quartzite, making a total of 1.589 quartzite implements which, according to Mr. Holmes' theory, *might* have come from the Piney Branch Quarry, out of a total of 25,815 implements examined.

Out of all the "1,000 turtle-backs" (p. 14) gathered by Mr. Holmes, their "500,000 brothers and sisters' (p. 12) left, and the "millions of worked stone and unshaped fragments" (p. 7), all "refuse" (p. 12), "waste, failures" (p. 14), of which "these quarries on Rock Creek are the main source," all being done to produce these leaf-shaped blades to be carried away and buried (cached) in the damp earth "that they might be preserved to be made into the final forms required by the arts" (p. 18).—Out of all this toil, the result found up to date is but 2 caches with 12 blades. "The mountain was in labor," etc., etc. Out of a total of 26,812 implements reported in the collections mentioned, but 1,589 were of quartzite leaf-shaped blades that *could have come* from the Piney Branch quarry. Yet the leaf-shaped blades were, according to Mr. Holmes, the "entire product of the quarry" (pp. 13 and 15). What a deal of sack for a pennyworth of bread.

Mr. Holmes' theory that the leaf-shaped blade was the sole product of the quarry workshop, to be afterwards "flaked into the final form" of the common implements of the region, be correct, then the problem may be stated according to the arithmetical law of proportion, as follows: If 1,589 leaf-shaped *quartzite* blades, cached and not cached, finished and unfinished, have been produced from Indian toil and exertion in making the "500,000 turtle-backs," and the "million of worked stones which now occupy the site" (p. 7), all of which are wastes and failures; then how much toil and exertion, and how many millions of worked stones, wastes and failures,

would be required to produce the 26,812 specimens reported in the collections mentioned? I have inveighed against the speculation by which we sometimes attempt to determine, what a great amount of labor the Indian would do for the accomplishment of so little, sometimes the reverse; but we may fairly assume that the aborigine was not such a consummate idiot as to open a quarry as large as this at Piney Branch, and do as much hard work as must have been done there, with the paltry outcome of the insignificant number of quartzite implements shown in the aggregate collections form the District of Columbia. To complete the information on this branch, I have introduced the consolidated tables VI and VII showing the subdivisions according to material and locality.

TABLE VI. RECAPITULATION ACCORDING TO MATERIAL.

Quartz	8,058
Quartzite	9,674
Porphyritic felsite, rhyolite	5,478
Argillite, shale, fer. sandstone	3,541
Chert, flint, jasper	64
	26,815

TABLE VII. RECAPITULATION ACCORDING TO LOCALITY,

Bennings	11,108
Red Bank	4,765
Jones Landing	1,405
Piney Branch[1]	32
Little Falls Church Branch	5,071
Pierce's Mill	32
Anacostia	39
Piscataway, Md	603
Falls Church	66
Mr. McGuire's 8 caches from Md	353
District of Columbia, Museum Collection, locality unidentified	3,341
	26,815

[1] Mr. Holmes' objects are not included.

III.

Mr. Holmes' theory is that the sole implement sought to be obtained by the workman from this quarry, was the thin, leaf-shaped blade, the result of what he calls the third process. His processes Nos. 1 and 2 for making turtle-backs were according to his theory, only designed to lead up to process No. 3, which should produce the thin, leaf-shaped implement.

I think this conclusion does not accord with the facts. Whatever may have been the intention of the workman in making the single or the double turtle-back by processes 1 and 2, (figs. 1, 2, p. 878,) I feel constrained to believe that these were not stages in the production of leaf-shaped implements. I see no evidence of it. I know of no reason why the aboriginal man might not as well have been making the turtle-back for its own sake. It is found all over the United States, it corresponds in a remarkable degree with prehistoric implements from all parts of the world, and no reason is given why it should not have been as much an implement as were the leaf-shaped blades. I do not believe it possible, by any process suggested by Mr. Holmes, nor by the methods apparent from the examination of the leaf-shaped implements themselves, that they were made from the double turtle-back. Mr. Holmes himself is hazy and uncertain about his third process. It consisted, he says, p. 12, "in going over both sides a second and, perhaps, a third time, securing, by the use of small hammers and by deft and careful blows upon the edges, a rude and symmetrical blade." This might mean chipping, or it might mean pecking, hammering or battering. But the process of pecking, hammering or battering is an abrasion by which the substance is worn away grain by grain, passing off in dust; and we know that the leaf-shaped implements were all made by chipping or flaking, and not by pecking, hammering or battering.

I think I may defy Mr. Holmes to make the double turtle-back into a leaf-shaped implement by the process of chipping without treating it as an natural unworked stone and splitting it down through its center regardless of the edge which had been before made, thus destroying its edge and with it the implement. In this operation, the double turtle-back has no advantage over a natural pebble, and it must be treated as such. The operation of striking the turtle-back on the edge to split it and thereby reduce its thickness, has the effect of reducing its size correspondingly. It will have to be reduced considerably when made from the natural pebble, but it will be subjected to a double reduction in size when made from the

turtle-back. The turtle-back and the leaf-shaped implement are practically the same size, except the latter is only $\frac{3}{8}$ or $\frac{1}{2}$ inches in thickness. This reduction in thickness cannot be done without striking the turtle-back on its edge (Plate XXVI) thus working its total destruction and treating it as if it were an original pebble. The plate will make this apparent.

This argument demonstrates that the pretended evolutionary series of Mr. Holmes set forth in his Plate IV, (My Plate XIX) is incorrect. While all the implements are there truly represented, yet they do not form a continuous series. The leaf-shaped implements in the bottom row, "3rd stage, both sides re-worked," could not be made from the "turtle backs" in the two upper rows. Therefore, I deny Mr. Holmes' fundamental proposition. I am fully persuaded that the maker of these implements, whatever else he intended to do, did not intend or attempt to make the leaf-shaped blades out of the turtle-back, or at least that turtle-backs were not a stage in the process of making leaf-shaped implements. If my proposition in this regard be true it breaks Mr. Holmes' theory in the middle.

IV.

Mr. Holmes Says, p. 17, "that to a limited extent, the rude forms—the turtle-back and its near relation—are also found widely scattered over the Potomac Valley outside of the shops on the hills." The suggestion is that these came from this quarry or from similar quarries, and he charges flat-footed that they were the "rejects," "refuse," "debris," "failures."

In January, 1888, the Smithsonian Institution issued a circular, No. 36, asking of its correspondents throughout the United States and Canada, for information as to the number of these implements in their respective localities. This was accompanied with elaborate description and many illustrations, so there should be no mistake in their identity. Answers were then received, from every state in the United States and some from Canada. A consolidation of these answers, with briefs, was published in the Annual Report of the U. S. National Museum for 1888, pp. 766–702, wherein the number reported up to that time is stated at 8,502. This has been largely in-

creased since, and if now subjected to actual count, would be multiplied many times. Many of the specimens, those of quartz and quartzite or other refractory material, were rude like those from Piney Branch, Holmes' Pl. IV (Pl. XIX), but those made of flint or other homogeneous material which chipped easily, were smooth and clean, and on comparison with paleolithic implements from Europe could scarcely be distinguished; those from Texas and Utah especially so.

Bearing on this question, I chose 72 specimens out of some hundreds of the "double turtle-backs," as Mr. Holmes calls them, collected by Mr. Wm. Hunter from the neighborhood of Mt. Vernon, Va., and have had them photographed and made into a Plate XXVII. The specimens on this plate could be duplicated from almost any state. A comparison will show that the same implements are found in every state in the United States. The hammer-stone in the center happens to have been from Piney Branch. The introduction of this is to show that "the double turtle-backs" are found elsewhere than at Piney Branch in considerable numbers; that they are not isolated and sporadic, and that they are shapely and regular, even when made from the refractory quartzite, so much so as to demonstrate them to have been intentional and not accidental forms, and were neither "rejects," "refuse," nor "failures."

V.

Mr. Holmes refuses to consider the implements as furnishing any evidence of their own antiquity. He refuses to compare them with European or other known paleolithic implements, or to accept them as paleolithic because of any similarity of form, appearance, or mode of manufacture. I agree that all existing evidence should be presented and I suppose this has been done in the present case. Accepting this proposition only for the sake of this argument, my reply is that he then has no evidence of antiquity of any portion of the quarry.

Mr. Holmes contends that this great quarry, nigh a quarter of a mile square, had been dug over and excavated, (as is shown by the section, his Plate I), to an average depth of six feet and in many places to eight and nine feet, along the entire hillside

and around its point. He contends that every cubic foot of this section had been dug over, in places to the bed rock, and the stones and clay handled and worked. All the boulders and earth had been loosened and shovelled, and the entire mass re-deposited by the diggers, as the work progressed. Mr. Holmes not only admits this anterior disturbance, but claims it as giving the chief importance to his discovery. His Plate III, a photograph of the quarry face, is introduced by him to demonstrate this prior excavation.

But all this has naught to do in showing the antiquity of the quarry. If he refuses consideration and comparison of the implements and objects found therein, there is nothing to show that all this excavation, trench making and stone breaking may not have been done in comparatively modern times. There is nothing to indicate its antiquity unless it be the appearance of the surface, and this is only by the thickness of soil and the size of the trees; and both of these may have been, the latter must have been, commenced since the early part of this century.

If these trenches, of such length, depth and extent, had been dug by the modern Indian, as declared by Mr. Holmes, we can scarcely imagine that it would have been filled up, raked down, and smoothed over to a regular slope as it now is and was when the trees began to grow on it. Mr. Holmes' Plate I shows the regularity of this slope correctly. Where Mr. Holmes' greatest trenches were dug, the slope from the top of the hill to the bottom is regular and true without any ridge or hollow to indicate an open trench or pit left by the Indian who is alleged to have made it. By whomsoever that quarry was opened and whoever dug those trenches, they were afterward filled up and smoothed over, leaving no break or depression affecting the regularity of the outline of the hill-side. Our knowledge of the modern Indian teaches us that he would not perform this, to him, useless labor. This profound disturbance (the French call it *remaniement*) of the bowlders, clay and earth of the section, leaves no stratification and destroys all evidences of the age of the deposit. There is no fauna.

This, with the item just mentioned, leaves us without evidence as to the antiquity of the quarry work except as furnished by the implements themselves. Their rejection as evidence would leave the question of its antiquity unanswered, and would render the quarry of slight archaeological value. If Mr. Holmes had found stone axes, hatchets or gouges, spear or arrow heads, pieces of pipes, or fragments of pottery, these would have served as evidence of Indian origin, but the utter absence of any of these leaves the Indian theory unsupported; It is a canon of prehistoric archæology, verified by every worker in the field, that no such extensive work as claimed for this quarry could have been done by prehistoric man without having left some of his tools, implements or utensils. But here not an implement or weapon fragment of polished or smoothed stone, not an arrow or spear head, nor pottery, was found. Mr. Holmes says (p. 13), "Only one was found * * * (with) a rude stem worked out at the broad end. This specimen was found near the surface. Two other pieces found at considerable depth exhibit slight indication of specialization of form, which, however, might have been accidental." And this was all.

If it be said that this was a quarry for bowlders with which to make these implements, and that their finding in the disturbed and disarranged deposits is evidence of this fact, I reply, that the surface of the neighborhood is covered with the same kind of bowlders and many of the same kind of implements, and there is no more evidence to show that the implements were made in the quarry than there is that they were made on the surface. For anything shown in the quarry, the whole batch of turtle-backs, double and single, flaked stones, waste, debris, etc., etc., might have been originally on the surface, made there, possibly, in times of antiquity and been tumbled into the ditch, whenever it was filled up.

VI.

Mr. Holmes' paper is radical and final. He not only determines every proposition presented by the implements found at Piney Branch, but he determines them finally, and further

discussion is useless. According to him, we know (from his investigation) all about these implements, all about the man who made them, the race to which he belonged, his use of tools, his machinery and mode of manufacture, his transportation, and a large suggestion concerning his culture. If his conclusion be correct, then Mr. Holmes has determined the entire history of this man as well as that of the implements themselves. His statement is no longer a theory, it is a demonstrated proposition, a proved problem, the work is finished and the book is sealed. It is submitted that this is a greivous mistake.

VII.

I do not attempt any argument to account for this quarry or to explain either the manufacture or use of its implements. It is not my discovery, and I am in no wise bound to sustain or uphold it.

In the discussion, I have said no word about Paleolithic man in America. That question is not involved here. I have elsewhere set forth my opinion on that subject, and I may enlarge upon it on some other occasion, but not here or now.

I have sought only to criticise the theories of Mr. Holmes in reference to the quarry and its implements, and to show what I deem to be the errors in his conclusions, and in doing so I have avoided personalities. I have indulged in no maligning or abusive words, have conceded to him the most honorable intentions, and a truthful rendering of all his facts; and professing for him the kindest and most friendly feeling, I assert that in what I have said, I have given my own fair, and, as far as possible, unbiased opinion and judgment, being moved thereto solely in the interest of truth and science.

FOSSILS AND FOSSILIZATION.

BY L. P. GRATACAP.

II.

(*Continued from page 912*).

The replacing and mineralizing influence of surface waters may preserve bones which would otherwise quickly disappear. At Big Bone Lick in Kentucky the great numbers of bones of the buffalo are found according to Prof. Shaler "near the present position of the springs and never at any depth beneath the surface." These bones are in some places "massed to the depth of two feet or more, as close as the stones of a pavement, and so beaten down by the succeeding herds as to make it difficult to lift them from their bed." The attraction of this locality for the herds of wild animals spread through the forests of Kentucky in plistocene and recent times, arose from the saline encrustations made by the natural brines which spring to the surface at this point. There is an ossuary of their remains, the mastodon and elephant bones being upon the higher levels and the buffalo skeletons placed more within the swampy basin, which has itself undergone denudation since the advent of the great proboscideans. These bones are impregnated with salt[1] and have become partially mineralized, while the salt solution itself neutralizes any vegetable acids arising from the decomposition of the reeds which, according to Mr. Cooper, accompany the bones. Yet the falling into swamps or bogs of the great mammals and their gradual submersion and burial in the deeper layers of the tenacious and yielding mixture, has been a means of preserving their remains, especially, as besides their partial immunity from the action of organic acids, their great bones have formed, from their formidable size and texture, an irreducible nucleus. But

[1] The preservation of the bones of the Megalonyx in the Big Bone cave in Tennessee, may be partially ascribed to the presence of large quantities of saltpeter earth.

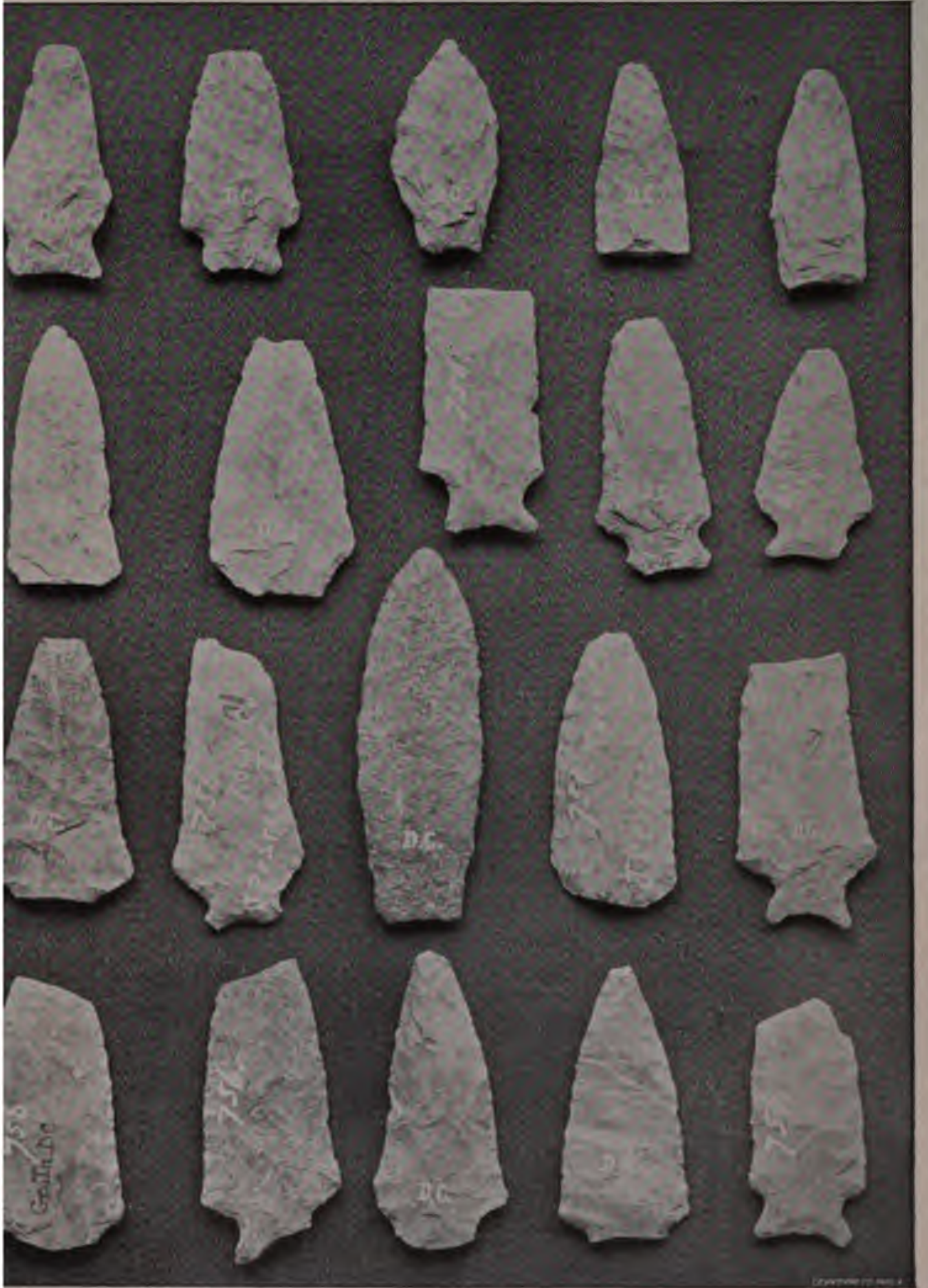
when we reinvest this continent with herds of wild animals, gregarious in habit, and probably reaching a great numerical aggregate, it seems at first singular that their entire skeletons should be so infrequent. The Mastodon, the Elephant, the Musk Ox,[2] the Caribou Moose[3], and the Reindeer, Horse, Buffalo and Mylodon have been distributed in plistocene and recent times as far south as Kentucky, yet except under peculiar circumstances of sepulture, their remains have disappeared. The conclusion is irresistible that the placement of the bones of vertebrates upon the surface of the ground is unfavorable to fossilization, that they must be covered in by deposits, and while thus held together become hermetically sealed against the accidents of surface conditions and the solution by carbonated and acid waters. The rhinoceros and elephant which were disappearing from Sumatra at the time of Mr. Wallace's visit had, after so recent a withdrawal, left few traces more than crania, tusks and teeth. Prof. Nordenskiold speaking of the polar regions pertinently remarks, "the Polar bear and the reindeer are found there in hundreds, the seal, walrus and white whale in thousands, and birds in millions. These animals must die a 'natural' death in untold numbers. What becomes of their bodies? Of this we have for the present no idea."

The isolated death of individuals from packs of wild animals or the death of those less social in instinct, does not, under most circumstances, insure preservation. When some spot chosen for its proximity to water, or because of its fertility and nourishing vegetation, becomes a rendezvous of groups of animals, the herbivores being followed by the beasts of prey, and the region thus frequented is so situated as to receive the

[2] The Musk Ox, *Ovibos cavifrons* Leidy, was found in Loess of Iowa at Council Bluffs, twelve feet below the surface; also at Ft. Gibson, I. T., St. Louis, New Madrid, Mo., Ohio, Big Bone Lick, Ky. These specimens afforded little else than the head, separated vertebræ and leg bones.

[3] Bones of the fossil elk or moose have been found at Big Bone Lick, Kentucky, but it was reserved for Prof. W. B. Scott, of Princeton College, N. J., to obtain the magnificent example of *Cervalces americanus* Harlan now exhibited in the cabinet of that institution. This almost complete skeleton of a very large extinct elk or moose was discovered in a *shell marl deposit under a bog* at Mt. Hermon, N. J. (See Proc. Acad. Nat. Sci. Phila., 1885, p. 181).

PLATE XXIII.



cimens from a cache found near Pierce's Mill, Rock Creek, D. C. Porphyritic felsite.

PLATE XXIV.

Representatives of eight caches of leaf-shaped implements found in Maryland. None quartzite.

PLATE XXV.

How some leaf-shaped implements are made into "final forms," and some not.

PLATE XXVI.

Leaf-shaped implements cannot be made from "turtle backs" without first destroying them.

PLATE XXVII.

Double "turtle backs" of quartzite found at Mt. Vernon, Va. Samples of thousands, not from Piney Branch Quarry.

drainage of the higher land, a combination of conditions is provided by which a varied fauna may secure an approximately complete representation in fossils. Such localities as those described by Captain Johnston in his Nyassaland are suggested. He says: "At the close of the dry season when the tall grass has been burnt down and there is little or no cover for the game to hide in, it is really a remarkable spectacle as seen from the deck of a steamer, to watch the great herds of big animals wandering over these savannahs in search of the young verdure springing up amid the charred stubble of the old grass.[4] With an opera glass you may distinguish water-buck, gnu, buffalo, eland, pallah, reed-buck and zebra, and occasionally some dark blue-gray blobs, much larger than the other specks and forms which are in their vicinity, turn out to be elephants." Again he says "game in the shape of antelopes and buffaloes was evidently abundant, and no doubt was attracted to the vicinity of this brackish pool by the flakes of salt which remained on the soil when the water had evaporated; and the game in its turn was followed by hyenas, lions and vultures."[5]

In geological time such localities would have afforded a rich commixture of fossil remains if the circumstances favorable for the deposition of a protecting stratum of earth existed. Yet in all instances the social relations of the animals have an importance, and those social relations are somewhat modified by the topography of the country they inhabit. The wide plains of south-eastern or subcentral Africa, with an unchecked communication for miles, numerous rivers and rich vegetation,

[4] Such scenes described by Capt. Johnston are closely imitated in the picture, drawn by W. Boyd Dawkins, of the Bristol Channel in plistocene times, when it was a fertile plain "supporting herds of reindeer, horses and bisons, many elephants and rhinoceroses, and now and then being traversed by a stray hippopotamus, which would afford abundant prey to the lions, bears and hyenas inhabiting all the accessible caves, as well as to their enemy and destroyer, man." See also Dr. E. Holub's "Seven Years in S. Africa," Vol. I, p. 267.

[5] See also East Africa and its Big Game by Sir John C. Willoughby, wherein he describes the open plain with buffalo, zebra, hartebeest, eland, rhinoceros, ostrich, Grant's antelope.s teinbock and wart-hog scattered over it, and in another place where he saw "lions, rhinoceros, lesser kudu, wild dogs, hyenas, cheetah, water-buck and zebra, the total seen amounting to nineteen varieties."

permit a most heterogeneous assemblage of animals, whereas a disturbed and mountainous country divided into lofty plateaux, low savannahs and barricaded valleys would separate and isolate related groups, and from a low zoic maxima,[6] in consequence of its irregularity and limited geographical scope, afford a meagre and less diversified fauna, and actually fewer fossils. The successful fossilization of the bones of terrestrial vertebrates can best be secured by their rapid burial within impervious beds of enveloping material wherein they undergo a slow process of partial or complete petrifaction. Nordenskiold in his Voyage of the Vega discovered a very extensive deposit of whale bones in a sand dune upon a beach of Siberia in the Chuckchi country. These apparently had fallen to the bottom of a sea and were entombed by new layers and beds of sand. They were thus subfossil and possessed an immense age. Where they had become exposed by the violence of the waves they were decaying, but where buried in the sand the well preserved bones of these cetaceans were found in innumerable quantities.

The dispersal of bones in the ocean seems as unfavorable to their preservation as exposure on the surface of the earth or in the vegetable acids of its superficial covering. It is notorious that the dredging expeditions deputed to explore the depths of the oceans have seldom encountered the remains of vertebrate animals. The skeletons of whales, seals, porpoises and sharks have not been found commonly, though teeth brought up from these depths indicate the dissolution of the animals in the oceanic waters. So striking is this absence of osseous remains that Lyell remarks "there are regions at present, in the Indian and Pacific Oceans, coextensive in area with Europe and North America where we might dredge the bottom and draw up thousands of shells and corals without obtaining one bone of a land quadruped."[7]

[6] See AMER. NAT., Vol. XX, p. 1009.

[7] "Some astonishment was created on board the 'Challenger' that the dredge after having dragged over miles and miles of the bottom of the sea, and up and down almost every oceanic basin, should never bring up any bones of fish or whale, or any remains of other large animals which inhabit the sea or whose bodies may have been carried down to the sea." Thalassa, J. J. Wild, p. 133.

The explanation of this fact has hardly advanced further than the theoretical stage. It may be attributed to the dissolving agency of carbonic anhydride in the lower strata of water, to the insidious action of the products of organic change, which Julien seems inclined to exaggerate, or to the more ordinary factors of animal consumption. Prof. Verrill and Mr. Sanderson Smith hold that the disappearance of bone in the ocean is due to its being attacked and eaten by crustacea.[8] If this is true and solution has no practical bearing as an agent in their disappearance, it seems likely that some maceration and softening produced by pressure and soakage reduce the bone to such a consistency as to render it more easily attacked by these animals, and incidentally render the bones themselves liable to separation and absorption by the sea waters. It is peculiar that while great numbers of shells are raised from the bottom of the ocean, the same areas seldom or never produce mammalian relics, the otoliths or ear bones of whales, and the teeth of sharks excepted. The skeletons of sharks and fishes might naturally undergo softening or become from their semicartilaginous nature the prey of smaller animals, but the hard parts of whales seem well calculated to resist attack. These larger animals upon their death may be dragged by submarine currents into the deeper parts of the ocean and there become subjected to a stronger chemical action than is observable at less depths. The theory of Mr. Murray as to the formation of coral reefs would make it appear that bottom waters contain more carbonic acid than those on the surface, but this Prof. Dittmar[9] calls in question, thinking that solution is effected by the prolonged contact with the sea water itself. At any rate "the alkalinity of bottom waters was

[8] It is a matter of some interest to learn that "remains of the Atlantic Walrus in a fossil state, have been found at various points along the Atlantic Coast from Maine to South Carolina, and in Europe as far south as England and France" (J. A. Allen). The most striking instance is that of a skull found on the sea beach at Long Branch, New Jersey, of which Dr. Leidy said it "has lost a porsion of the cranium proper, and the exserted portion of one tusk, but otherwise, except being a little water worn, is in a good state of preservation."

[9] The Physics and Chemistry of the voyage of H. M. S. Challenger, Vol. I, pt. 1, Composition of Ocean Water by Prof. W. Dittmar.

found to be distinctly greater than that of those from the surface, and this increase was exactly proportional to the larger quantity of lime present in the former," and upon this fact we might found a belief that bones disappeared through solution. At considerable depths pressure would greatly reinforce chemical action, and as carbonic anhydride is liquified under a pressure of something over 38 atmospheres, in the deeper basins of the ocean this reagent may exist as a liquid. In a French experiment, water, taken from a great depth of ocean, was under so high a tension from the enclosed gas, that upon release it spurted in a jet from the containing vessel.

The remains of terrestrial vertebrates represent most generally the submergence and death of the living animals themselves, and Lyell has well described the way. He says (Principles, (Vol. II, p. 542), "river inundations recur in most climates at very irregular intervals and expend their fury on those rich alluvial plains, where herds of herbivorous quadrupeds congregate together. These animals are often surprised, and, being unable to stem the current, are hurried along until they are drowned, when they sink at first immediately to the bottom. Here their bodies are drifted along, together with sediment, into lakes or seas, and may then be covered by a mass of mud, sand and pebbles thrown down upon them.

"Where the body is so buried in drift sand, or mud accumulated upon it, as never to rise again, the skeleton may be preserved entire; but if it comes again to the surface while in the process of putrefaction, the bones commonly fall piecemeal from the floating carcass, and may in that case be scattered at random over the bottom of the lake, estuary, or sea, so that a jaw may afterwards be found in one place, a rib in another, a humerus in a third—all included, perhaps, in a matrix of fine materials where there may be evidence of very slight transporting power in the current, or even of none, but simply of some chemical precipitate."

Entire skeletons of animals or their bones scattered over declivities or plains are not so likely to be gathered together and deposited in some spot, because, as we have seen, they become subject to decay and dissipation., and because it would

appear that any wide-spread catastrophes killing large numbers of a herd are rare, and in the case of individual deaths, the remains, if carried by freshets to some lower level, would seldom undergo the same vicissitudes and be buried at the same point. Yet the wholesale destruction of mammals in a state of nature may be considered a possibility, though improbable. Sir Samuel H. Baker speaks[10] of cattle introduced at his camp at Fatiko, in Africa, who could not live there, "as the herbage was quite different to that to which they had been accustomed." They died so rapidly and in such numbers that in three months only three or four remained out of as many thousand. In the over stocked ranches of California, thousands of heads of sheep have been seen lying dead in vast heaps in ravines and valleys, as if nourishment had become exhausted by draught or by actual deplenishment of the available pasturage. Is not a similar mortality possible under natural conditions when, as in the case of the Fatiko cattle, animals have been driven by storms into localities incapable of their support, or when, as with the ranch sheep of California, an area previously put under severe strain for the support of its feral population, by an accident of weather or season fails entirely to furnish its occupants with food?

Wallace in his Malay Archipelago speaks of the destructive effects of drought upon animal life in the Arne Islands, where from an excessive scarcity of water, "sometimes hundreds of birds and other animals die." The effects of sudden and violent falls of hail and snow are noticed by Stansbury, who found on the shores of the Great Salt Lake, a large number of young pelicans killed by the severity of a hail storm. The possibility of numbers of animals becoming buried at once, may be illustrated in the condition of the banks of the River Vaal in South Africa, of which Dr. Holub says that "its banks almost to the very middle of the channel are so soft and slippery, that draught animals going to drink are liable to sink so deep into the mud that it is impossible to extricate them." The internecine strife of wild animals may itself result in the accidental death of numbers, as when the hunters

[10] Ismailia, Sir S. H. Baker. p. 294.

—the carnivora—pursue their prey and drive them into lakes or rivers or, perhaps, force them over precipices. Dr. Hayden has observed similar occurrences, and in the same place where he records this observation corroborates the interesting suggestion of Lyell as to animals meeting their death by falling through thin ice. He says: "The wolves watch the deer, antelope and other feebler animals as they go down to the streams to drink, and all over the wide bottoms are the skeletons of these animals in a more or less perfect condition. It is not an uncommon occurrence for a band of wolves to attack an aged buffalo too old to offer a successful resistance. He must always betake himself to the river, where he is not unfrequently drowned, or is destroyed by the wolves on a sand bar or island. Annually thousands of buffaloes are drowned in attempting to cross the Missouri on the ice, as it is breaking up in the spring. Their bodies have been seen floating down the Missouri at Fort Union and Fort Clark by hundreds, and lodging on some of the islands or sand bars in the river. In the spring of 1858 several thousand bodies of buffaloes passed down the Kansas River below the mouth of Solomon's Fork and were carried into the Missouri."

Dr. Holub speaks of the devastation amongst oxen, elands, hartebeests, sheep, goats, wild pigs, etc., wrought by the attacks of hyenas in South Africa as "really frightful," and in pliocene times on our globe similar causes may have led to the collection of important groups of fossils. Even the instincts of animals may lead to their wholesale destruction. Mosely speaks of the migration of turtles at Ascension Islands saying, "the young turtles on leaving the egg go down to the sea and disappear, returning only when full grown to breed; this is the account given by the residents. If they do really leave the neighborhood of the island, there seems no possible means by which they can find their way back."

Although it is impossible that any of the fish beds, found as fossils, can, at least in paleozoic or mesozoic rocks, have been formed in the way instanced by Nordenskiold as a cause of death amongst arctic fish, yet the circumstance is intrinsically interesting, and the the reflections it suggests as to the likeli-

hood of general destruction from other causes of the fish in geological strata, wherein they abound, make it a useful point of reference in this matter. Nordenskiold says (Voyage of the Vega), " a large number of fish (*Gadus polaris*) were seen above the foot of a large block of ground ice, near which we lay to for some hours. Next day we saw near one of the islands, where the water was very clear, the sea-bottom bestrewed with innumerable fish of the same species. They had probably perished from the same cause, which often kills fish in the river Obi in so great numbers that the water is infected, namely, from a large shoal of fish having been inclosed by ice in a small hole, where the water, when its surface has frozen, could no longer by absorption from the air replace the oxygen consumed, and where the fish have thus been literally drowned."

However accumulated, whether by the sudden death of large numbers of animals by floods and storms along the banks of streams or the margins of lakes, or whether from other natural causes animals perish in numbers and upon restricted areas, their bones are carried by water action into the depressions, sinks, crevices and basins of a country, and being there sealed up from decomposition or dispersion by the silt and gathering accretions of various mineral deposits above them, they become fossils. The caves in limestone districts receive a contribution of animal remains partially brought into them by surface water partially by the the predatory instincts of carnivorous mammalia or birds. Prof. Hartt in his Geology of Brazil instances the interesting examples in the Sao Francisco basin wherein the numerous caverns, extending sometimes two thousand feet into the rock, furnish abundant remains both of long extinct mammalia as the Glyptodon, Mastodon, Mylodon, Megatherium, Chlamydotherium, Toxodon and Macrauchenia, and innumerable remnants of the smaller living animals brought there by owls, whose bones mingle with those of other occupants, as bats and felidæ. These variously distributed in the different caves were mingled in a red clay earth more or less cemented and encrusted by a stalagmitic crust. Similarly fossil vertebrates have been entombed in the caves of England, France, Belgium, Spain,

Sicily, Germany, Russia and Australia. Many of these caves open by vertical fissures to the surface, and down these irregular holes and chimneys the bones have been washed, while in some cases as at Wirksworth, in Derbyshire, England, or at the *cave* in the limestone at Port Kennedy, Pa., described by C. M. Wheatley, animals may have plunged into them and died in their imprisonment. Once gathered in their final resting place the process of burial goes on, in many cases rapidly, and in others slowly, and, according to the completeness of their sepulture, the condition of the bone is more or less perfectly preserved. In river floods the animals or osseous debris borne forward in their waters are soon enveloped in the midst of the accompanying clay, sand, gravel and calcareous mud torn from the channel and banks of the stream. They sink to the bottom and are rapidly covered in by a rising blanket of deposition. In caves and hollow receptacles the infiltrating streams bring constant additions of mud, and by their erosive action upon the surrounding limestone which they dissolve, they redeposit carbonate of lime through the interstices of the granular accumulation or form a hard layer above it, upon which again later ossuaries may be made, and the cave floor offer a study of separate and successive periods.

Amongst the multitudinous details connected with the exploration of caves for the traces and evidence of prehistoric man, the following facts have some reference to the fossilization of terrestrial vertebrates. A cave at Gailenreuth, Germany, contains an enormous quantity of bones and teeth of animals formerly living in its vicinity, and according to Dr. Buckland, introduced by a stream which passed through this chain of caverns in its subterranean course to lower levels. In the cave of Kühloch, Germany, a black animal dust covers the whole floor to the depth of six feet, derived says the same authority "from comminuted and pulverized bone." This accumulation is attributed to the use of the cave by bears through centuries.[11] In the caves of North Wales near St.

[11] The habits of wild animals in resorting to caves is fully established by observation. The custom of the panthers to make lairs of natural caves is instanced by Prof. Baird, and Major Pinto speaks of a circular chamber in a limestone mountain in Western Africa as a "regular haunt of wild beasts, as one might

Asaph, the bones are in a similarly pulverulent state and produce clouds as they are disturbed. In a cave at Banwell in the Mendip Hills, England, thousands of bones of bison, horse and reindeer were taken out of a red silt which filled the cave to its roof. The entire deposit has been introduced by water through a vertical fissure which opened on the surface. In the Hyena Den, at Wookey Hole, "the organic remains were in all stages of decay, some crumbling to dust at the touch, while others were perfectly preserved and had lost very little of their gelatine." In an arm or section of this same cavern according to Dawkins, "most of the bones were as soft as wet mortar," an interesting statement which throws light upon the probable state of maceration which bones attain before disappearance by trituration or solution. The mineralization of the bones in the various caves, so patiently explored, presents striking differences. In some the bone seems reduced to the last stages of cohesion, while in others it has become filled with carbonate of lime or partially silicified, and attains a considerable gravity.

(*To be continued.*)

THE GEOGRAPHICAL DISTRIBUTION OF BATRACHIA AND REPTILIA IN NORTH AMERICA.

By E. D. Cope.

(*Continued from page 902.*)

III. The Eastern Subregion.

The fauna of Batrachia and Reptilia of this subregion is characterized by what it lacks as much as by what it possesses. The number of species which occupy its entire extent exclu-

judge from the air which was perfectly saturated with the pungent smell of certain animals, as well as from the traces of a lion impressed on the impalpable powder which covered the ground, where we met with a few quills of the *Hystrix africana*."

sively of other subregions is small, while a larger number are restricted to parts of it. Verrill divided it into four districts, viz.: the Carolinian, the Alleghenian, the Canadian, and the Hudsonian. These are distinguished by the ranges of mammals and reptiles, and the breeding-places of birds. The Carolinian fauna extends in a belt north of the Austroriparian subregion, from Long Island, south of the hill region of New Jersey, to the southeastern corner of Pennsylvania, and thence inland. It embraces a wide belt in Maryland and Virginia, and all of central North Carolina, and then narrows very much in passing round south of the Alleghenies of Georgia. It extends north again, occupying East Tennessee, West Virginia, Kentucky, Indiana, the greater parts of Illinois and Ohio, and the southern border of Michigan. It includes southern Wisconsin and Minnesota, all of Iowa, and the greater part of Missouri. The Alleghenian embraces the States north of the line just described, excepting the regions pertaining to the Canadian fauna, which I now describe. This includes northern Maine, New Hampshire and Vermont, with the Green Mountains, the Adirondacks and summits of the Allegheny Montains as far as Georgia. It includes Canada east and north of the lakes. The Hudsonian fauna is entirely north of the isothermal of 50°. It has great extent west of Hudson's Bay, and is narrowed southeastward to Newfoundland.

The information as to the distribution of the Batrachia and Reptilia now at hand, points to the following conclusions. The Hudsonian fauna need not be further referred to here, as it is part of the Holarctic region. The Canadian is sustained, as defined by the range of certain Batrachia. The demarkation between the Alleghenian and Carolinian is determined by the northern limit of most of the species common to the Eastern and Austroriparian subregions. An important division is indicated by the boundaries set to the range of certain species by the Allegheny Mountains. This division affects chiefly the Carolinian district of Verrill, and I therefore propose to abolish that name, and replace it by the two terms Cisalleghenian for Eastern, and Transalleganian for the Western districts. They are separated from each other by the Alleghenian district of

the foot hills, and the Canadian of the summits of the Allegheny Mountains.

The species which are found over the entire eastern subregion, and not elsewhere, are the following:

Amblystoma jeffersonianum Green.
Plethodon cinereus Green.
Rana silvatica Lec.
Rana palustris Lec.
Osceola doliata triangula Boie.
Natrix fasciata sipedon Linn.
Eutænia sirtalis graminea Cope.

The Canadian district is characterized by the following species, which are restricted to it:

Amblystoma jeffersonianum laterale Hallow.
Gyrinophilus porphyriticus Green.
Desmognathus ochrophæa Cope.
Desmognathus nigra Green.
Bufo lentiginosus fowlerii Putn.
Rana cantabrigensis Baird.
Rana septentrionalis Baird.

The list above given as universally distributed in the Eastern subregion characterizes *the Alleghenian district.* I know of no species that is restricted to it. The genera which do not extend north of it are the following:

BATRACHIA:

Chorophilus,
Hyla,
Hemidactylium,
Cryptobranchus.
Necturus.

SAURIA:

Sceloporus,
Eumeces.

SERPENTES:

Carphophiops,
Coluber,
Cyclophis,
Natrix,
Ophibolus,
Heterodon,
Ancistrodon.
Systrurus,
Crotalus,

The two remaining districts include the large number of species which are common to the Eastern and Austroriparian subregions enumerated under the latter head. The Cisalleghenian is further characterized by the following:

Hyla andersonii Bd.
Rana virgatipes Cope.
Ophibolus rhombomaculatus Holbr.

To these must be added from the Austroriparian list:
Abastor erythogrammus Daud.

The following species are peculiar to the *Transalleghenian district:*

Chondrotus microstomus Cope.
Spelerpes maculicaudus Cope.
Rana areolata circulosa R. & D.
Carphophiops vermis Kenn.
Coluber vulpinus B. & G.
Ophibolus calligaster Say.
Eutænia radix B. & G.
Eutænia butlerii Cope.
Tropidoclonium lineatum Hallow.
Natrix kirtlandii Kenn.
Systrurus catenatus Raf.

Probably *Eutaenia brachystoma* Cope belongs to this district but only one specimen has been found.

The following species enter this district only from the Austroriparian:

Natrix grahamii B. & G.
Eutænia proxima Say.

Of the species peculiar to the Transalleghenian district, *Ophibolus calligaster* and *Tropidoclonium lineatum* extend into the northern limits of the Texan district.

The genera which do not range northward of the Cisalleghenian district are Cnemidophorus, Liolepisma and Abastor.

The total number of species of the Eastern subregion is thus:

Generally distributed,	7
Peculiar to Cisalleghenian,	3
Peculiar to Transalleghenian,	9
Peculiar to Canadian,	7
Common to Austroriparian,	34
	60

IV. The Austroriparian Subregion.

This subregion is the range of a large number of species of Batrachia and Reptilia, only a part of which occupy it to the exclusion of all other subregions, and another series of which occupy parts only of its area. Three centers of distribution

within its borders may be discerned—the Ocmulgian, the Louisianian and the Texan. The Texan is especially characterized by the combination of the Austroriparian fauna with a considerable number of the species of the Sonoran subregion. The characteristic Austroriparian species are the following:

TRACHYSTOMATA:

Siren lacertina L.

URODELA:

Amphiuma means Gard.
Amblystoma talpoïdeum Holbr.
Manculus quadridigitatus Holbr.

SALIENTIA:

Bufo lentiginosus lentiginosus Shaw.
Chorophilus occidentalis B. & G.
Hyla carolinensis Penn.
Engystoma carolinense.

LORICATA:

Alligator mississippiensis Daud.

SAURIA:

Ophisaurus ventralis Daud.
Anolis carolinensis.

SERPENTES:

Heterodon simus Linn.
Cyclophis æstivus Linn.
Zamenis flagelliformis Catesb.
Coluber spiloides D. & B.
Compsosoma corais couperii Holbr.
Osceola doliata syspila Cope.
Osceola doliata coccinea Schl.
Ophibolus getulus sayi Hobr.
Cemophora coccinea Blum.
Natrix clarkii B. & G.
Natrix fasciata fasciata L.
Natrix fasciata erythrogaster Shaw.
Natrix cyclopium D. & B.
Virginia valeriæ B. & G.
Haldea striatula L.
Tantilla coronata B. & G.
Elaps fulvius L.
Ancistrodon piscivorus Lacep.
Systrurus miliarius L.
Crotalus adamanteus adamanteus Beauv.

Thirty-one species and subspecies.

The Austroriparian shares with the Floridan subregion all of the above species except *Coluber spiloides*, *Natrix clarkii*, *Virginia valeriæ* and *Haldea striatula*, so far as yet known. It shares with the Eastern subregion the following thirty-four species.

PROTEIDA:

Necturus maculatus Raf.

URODELA:

Cryptobranchus alleghaniensis Daud.
Amblystoma opacum Grav.
Amblystoma punctatum L.
Amblystoma tigrinum Green.
Plethodon glutinosus Green.
Spelerpes guttolineatus Holbr.
Spelerpes ruber Daud.
Desmognathus fusca Raf.
Diemyctylus viridescens Raf.
Bufo americanus americanus Lec.
Scaphiopus holbrookii Harl.
Acris gryllus Lec.
Hyla versicolor Lec.
Rana pipiens pipiens Kalm.
Rana areolata B. & G.
Rana clamata Daud.
Rana catesbiana Shaw.

SAURIA:

Sceloporus undulatus Latr.
Cnemidophorus sexlineatus L.
Eumeces quinquelineatus L.
Liolepisma laterale Say.

SERPENTES:

Abastor erythrogrammus Daud.
Carphophiops amœnus Say.
Heterodon platyrhinus Latr.
Diadophis punctatus L.
Liopeltis vernalis L.
Zamenis constrictor L.
Coluber obsoletus Say.
Pityophis melanoleucus Daud.
Ophibolus getulus getulus L.
Eutænia sirtalis sirtalis L.
Ancistrodon contortrix L.
Crotalus horridus L.

The following species are restricted to the eastern part of the Austroriparian subregion, not extending west of the Atlantic drainage. To this district I have the name of *the Ocmulgian.*

PROTEIDA:

Necturus punctatus Gibbs.

URODELA:

Stereochilus marginatum Hallow.
Chondrotus cingulatus Cope.

SALIENTIA:

Bufo quercicus Holbr.
Chorophilus ornatus Holbr.
Chorophilus oculatus Holbr.

SERPENTES:

Abastor erythrogrammus Daud.
Rhadinæa flavilatus Cope.
Coluber quadrivittatus Holbr.
Natrix rigida Say.

The following species are restricted to the Ocmulgian and Louisianian districts with present information. First, all the Batrachia which the Austroriparian subregion shares with the Eastern, excepting *Amblystoma tigrinum*, *Diemyctylus viridescens*, *Acris gryllus*, *Rana areolata*. Second, *Farancia abacura* Holbr., *Coluber guttatus* L.

The following species are to be added to the general Austroriparian (p. 1007) to form the list of the Texan district:

PROTEIDA:

Typhlomolge rathbunii Stejn.

URODELA:

Diemyctylus meridionalis Cope.
Chondrotus texanus Matth.

SALIENTIA:

Bufo debilis B. & G.
Bufo punctatus B. & G.
Bufo valliceps Wiegm.
Bufo compactilis Wiegm.
Lithodytes latrans Cope.
Chorophilus triseriatus clarkii B. & G.

SAURIA:

Holbrookia texana Trosch.
Holbrookia maculata B. & G.
Crotaphytus collaris Say.
Sceloporus spinosus Wiegm.
Sceloporus consobrinus B. & G.
Phrynosoma cornutum Harl.
Eublepharis variegatus Bd.
Gerrhonotus liocephalus Wiegm.
Eumeces epipleurotus Cope.
Eumeces pachyurus Cope.
Eumeces brevilineatus Cope.
Eumeces tetragrammus Bd.
Eumeces obsoletus B. & G.

SERPENTES:

Diadophis amabilis docilis B. & G.
Diadophis amabilis stictogenys Cope.
Hypsiglena ochrorhynchus Cope.
Rhinochilus lecontei B. & G.
Coluber emoryi B. & G.
Osceola doliata annulata Kenn.
Ogmius episcopus episcopus Kenn.
Natrix rhombifera Hallow.
Natrix fasciata transversa Hallow.
Virginia elegans Kenn.
Eutænia proxima Say.
Eutænia elegans marciana B. & G.
Eutænia eques ocellata Cope.
Tantilla gracilis B. & G.
Tantilla nigriceps Kenn.
Systrurus catenatus edwardsii B. & G.
Crotalus adamanteus atrox B. & G.

Sixty-one species and subspecies, making a total for the Austroriparian as follows:

Generally distributed,	31
Shared with the Eastern subregion,	34
Ocmulgian only,	10
Louisianian and Ocmulgian only,	2
Texan exclusively (in the subregion),	38
	115

The species which enter the Texan territory from the Sonoran extend to various distances to the north and east. Thus, *Crotaphytus collaris* ranges to southern Missouri, and *Holbrookia maculata* to Arkansas. *Sceloporus spinosus* extends along the Gulf States to western Florida. *Phrynosoma cornutum* extends eastward to Dallas, Texas. *Rhinochilus lecontei* on the other hand has not been found east of Austin. Several species from the extreme southwest of Texas have not been included in the above lists, since some of them are well-known to belong to the Central American fauna, while the range of others is probably similar, but is not sufficiently known. Of the former kind are *Drymobius margaritiferus* Schl., *Sibon albofuscum* Lac., and *Coniophanes imperialis* B. & G.; of the latter are *Lystoptychus lateralis* Cope, *Holbrookia propinqua* B. & G. and *Hypopachus cuneus* Cope.

V. The Floridan Subregion.

The species and subspecies peculiar to this subregion are the following:

Batrachia:

Pseudobranchus striatus Lec.
Hyla gratiosa Lec.
Rana areolata æsopus Cope.

Sauria:

Eumeces egregius Bd.
Rhineüra floridana Bd.

Serpentes:

Coluber rosaceus Cope.
Coluber guttatus sellatus Cope.
Osceola doliata parallela Cope.
Stylosoma extenuatum Brown.
Eutænia sackenii Kenn.
Seminatrix pygæa Cope.
Natrix usta Cope.
Natrix compressicauda Kenn.
Natrix fasciata pictiventer Cope.
Liodytes allenii Garm.

Species which are wanderers from the West Indian region are:

Lithodytes ricordii D. & B.
Sphærodactylus notatus Bd.
Crocodilus americanus Seba.

The *Rhadinæa flavilatus* Cope ranges throughout both the Floridan subregion and the Ocmulgian district. Two other species may be characteristic of the Floridan subregion, but only one specimen of each has been obtained. These are *Manculus remifer* Cope, and *Elaps distans* Kenn.

Species which the Floridan subregion shares with the Austroriparian are the following:

TRACHYSTOMATA:

Siren lacertina L.

AMPHIUMOIDEA:

Amphiuma means Gard.

PSEUDOSAURIA:

?*Plethodon glutinosus* Green.

SALIENTIA:

Bufo lentiginosus lentiginosus Shaw.
Bufo quercicus Holbr.
Hyla squirella Bosc.
Hyla femoralis Latr.
Hyla carolinensis Penn.
Acris gryllus Lec.
Chorophilus nigritus Lec.
Scaphiopus holbrookii Harl.
Rana pipiens sphenocephala Cope.
Rana catesbiana Shaw.

LORICATA:

Alligator mississipiensis Daud.

SAURIA:

Sceloporus undulatus Latr.
Cnemidophorus sexlineatus L.
Liolepisma laterale Say.
Eumeces quinquelineatus L.

SERPENTES:

Heterodon simus L.
Diadophis punctatus L.
Abastor erythrogrammus Daud.
Farancia abacura Holbr.
Coluber guttatus L.
Coluber quadrivittatus Holbr.
Zamenis constrictor L.
Zamenis flagelliformis Shaw.
Compsosoma corais couperii Holbr.
Pityophis melanoleucus Daud.
Ophibolus getulus getulus L.
Osceola doliata coccinea Schl.
Osceola elapsoidea Holbr.
Storeria dekayi Stor.
Natrix fasciata erythrogaster Shaw.
Natrix cyclopium D. & B.
Natrix taxispilotus Holbr.
Eutænia sirtalis sirtalis L.
Tantilla coronata B. & G.
Elaps fulvius L.
Sistrurus miliarius L.
Crotalus adamanteus adamanteus L.

The total number of species of the Floridan subregion is as follows:

Peculiar species, ...	15
Species common to the Ocmulgian district, . .	1
Species common to the Louisianian district, ...	40
Species common to the West Indian region, . .	3
Little known species, ...	2
	61

VI. The Sonoran Subregion.

This subregion presents several natural divisions, as follows: I. The Lower Californian district, including only the region at the extremity of the peninsula of Lower California; II. The Chihuahuan district, embracing the State of Sonora, Mexico, the northern part of the Mexican Plateau, Arizona south of the San Francisco Mountains, most of the peninsula of Lower California, and most of New Mexico; III. The Basin district, embracing the Great Basin of Utah and Oregon, to Vernon, British Columbia; and IV. The Central district, which includes the high plains east of the Rocky Mountains, from Texas northward, excepting the river bottoms which cross it from west to east. This great subregion is bound together by the general distribution of numerous genera; but I do not know a single species which covers its entire area which is not found elsewhere. These define the districts.

The Lower Californian district is defined by the following fourteen species, which are restricted to it:

Hyla curta Cope.
Ctenosaura hemilopha Cope.
Uta thalassina Cope.
Uta nigricauda Cope.
Phyllodactylus unctus Cope.
Cnemidophorus maximus Cope.
Euchirotes diporus Cope.
Lichanura trivirgata Cope.
Zamenis aurigulus Cope.
Phyllorhynchus decurtatus Cope.
Pityophis vertebralis Blv.
Chilomeniscus stramineus Cope.
Tantilla planiceps Blv.
Crotalus enyo Cope.

The district shares with the Chihuahuan the following species:

Bufo punctatus B. & G.
Dipsosaurus dorsalis Hallow.
Crotaphytus wislizenii B. & G.
Callisaurus draconoides Blv.
Sauromalus ater Dum.
Uta stansburiana B. & G.
Uta ornata B. & G.
Sceloporus zosteromus Cope.
Phrynosoma coronatum Blv.
Phyllodactylus tuberculosus Wiegm.
Salvadora grahamiæ B. & G.
Ophilobolus getulus boylii B. & G.
Chilomeniscus fasciatus Cope.
Hypsiglena ochrorhynchus Cope.
Natrix valida Kenn.
Eutænia eques Reuss.
Trimorphodon lyrophanes Cope.
Crotalus adamanteus atrox B. & G.
Crotalus mitchellii Cope.

Species common to the Lower Californian district and the Western subregion (mostly to the Diegan district) are the following:

Hyla regilla B. & G.
Phrynosoma coronatum Blv.
Verticaria hyperythra Cope.
Gerrhonotus multicarinatus Blv.
Opibolus getulus boylii B. & G.
Opibolus getulus californiæ Blv.
Plethodon croceater Cope.

Total species of the Lower Californian district:

Peculiar to it, ...	14
Common to the Chihuahuan district, ...	18
Common to the Western subregion, ...	7
	39

Thirty-eight species, one being twice enumerated as common to the Chihuahuan district and Western region.

The Chihuahuan district possesses the following peculiar species:

BATRACHIA SALIENTIA:

Bufo alvarius Grd.
Hyla arenicolor Cope.

SAURIA:

Ctenosaura multispinis Cope.
Crotaphytus reticulatus Bd.
Callisaurus notatus Bd.
Callisaurus rufopunctatus Cope.
Callisaurus inornatus Cope.
Callisaurus scoparius Cope.
Uta symmetrica Bd.
Uta bicarinata Dum.
Uta graciosa Hallow.
Sceloporus clarkii B. & G.
Sceloporus couchii B. & G.
Sceloporus jarrovii Cope.
Sceloporus ornatus B. & G.
Phrynosoma solare Gray.
Anota modesta Gir.
Anota maccallii Hallow.
Heloderma suspectum Cope.
Gerrhonotus multifasciatus D. & B.
Cnemidophorus tessellatus Say.
Cnemidophorus inornatus B. & T.
Cnemidophorus octolineatus B. & S.
Cnemidophorus guttatus B. & G.
Eumeces guttulatus Hallow.

OPHIDIA:

Glauconia dissecta Cope.
Glauconia dulcis B. & G.
Glauconia humilis B. & G.
Lichanura roseofusca Cope.
Diadophis regalis regalis B. & G.
Heterodon nasicus kennerlyi Kenn.
Zamenis semilineatus Cope.
Coluber emoryi B. & G.
Rhinechis elegans Kenn.
Pityophis sayi sayi Schl.
Epiglottophis pleurostictus D. & B.
Ophibolus getulus splendidus B. & G.
Chionactis occipitalis Hallow.
Chilomeniscus ephippicus Cope.
Gyalopium canum Cope.
Eutænia megalops Kenn.
Eutænia elegans marciana B. & G.
Eutænia elegans dorsalis B. & G.
Eutænia augustirostris Kenn.
Eutænia nigrilatus Brown.
Eutænia rufopunctata Cope.
Eutænia multimaculata Cope.
Trimorphodon upsilon Cope.
Trimorphodon lambda Cope.
Trimorphodon wilkinsonii Cope.
Scolecophis æmulus Cope.
Elaps euryxanthus Kenn.
Crotalus molossus B. & G.
Crotalus scutulatus Kenn.
Crotalus lepidus Kenn.
Crotalus cerastes Hallow.

Fifty-eight species, disposed of as follows: Batrachia salientia, 2; Sauria, 25; Serpentes, 31. Three species of Testudnata are peculiar to this district, viz.: *Kinosternum henrici* Lec., *K. flavescens* Agass., *Xerobates agasizii* Cooper. This district possesses a larger number of peculiar species than any other in the Medicolumbian Region.

The Basin district has but few peculiar species. Its southern boundary may be regarded as the San Francisco Mountains in northern Arizona. The *Crotalus tigris* which is restricted to it has been shown by Merriam to inhabit only the mountains, and its northern limit is as yet unknown. The following are the species of the Great Basin:

BATRACHIA:

Amblystoma tigrinum Green.
Spea intermontana Cope.*
Rana draytonii onca Cope.*
Rana pipiens brachycephala Cope.*

SAURIA:

Crotaphytus collaris Say.†
Crotaphytus wislzenii B. & G.†
Uta stansburiana B. & G.†
Sceloporus biseriatus Hallow.†
Sceloporus graciosus B. & G.†
Sceloporus consobrinus B. & G.†
Phrynosoma douglassii ornatissimum Gird.†
Anota platyrhina Gird.†
Zamenis tæniatus Hallow.†
Pityophis sayi bellona B. & G.†
Chionactis episcopus isozonus Cope.*
Eutænia elegans vagrans B. & G.
Crotalus tigris B. & G.†
Crotalus confluentus lecontei Hallow.

The species and subspecies peculiar to the Basin district are marked with a star, and those found also in the Chihuahuan with a dagger.

The Central district possesses but few peculiar species. These with certain Chihuahuan species give it a distinctive character. There are also a few species which enter it from the Eastern subregion. These are marked with a dagger, while the peculiar forms are marked with a star.

URODELA:

Amblystoma tigrinum Green.

SALIENTIA:

Bufo cognatus Say.*
Spea hammondii bombifrons Cope.*

SERPENTES:

Heterodon nasicus nasicus B. & G.*
Ophibolus multistratus Kenn.*
Zamenis constrictor L.†
Eutænia radix B. & G.†
Eutænia sirtalis parietalis Say.
Eutænia elegans vagrans B. & G.
Crotalus confluentus confluentus Say.

SAURIA:

Crotaphytus collaris Say.
Holbrookia maculata B. & G.
Phrynosoma douglassii hernandesii Gir.*
Eumeces septentrionalis Bd.*
Eumeces multivirgatus Hallow.†
Eumeces obsoletus B. & G.

The species not marked with dagger or star are Chihuahuan, except *Eutænia elegans vagrans*, which is also found in the Basin district, *E. sirtalis parietalis*, which extends to the Pacific district, and the *Amblystoma tigrinum*, which is Medicolumbian throughout.

The total number of species of the Sonoran subregion is as follows:

Peculiar to the Chihuahuan district,	58
Common to Lower Californian and Chihuahuan districts,	19
Peculiar to the Lower Californian district,	14
Peculiar to the Basin district,	6
Common to the Basin and Chihuahuan,	8
Peculiar to the Central district,	8
Common to the Central and Chihuahuan,	3
Common to the Chihuahuan and Texan,	14
	126
Doubles emplois,	4
	122

VII. The Western Subregion.

This subregion presents two distinct modifications, a northern and a southern. The boundary between the two has not yet been defined; it represents the demarkation between the greater humidity of the north and the arid conditions of the south. The name of Diegan has been given by Mr. Van Denberg to the southern region; to the northern I propose to restrict the name Pacific, which I formerly used for the entire subregion, which had been previously named the Western by Baird. The Pacific district extends further south along the Sierra Nevada than in the San Joaquin Valley. Some of the forms of the Diegan district extend north to the latitude of San Francisco, but the majority of the species are restricted to more southern latitudes. How far the Diegan district extends on the Lower Californian Peninsula is uncertain. The separation from the Chihuahuan district is also undertermined, and the species of both districts mingle in some degree on their borders.

Species peculiar to the Diegan district are the following:

Batrachia:

Bufo columbiensis halophila B. & G.

Sauria:

Uta repens Van Denberg.
Uta mearnsii Stejneger.
Sceloporus orcuttii Stejneger.
Sceloporus vandenbergianus Cope.
Phrynosoma cerroënse Stejneger.
Anota goodei Stejneger.
Xantusia vigilis Bd.
Xantusia riversiana Cope.
Xantusia picta Cope.
Zablepsis henshavii Stejneger.
Amæbopsis gilbertii Van Denburg.
Verticaria sericea Van Denberg.
Cnemidophorus tessellatus multiscutatus Cope.
Cnemidophorus tessellatus rubidus Cope.
Anniella pulchra Gray.

Serpentes:

Lichanura orcuttii Stejn.
Diadophis amabilis amabilis B. & G.
Crotalus ruber Cope.

To these must be added the species already enumerated as common to the Diegan and Lower Californian districts, and the following list of species which occur also in the Chihuahuan district:

Crotaphytus wislizenii B. & G.
Callisaurus draconoides Blv.
Uta stansburiana B. & G.
Sceloporus biseriatus Hallow.
Lichanura roseofusca Cope.
Crotalus adamanteus atrox B. & G.

The following species are common to the Diegan and Pacific districts:

BATRACHIA:

Diemyctylus torosus Esch.
Hyla regilla B. & G.*

SAURIA:

Phrynosoma blainvillii Gray.
Gerrhonotus multicarinatus Blv.*
Gerrhonotus burnettii Gray.
Eumeces skiltonianus B. & G.

SERPENTES:

Charina bottæ Blv.
Zamenis lateralis Hallow.
Zamenis tæniatus Hallow.*
Pityophis catenifer Blv.
Ophibolus getulus boylii B. & G.*
Eutænia elegans couchii Kenn.*
Eutænia infernalis infernalis Blv.
Crotalus confluentus lucifer B. & G.

These species are then characteristic of the Western subregion as a whole, except those marked with a star, which occur elsewhere.

The Pacific district is especially characterized by certain genera and species of Batrachia. No certainly known genus of scaled reptiles, and a limited number of species and subspecies are peculiar to it. Conspicuous among these are the species of Eutænia, which display great variety, while they are but sparsely represented in the Diegan district. The peculiar species are as follows:

URODELA:

Amblystoma macrodactylum Baird.
Amblystoma epixanthum Cope.
Chondrotus paroticus Baird.
Chondrotus decorticatus Cope.
Chondrotus aterrimus Cope.
Chondrotus tenebrosus B. & G.
Batrachoseps caudatus Cope.
Batrachoseps attenuatus Esch.
Plethodon intermedius Bd.
Plethodon oregonensis Gird.
Autodax lugubris Hallow.
Autodax iëcanus Cope.
Autodax ferreus Cope.
Diemyctylus torosus Esch.
Bufo columbiensis columbiensis B. & G.
Spea hammondii hammondii Bd.
Rana temporaria pretiosa Bd.
Rana cantabridgensis latiremis Cope.
Rana agilis aurora B. & G.
Rana draytonii Baird.
Rana boylii Baird.

SAURIA:

Sceloporus undulatus occidentalis Bd.
Phrynosoma douglassii douglassii Bell.
Gerrhonotus principis B. & G.
Cnemidophorus septemvittatus Cope.

SERPENTES:

Diadophis amabilis pulchellus B. & G.
Zamenis constrictor vetustus B. & G.
Contia mitis B. & G.
Eutænia elegans elegans B. & G.
Eutænia elegans lineolata Cope.
Eutænia elegans ordinoides B. & G.
Eutænia infernalis vidua Cope.
Eutænia sirtalis parietalis Say.
Eutænia sirtalis trilineata Cope.
Eutænia sirtalis pickeringii B. & G.
Eutænia sirtalis tetratænia Cope.
Eutænia sirtalis concinna Hallow.
Eutænia biscutata Cope.
Eutænia leptocephala B. & G.

There are therefore peculiar to the Pacific district eighteen species and three subspecies of Batrachia (two species found in the Holarctic region represented by subspecies, and one species

from the Canadian); two species and two subspecies of lizards; and three species and eleven subspecies of snakes.

We have of species and subspecies of the Western subregion the following synopsis:

Peculiar to the Diegan district,	19
Common to the Diegan and Chihuahuan,	6
Common to the Diegan and Pacific,	11
Peculiar to the Pacific,	39
	75

VIII. The Toltecan Subregion.

This subregion includes three districts which possess characteristic species, and which differ in climate. The Austroriental is a humid region with abundant rains and fogs, and includes the eastern face and slope of the central plateau, with the mountain elevations, including parts of the States of Puebla, Vera Cruz, Hidalgo and San Louis Potosi. It is cut off to the north from the Austroriparian subregion by an interval in the States of Nuevo Leon and Tamaulipas. The middle or Austrocentral district includes the valleys of Mexico and Toluca, and the region northward to the edge of the Sonoran subregion, including the State of Guanajuato, and perhâps further north. The climate of this district is much less humid than that of the Austroriental district. The Austroccidental district includes the high lands of Oaxaca, Guerrero, Michoacan and Jalisco. It is the most arid of the three divisions, and extends furthest to the south and west.

The northern boundary of the Toltecan district is not yet determinable; hence it is not possible to state whether species from the States of Durango and Zacatecas, such as *Eutænia angustirostris*, should be referred to it or not. A small collection made by Wilkinson in southern Chihauhua at Batopilas[1] has the character of the Chihuahuan fauna, with the following species not otherwise found in it:

Anolis nebulosus Wiegm.	*Scolecophis æmulus* Cope.
Uta bicarinata Dum.	

[1] Cope, Proceeds. Amer. Philosoph. Soc., 1879, p. 261.

The humid and dry districts of the Toltecan subregion repeat *in petto* the differences between the Austroriparian and Sonoran subregions. The Austroriental district is distinguished by the larger number of batrachian genera and species, and of certain genera of Crotalidæ. It also includes some genera which may be regarded as immigrants from the Central American region of the Neotropical Realm.

The characteristic species of the *Austrocentral district* are[2]:

BATRACHIA URODELA:

Siredon mexicanum Shaw.
Amblystoma tigrinum Green.

BATRACHIA SALIENTIA:

Bufo compactilis Wiegm.
Bufo intermedius Gthr.
Spea multiplicata Cope.
Spea hammondii Bd.
Hyla eximia Bd.
Hyla arenicolor Cope.
Rana montezumae Bd.

TESTUDINATA:

Kinosternum pennsilvanicum.
Onychotria mexicana Gray.

SAURIA:

Phrynosoma orbiculare Wiegm.
Sceloporus scalaris Wiegm.
Sceloporus microlepidotus Wiegm.
Sceloporus torquatus Green.
Sceloporus minor Cope.
Sceloporus melanogaster Cope.
Barissia imbricata Wiegm.
Cnemidophorus guttatus B. & G.
Eumeces brevirostris Gthr.

SERPENTES:

Conopsis nasus Gthr.
Toluca lineata Kenn.
Chionactis varians Jan.
Salvadora bairdii Jan.
Epiglottophis pleurostictus D. & B.
Hemigenius variabilis Dugés.
Natrix storerioides Cope.
Eutænia macrostemma Kenn.
Eutænia eques Reuss.
Eutænia pulchrilatus Cope.
Eutænia scaliger Jan.
Eutænia melanogaster Wiegm.
Tantilla bocourtii Gthr.
Tantilla calamarina Cope.
Crotalus basiliscus Cope.
Crotalus polystictus Cope.

[2] For the exact habitat of several of these I am indebted to the important papers of Dr. A. Dugés, in La Naturaleza, 1888, p. 97, and 1896 p. 3.

Of these species the following occur in the Chihuahuan district:

Amblystoma tigrinum Green.
Spea hammondii Bd.
Hyla arenicolor Cope.
Sceloporus scalaris Wiegm.
Sceloporus microlepidotus Wiegm.
Cnemidophorus guttatus B. & G.
Epiglottophis pleurostictus D. & B.
Eutænia macrostemma Kenn.
Eutænia eques Reuss.

The Austroriental district includes the mountainous region which bounds the Mexican Plateau on the east, from some part of the State of Puebla to a point to the north not yet ascertained. It is probably separated by, a considerable interval from the Austroriparian in the States of Tamaulipas and Nuevo Leon. Its climate is moist, and vegetation is abundant, and of principally Medicolumbian type. Various peculiar species of Acer, Platanus, Quercus, Andromeda and other forms are abundant. The Batrachian and Reptilian species are the following:[3]

BATRACHIA URODELA:

Spelerpes chiropterus Cope.
Spelerpes leprosus Cope.
Spelerpes cephalicus Cope.
Spelerpes orizabensis Blatchley.
Spelerpes gibbicaudus Blatchley.
Oedipina lineola Cope.
Thorius pennatulus Cope.

BATRACHIA SALIENTIA:

Hyla gracilipes Cope.
Hyla miotympanum Cope.
Hyla bistincta Cope.
Smilisca baudinii D. &. B.

SAURIA:

Sceloporus variabilis Wiegm.
Sceloporus æneus Wiegm.
Sceloporus microlepidotus Wiegm.
Phrynosoma orbiculare Wiegm.
Phrynosoma taurus Dugès.
Barissia imbricata Wiegm.
Barissia antauges Cope.
Gerrhonotus gramineus Cope.
Gerrhonotus tæniatus Wiegm.
Gerrhonotus liocephalus Wiegm.

[3] For a knowledge of the distribution of many of these species I am indebted to Francois Sumichrast, in Archives des Sciences, in Bibliotheque Universelle, 1873, p. 233, and *in litteris*.

Celestus enneagrammus Cope.
Liolepisma laterale Say.
Anelytropsis papillosus Cope.

SERPENTES:

Atractus latifrontalis Garm.
Ficimia olivacea Gray.
Epiglottophis lineaticollis Cope.
Osceola doliata polyzona Cope.
Ninia diademata B. & G.
Storeria dekayi Stor.
Storeria occipitomaculata Holbr.
Rhadinæa vittata Jan.
Rhadinæa decorata Gthr.
Eutænia sumichrastii Cope.
Eutænia chrysocephala Cope.
Eutænia pulchrilatus Cope.
Eutænia scalaris Cope.
Eutænia phenax Cope.
Sibon frenatum Cope.
Sibon personatum Cope.
Sibon albofuscum Lac.
Bothriechis mexicanus D. & B.
Ophryacus undulatus Jan.
Systrurus ravus Cope.
Crotalus triseriatus Wagl.

Of all the above species the following are found also in the Austrocentral district:

Barissia imbricata Wiegm.
Sceloporus variabilis Wiegm.
Sceloporus microlepidotus Wiegm.
Phrynosoma orbiculare Wiegm.
Eutænia pulchrilatus Cope.

Species found in the Austroriparian subregion:

Liolepisma laterale Say.
Storeria dekayi Stor.
Storeria occiptomaculata Holbr.

To the Austroriental list might be added *Spelerpes bellii* Gray, which is stated by Sumichrast to inhabit also the Tierra Caliente; and *Anolis nannodes* Cope, which the same authority says ranges from the Tierra Caliente into the Alpine district. The water-snake *Natrix rhombifera* Hallow. may occur in the Austroriental district, but this needs confirmation.

The *Austroccidental district* is inhabited by a number of peculiar species, together with some which occur in the other two districts of the Toltecan subregion. One peculiarity of this district is the poverty in Batrachia and the absence of Urodela. The peculiar species are the following:

BATRACHIA ANURA:

Leptodactylus melanonotus Hallow.
Hypopachus variolosus Cope.

SAURIA:

Sceloporus siniferus Cope.
Sceloporus horridus Wiegm.
Sceloporus rubriventris Gthr.
Sceloporus pyrrhocephalus Cope.
Sceloporus omiltemanus Gthr.
Sceloporus dugesii Boc.
Sceloporus bullerii Boul.
Sceloporus heterolepis Boul.
Cnemidophorus deppei lineatissimus Cope.
Eumeces callicephalus Boc.

SERPENTES:

Pseudoficimia frontalis Cope.
Sympholis lippiens Cope.
Atractus omiltemanus Gthr.
Adelophis copei Dugès.
Rhadinæa laureata Gthr.
Eutænia godmanii Gthr.
Chionactis michoacanensis Dugès.
Coniophanes lateritius Cope.
Conophis vittatus Pet.
Himantodes gemmistratus latistratus Cope.
Sibon personatum Cope.
Manolepis nasutus Cope.

Of the above species there are found in the Tierra Caliente:

Sceloporus siniferus Cope.
Sceloporus horridus Wiegm.
Sceloporus pyrrhocephalus Cope.
Conophis vittatus Pet.
Sibon personatum Cope.
Manolepis nasutus Cope.

And in the region south to Costa Rica:

Hypopachus variolosus Cope.
Himantodes gemmistratus Cope.

The Austroccidental district shares with the Austrocentral the following:

BATRACHIA ANURA:

Bufo compactilis Wiegm.
Hyla eximia Bd.
Rana pipiens austricola Cope.

SAURIA:

Phyllodactylus tuberculosus Wiegm.
Uta bicarinata Dum.
Barissia imbricata Wiegm.

Cnemidophorus guttatus B. & G.
Sceloporus scalaris Wiegm.
Phrynosoma orbiculare Wiegm.
Anolis nebulosus Wiegm.

SERPENTES:

Drymobius margaritiferus Schl.
Diadophis lætus Cope.
Osceola doliata polyzona Cope.
Hemigenius variabilis Dugés.
Natrix storerioides Cope.
Eutænia eques Reuss.
Eutænia melanogaster Wiegm.
Epiglottophis pleurostictus D. & B.
Tantilla calamarina Cope.
Trimorphodon biscutatus D. & B.
Trimorphodon upsilon Cope.
Crotalus triseriatus Wagl.
Crotalus polystictus Cope.
Crotalus basiliscus Cope.

A number of species inhabit the Austroccidental and Austroriental districts, passing to the southward of the Austrocentral, at least so far as present information extends. These are the following:

BATRACHIA ANURA:

Smilisca baudinii D. & B.

SAURIA:

Sceloporus torquatus Green.
Phrynosoma taurus Dugés.
Gerrhonotus oaxacæ Gthr.

SERPENTES:

Rhadinæa vittata Jan.
Eutænia chrysocephala Cope.
Coniophanes proterops Cope.
Ophryacus undulatus Jan.
Crotalus triseriatus Wagl.

The species of the Toltecan subregion are as follows:

Austroriental district,	44
Austrocentral district,	36
Austroccidental district,	24
	73
Doubles emplois,	2
	71

VIII. Recapitulation.

The number of species of Reptilia Squamata of the Medicolumbian region is as follows. The species of Batrachia have been already enumerated in my book on that class.[4]

Superfamilies.	Families.	Genera.	Species.
	SAURIA.		
Pachyglossa	Iguanidæ	12	79
Nyctisaura	Gecconidæ	2	2
	Eublepharidæ	1	1
Helodermatoidea	Helodermidæ	1	1
Diploglossa	Anguidæ	4	17
Leptoglossa	Tiidæ	2	11
	Xantusiidæ	3	5
	Scincidæ	2	20
	Anelytrorsidæ	1	1
Annielloidea	Anniellidæ	1	2
Annulati	Euchirotidæ	1	1
	Amphisbænidæ	1	1
	Total Sauria	31	141
	SERPENTES.		
Catodonta	Glauconiidæ	1	3
Colubroidea	Boidæ	1	3
	Charinidæ	1	2
	Colubridæ	26	133
	Dipsadidæ	10	19
	Elapidæ	1	3
Solenoglypha	Crotalidæ	5	25
	Total Serpentes	45	188
	Sauria	31	141
Total Squamata		76	329

[4] The Batrachia of North America, Bulletin of the U. S. Natl. Museum, No. 34, 1889, p. 451. The species of the Toltecan subregion are mostly omitted from this book.

EDITOR'S TABLE.

It is difficult to eradicate from scientific literature a name or word which has become current, even after it has been found to be an expression of ignorance or error. Thus some names introduced into Zoology die hard. It is perfectly well-known that the grouping of forms named by Cuvier Pachydermata, is entirely unnatural, and the appropriate positon of all of its contents has been exactly determined; yet the word occasionally crops up still in the literature. The supposed primary divisions of fishes Ganoidei and Teleostei, have a still more vigorous vitality, although it is perfectly clear that there is no use for either term. The supposed Ganoid division is thoroughly heterogeneous, its contents forming with the Teleostei a more comprehensive division, the Teleostomi of Owen, which naturally falls into several primary divisions three of which were included in the Ganoidei by Agassiz and Müller. Perhaps the most pestilent pretender of the list, is the word Amphibia, which is so frequently used instead of the proper name of the class Batrachia. The name Amphibia was originally applied to a combination of the Reptilia and Batrachia, before the fundamental differences between the two were known. When the Batrachia were first separated from the Reptilia, the new name was naturally applied to the new division, and the name Amphibia would have been more applicable to the larger division of its former self i. e. the Reptilia. As, however, its definition accorded with neither the Reptilia nor Batrachia, it was not used for either, nor was it introduced to take the place of Batrachia with a definition, until a few years ago by Huxley. This was done in defiance of the universal usage of naturalists at the time, and probably in ignorance of the real state of the case, since it frequently happens that men engaged in the real work of biological science, find questions of names irksome and stupid. Nevertheless it is a distinct advantage always to have but one name for one thing; and that name should be the oldest which was applied to the thing in question as determined by the definition given. Applying this principle, the name Batrachia has a quarter century priority over Amphibia.

In the April, 1896 number of this journal (p. 292) we published what purported to be a review of a work by Wachsmuth and Springer, which was signed by one of our frequent contributors. In a foot note the work is stated to have been published in 1895. We have learned

from leading authorities on the subject of the work, (the Crinoidea), that it was not published at the time the review was issued, nor it is yet published. We make this statement, since it is important that the date of publication of all books, especially scientific books, should be correctly ascertained and reported, and because we desire to prevent any confusion as to the date of this particular publication which might arise from our having published the review in question. As is usual with periodicals, we assume no responsibility for articles published in the NATURALIST unless they are anonymous.

The dates of publication of the numbers of the AMERICAN NATURALIST during the years 1895, and 1896 are as follows: for 1895; Jan., Jan. 15th; Feb., Feb. 14th; March, Mch. 6th; April, Apl. 9th; May, May, 13th; June, June 3d; July, July 9th; August, July 31st; Sept., Aug. 28th; Oct., Sept. 26th; Nov., Oct. 29th; Dec., Dec. 6th.

For 1896; Jan., Dec. 31st, 1895; Feb., Jan. 30th; March, Mch. 9th; April, Apl. 2d; May, May 2d; June, June 3d; July, July 2d; August, Aug. 6th; Sept., Sept. 9th; October, Oct. 3d; Nov., Nov. 2d; Dec., Dec. 5.

RECENT LITERATURE.

Gregory's Plant Anatomy.[1]—Among the host of botanical textbooks that are constantly appearing, it is a pleasure to welcome one that is a contribution to certain departments of botanical literature, rather than a mere exposition of the laboratory and lecture methods, good, bad, and chiefly indifferent, of the author. While it is to be assumed that American investigations in histology and in cytology have not been lacking during these past few years, the fact remains that they have not as yet resulted in an increase of literature upon these subjects.

While there can be no doubt that the tide is setting steadily and strongly in the direction of higher things in cisatlantic botany, this is as yet a premonition rather than a fact, and the few texts leading toward this are to be regarded as pioneers and valued as such. These books are divisible into two classes, and in evaluating them, it is necessary to measure them by a proper standard. Thus, a book which purports to be a textbook should not be criticized because it does not manifest

[1] Elements of Plant Anatomy, by Emily L. Gregory, Ph. D. Professor of Botany in Barnard College. Ginn & Co., Boston, 1895, pp. VIII, 148. 8vo.

the depth and comprehensiveness of an exhaustive treatise, nor should an elaborate work on original investigation be supposed to cover the details of elementary science.

The present book is intended to serve as an introduction to the elements of phytotomy. This purpose is effected more than ordinarily well. It is no mean task to distinguish between the relevant and the irrelevant, between the essential and the non-essential in the construction of an elementary text. In these very points, the author has been particularly happy, and deserves congratulation upon the coherency and the coordination manifested in the text.

A striking feature of the book is its prevailing clearness. Many otherwise well written and helpful text-books are marred by the fact that too much is written between the lines, a thing deplorable in any scientific writing, but especially so in an elementary one. The author has succeeded, however, not only in establishing delightful perspicacity of style, but also in maintaining it throughout the work. In consequence, the beginner may find here a text which presents in a remarkably easily assimilated condition those rudiments of plant anatomy which should serve as a foundation for advanced botanical study in all lines.

The merits of the book are many and obvious, and warrant passing its few defects in silence. Its inspiration is readily recognizable as of the German school, an additional point in its favor were it not for a prefatory remark to which the reviewer must enter serious objection. The author states that "it is quite certain that the measure of our progress in any science may be found in our ability to adapt the thought and experience of other nations to our special needs and resources," a statement of such a very peculiar nature that comment is superfluous.

The book is divided into two parts, the first of which treats of cytology, or, as the author terms it, the anatomy of the cell. Under this, the first chapter treats of the cell as a unit, the second and third present the subjects of cell-wall and cell-contents in their modern aspects. The second part discusses the anatomy of tissues, first generally, and then more specially, with reference to the thoroughly antiquated divisions, Thallophytes and Cormophytes. The last chapter, the irrelevancy of which is excused by its importance, is devoted to an exposition of the secondary growth of stems and roots.—FREDERIC E. CLEMENTS.

Boulenger's Catalogue of Snakes in the British Museum.[1] —In this work we have a manual of Ophiology in which the subject is

[1] Catalogue of the Snakes in the British Museum. Vol. I, 1893; vol. II, 1894; vol. III, 1896. By G. A. Boulenger, F. R. S.

as nearly as possible brought up to date. The especial advantage of being the work of the Keeper of the largest collection of Ophidians in the world, makes this catalogue of especial value to all students. The author informs us that there are known 1639 species of snakes, of which 1327 are represented in the collection of the British Museum by 11092 specimens.

A good deal of valuable new osteological work enters into the systematic, which will be at once recognized by specialists. Thus the determination of the forms which have elongate hypapophyses throughout the vertebral column is here made for the first time, and the discovery that all the Colubridæ of Madagascar have the prolonged series of hypapophyses, is one of the notable announcements of the work. The peculiar pterygoids of the Amblycephalidæ are the author's discovery, as are also the split ectopterygoids of Dispholidus, etc., and the confluent optic foramina of the Psammophiinæ[2] The labor of specific determination of over 11000 specimens, in an order where variation is often conspicuous, is, however, the great feature of such a work as this, and even the approximately complete form in which it is now presented, is a monument to the industry and acumen of its author, and a service rendered to science by the British Museum which will always remain.

There are, however, some spots on the face of this illuminating ing production. The labor of determining the true limits of variable species has in a good many instances, it seems to us, proven too much for the patience of the author, and he has resorted to the convenient method of "lumping" too often. He has given up a valuable feature of the older catalogues, the list of doubtful species. In the present work all published species are either good or bad, whether the author has had the requisite opportunity of determining their true status or not. Thus it has happened in not a few instances that names relegated to the synonymy in the body of the work are reintroduced in the Addenda as belonging to good species. Had the author the material it is probable that a good many others would have been recognized before the final issue of the Catalogue. The author has been especially unfortunate in his treatment of North American species, and the student of North American Ophiology will not find his knowledge of this subject increased by this publication. Some of the species studies are on the other hand very thorough, as for instance the genera Vipera and Naja. The revision of the synonymy of both the older and later European authors is a service for which all herpetologists will be grateful.

[2] I mention here that the genera Malpolum, Psammophis, Mimophis and Rhamphiophis have no protrusible male intromittent organ. For this reason I propose to arrange them as a special subfamily, the Psammophiinæ.

The primary divisions of the Ophidia (or Serpentes as they should be called) adopted, are nine families, which have very different values. These can be associated in superfamilies of approximately equal value, but this Dr. Boulenger has not done, but has contented himself with giving an analytical table (pp. 1–2), where some of the characters of these superfamilies are pointed out, in the dichotomous order, which does not express relative value. Many groups usually regarded as families are not recognized, as for instance the Najidæ and Dipsadidæ, which are included in the Colubridæ. In a phylogenetic table the interesting suggestion is made that the Solenoglyphous snakes are derived from the Opisthoglyphous, and not from the Proteroglyphous.

In seeking for generic characters the dentition has been closely examined. The value of dental characters has been thoroughly tested, and the result is valuable to the student, although we do not always agree with the use made of the information in the Catalogue. The author does not adopt the characters used by Duméril and Bibron in many instances, for good reasons, but he introduces others of his own which are no better, as the numbers and in some cases the relative lengths of the teeth. In practice it is often impossible to determine whether teeth are of equal length or a little longer at one or the other end of the jaw; nor is the number of the teeth in the jaws precisely definitive of anything but species, as can be readily seen from the results recorded in the present work. The division or union of the anal plate and urosteges, is generally rejected as a character, although its value is testified to by the uniform use made of it by ophiologists. In fact the generic definitions are based on no uniform principle, and the author seems to have been possessed at times with the idea that it were an especial merit to differ as much as possible from his predecessors.

One result of the study of this work will be to prove to ophiologists that it is desirable to become acquainted with new characters of definitive value before we can have the true system of the snakes. An important addition to our knowledge in this direction, i. e. of the characters of the hemipenis and of the lungs, came too late to be incorporated in the present work.—E. D. Cope.

Nuttall's Handbook of Birds.[1]—A new edition, with important additions, and a series of more than one hundred colored illustrations.

[1] *A Popular Handbook of the Ornithology of Eastern North America.*—By Thomas Nuttall. Revised and annotated by Montague Chamberlain. Vol. I, *Land Birds.* Vol. II *Game and Water Birds* Second edition, with corrections and additions Illustrated with one hundred and seventy-two figures, two colored

This favorite work, easily understood, handy, and popular, including all of Nuttall's delightful descriptions of bird-life, which was some time since fully annotated by Montague Chamberlain, who added the birds not known in Nuttall's time, will be found more useful and valuable than ever before, Mr. Chamberlain having again gone over the work with the greatest care, bringing the information down to date.

Colored representations of the birds being desirable for amateurs and students, a series of twenty plates, containing one hundred and ten figures of birds, has been added to the present edition. The drawings have been mostly copied from those of Wilson, and may be relied on for accuracy, although in some instances the tints do not come up to the brilliancy of Nature. We recommend the book as the one for the family, where the strictly scientific side of ornithology is not the chief desideratum. We mean by this that the work is not devoted to the anatomy and physiology of birds, but is one by which the species may be identified, and where descriptions of their habits and geographical range may be found; all set forth in admirable style.

Education of the Central Nervous System.[1]—This book is an endeavor to apply the most recent results of psychology and brain physiology to the theory of education. The author quotes from Donaldson and other well-known writers on the topography of the brain and localization of functions. In view of the close connection between cerebral development and mental capacity, he advocates an education which shall develop all parts of the brain to the greatest possible extent. He recommends especially that children be trained to distinguish every shade of sensation-difference, and to recall in vivid images the objects of every kind which they have experienced; if such training be begun early in life, the brain cells are better developed, and in after life our mental images are more numerous and more definite.

Unfortunately the book is limited almost exclusively to a discussion of sensation and memory, leaving out of account entirely the higher rational processes. It becomes an appeal for an education which is fundamentally æsthetic and literary, as distinguished from scientific. Book learning for children is decried, and teachers are urged to take their pupils out into the woods and fields, and have them learn from

frontispieces, and twenty colored plates, containing one hundred and ten figures of the most important land and water birds. 2 vols. Crown 8vo. Cloth, extra, gilt top, $7.50 *net;* half crushed Levant morrocco, extra, gilt top, $13.50 *net.*—LITTLE, BROWN & Co., Publishers, 254 Washington Sreet, Boston.

[1] The Education of the Central Nervous System, by R. P. Halleck. New York, The Macmillan Co., 66 Fifth Ave. 1896. Pp. xii, 258; price $1.

nature herself. This was the education, the author thinks, which made Shakespere really great. The study of nature is certainly of value, and the author's recommendations, together with the practical exercises in sense-training which he gives, will doubtless be an aid to this culture. But in these days of the supremacy of science, it is far more important to begin early to lay the foundations of habits of correct scientific thinking. The possession of clear and vivid mental imagery is a factor in correct thinking, of course; but unless accompanied by the logical treatment of ideas it is quite as likely to lead us in the wrong as in the right direction.

As a manual on the education of the central nervous system Mr. Halleck's work is very incomplete; it must be supplemented in several directions, and notably by a considerable amount of that very "book-learning" which the author treats so lightly. The treatment of motor education is inadequate, being confined to a single short chapter at the end of the book. By way of minor criticism, we may notice the author's fondness for repeating the same illustrations (e. g., pp. 82, 248). Some of his deductions are based on very inadequate data (e. g., p. 64); but this is rather the fault of his authorities. His list of great men who began to show talent at an early age, though large, calls to mind so many exceptions as to throw considerable doubt on the position which it seeks to establish.

The chapter entitled: "How Shakspere's Senses were Trained," is interesting to the student of literature, though somewhat too detailed. Throughout the book there is a wealth of quotations from Shakespere, Milton, and other writers, which add to its literary finish, if they do not improve its scientific quality.—H. C. W.

Lydekker on the Geographical History of Mammalia.[1]—I have already referred to this work in the last number of the NATURALIST in a paper on the Geographical Distribution of Batrachia and Reptilia of North America. I then pointed out that the author adopts the three Geographical realms of Huxley with the reasons why in my opinion the Ethiopian should constitute a fourth Realm. The divisions of the Notogæic Realm of Lydekker's system, are the Australian, Polynesian, Hawaiian and Austromalayan. The Neogæic realm has a sole region, the Neotropical. The Arctogæic is divided into the Malagasy, the Ethiopian, the Oriental, the Holarctic, and the Sonoran. Having otherwise disposed of the Ethiopian and its subdivision the Malagasy,

[1] The Geographical History of Mammals; by R. Lydekker A. B., F. R. S., V. P. G. S., etc. Cambridge University Press, 1896. 8vo. pp. 400.

I adopted the three remaining regions, the Oriental, the Holarctic, and the Medicolumbian ; the last name being derived from Blanford, and used as a substitute for Sonoran, which have been previously used for a subdivision.

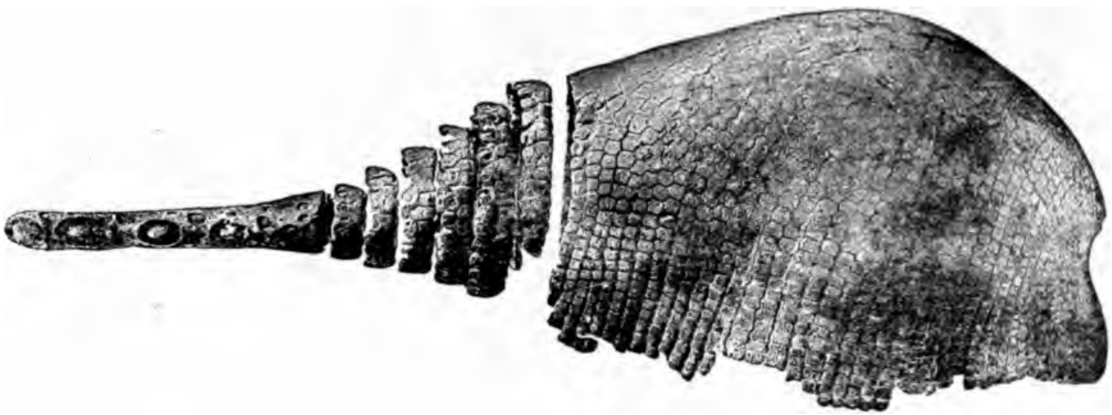
This work is a magazine of information on the subject of which it treats, and a unique feature is the large amount of reference to the facts of paleontology. This increases the value of the book to the general reader, but cannot be said to be germane to its main object. The introduction of the extinct forms of life necessarily changes the aspect of the faunal lists of a country to a marked degree, nowhere more so than in the Arctogæan Realm. Each geological period had in fact its own geographical distribution of forms, and when all are discovered a series of books on geographical distribution in each period might be written, each different from every other one.

Plagiaulax minor from the English Wealden; much enlarged.

Manis tricuspis West Africa.

The well-known familiarity of the author of this book with both Mammalian zoölogy and paleontology, gives it a value which no similar book possesses; and its compact form and fulness of illustration

PLATE XXVIII.



External skeleton of *Panochthus tuberculatus* from Argentina; much reduced.

make it especially convenient for the traveller who reads as he goes. The author writes clear and direct English, and correct classical orthography. His systematic of the Mammalia given on p. 11 is uncritical, though it includes most of the groups brought to light by paleontology. More detailed classification in later chapters elucidates the subject further.

The accompanying three illustrations give a good idea of their their general character.—E. D. Cope.

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General Notes.

PETROGRAPHY.[1]

The Sioux Quartzite of Iowa.—The Sioux quartzite has long been known as the oldest sedimentary rock in Iowa. It has recently been studied by Beyer.[2] It is a white or red vitreous rock with which is associated as its upper extension a séries of mottled reddish or purplish-black slates. The quartzites present the usual aspects of indurated sandstones. The constituent quartz grains are rich in 'quartz-needles' which can be traced directly into rutile spicules. The slates are arenaceous. They exhibit no traces of slaty cleavage, though in some cases their quartz grains and micaceous constituents are distorted in such a way as to testify to a horizontal movement in the rock mass containing them. All the slates are mottled by spheroidal masses of a lighter color than the body of the rock. These masses are spheroidal with the longer dimensions of the spheroids in the bedding planes of the shale. Their lighter color is supposed to be due to the removal of iron from those portions of the rock they occupy. Associated with the quartzites is a great mass of olivine diabase consisting of a coarse grained aggregate of labradorite and oligoclase zonally intergrown, olivine, augite, biotite, hornblende, apatite and magnetite. Most specimens are much altered, the components having been changed into the usual secondary substances common to diabase. In structure the rock varies from the ophitic, in which the plagioclase is older than the augite, to the gabbroitic, in which the augite is the older mineral. An analysis gave:

SiO_2	TiO_2	Fe_2O_3 FeO	Al_2O_3	CaO	MgO	K_2O	Na_2O	H_2O	P_2O_5	Total
42·85	tr	13.66	20.23	6.85	3.42	1.90	5.78	.88	tr	=100.57

The Peridotites of North Carolina.—In connection with a discussion of the occurrence and origin of corundum in North Carolina, Lewis[3] gives us an interesting account of the basic rocks associated with the gneisses in that portion of the Appalachian belt included within the limits of the State. These basic rocks, consisting mainly of

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.
[2] Iowa Geol. Survey, Vol. VI, p. 69.
[3] Bull. No. 11, North Carolina Geol. Survey, 1896.

peridotites, occur in small lenticular masses or in narrow strips, which are always enveloped in a sheet of schistose talc or chlorite, and thus are never in direct contact with the gneisses through which they are believed to cut. They are classed as peridotites, pyroxenites and amphibolites, the former being the most common. The peridotites present several types in each occurrence, all merging into one another and forming a single geological unit. The principal types of the peridotites are dunite, harzburgite, amphibole-picrite and forellenstein. All are massive, as a rule, though exceptions are noted. The dunite is composed of olivine grains, octahedrons and rounded grains of picotite and chromite, plates of enstatite, prisms of light green hornblende and various alteration products of these, the most common being serpentine tremolite and chlorite. The Harzburgite and the other peridotites present no unusual features. They appear to be transition phases between the dunite and the various pyroxenites among which are recognized two types, an enstatite rock and websterite. The enstatite rock is made up almost exclusively of enstatite or bronzite and its alteration product talc. An analysis of the enstatite gave:

SiO_2	Al_2O_3	FeO	CaO	MgO	MnO	H_2O	Total
51.64	.12	9.28	.45	31.93	.56	5.45	99.43

The amphibolites are composed chiefly of amphibole. The most important type is composed of grass-green hornblende, anorthite and more or less corundum. The rock is fine grained and it is usually gneissic, although occasionally massive. Transitions through forellenstein into dunite were observed, although the distribution of the rock suggests its occurrence in a system of dykes cutting the latter rock. The hornblende has the following composition:

SiO_2	Al_2O_3	Cr_2O_3	FeO	NiO	MgO	CaO	NaO	K_2O	H_2O	Total
45.14	17.59	.79	3.45	.21	16.69	12.51	2.25	.36	1.34 =	100.33

Genth called the mineral smaragdite. Dana regards it as edenite. In addition to the rocks mentioned above, there are also present in the region massive serpenite, which was unquestionably derived from dunite, talc-schists, and soapstones derived from enstatite rocks and chlorite schists.

In a second paper[4] the same author gives his reasons for considering these rocks as eruptive in origin.

Shales and Slates from Wales.—Hutchins[5] continues his studies of clays, shales and slates by an investigation of the nature of

[4] Elisha Mitchell Sci. Soc. Jour., Pt. II, 1895, p. 24.

[5] Geol. Mag., Vol. III, 1896, p.

shales taken from some of the deepest coal mines in Wales. The chemical composition of the particular shale analyzed does not differ materially from that of some of the carboniferous shales from other coal fields. Physically the deeper shales are not much more compact than hard clays. The author reviews the results of his observations on shales and slates. He states that what takes place in a rock during its progress from clay to shale, is the development and crystallization of muscovitic mica and the production of chlorite. He also calls attention to the fact that dynamic metamorphism is made to explain many phenomena connected with the crystallization of slates, that are capable of being explained better by static metamorphism. The spots of many contact rocks are now thought to be secretions from a mineralizing solution, depositing in these spherical forms material collected from the rock body. By crystallization the spots pass over into cordierite, biotite or staurolite crystals.

Notes.—Cushing[6] declares that in addition to the rocks described by Kemp from the eastern Adirondacks there is a system of diabase dykes, which are older than the monchiquites and camptonites of the district.

By melting certain rock powders in the presence of reagents Schmutz[7] has obtained aggregates of minerals which in most cases are very different from those composing the original rocks. Eklogite fused in the presence of calcium and sodium fluride yielded a mass of meionite, plagioclase and glass; leucitite with calcium chloride gave a mass composed of a glassy groundmass and plagioclase; with the addition of sodium fluride and potassium silico-fluride it yielded scapolite, mica, magnetite; with sodium chloride it produced augite, scapolite and magnetite and a glass matrix. Granite fused with magnesium and calcium chlorides and sodium fluoride gave andesine and olivine in a groundmass containing augite. Other rocks treated with other reagents gave analagous results.

As the result of a series of experiments made with the view of discovering a medium with a very high specific gravity that will not attack sulphides, Retgers[8] finds that the acetate and the mixed nitrate and acetate of thallium are both neutral toward sulphides. The former is available for separating minerals with a density below 3.9, and the latter those with a density below 4.5.

[6] Trans. N. Y. Acad. Sci., XV, 1896, p. 249.
[7] Neues Jahrb. f. Min., etc., 1896, I, p. 211.
[8] Neues Jahrb. f. Min., etc., 1896, I, p. 213.

In an article in the *Neues Jahrbuch* Bauer[9] gives a German transcription of his article[10] on the rocks associated with the jadeite of Turmaw, Burmah.

Schroeder vander Kolk[11] describes briefly a series of rocks collected by Martin in the Moluccas. In the southern part of Amboina the rocks are mainly granite and peridotite, while in the larger northern part they consist of modern volcanics, as they do also on the other islands studied. These rocks are principally dacites and liparites, but on one island andesites occur. Both the dacites and the granite contain cordierite. The dacites are pyroxene and biotite varieties. The andesites are pyroxenic; mica schists, breccias and limestones occur also on the islands. The residue left after treatment of the limestone with acid contains quartz, sanidine, plagioclase, biotite, amphibole, orthorhombic pyroxene, hematite, garnet, cordierite, sillimanite and pleonost.

BOTANY.[1]

The Evolution of a Botanical Journal.[2]—In November of the present year the *Botanical Gazette* reaches its majority, by attaining the age of twenty-one years. It first appeared in November, 1875 under the name of the *Botanical Bulletin*, and consisted of four pages of short notes. It was edited by John M. Coulter, then professor of Natural Sciences in Hanover College (Hanover, Indiana). In his introductory note the editor stated that the object of the new journal was "to afford a convenient and rapid means of communication among botanists. The context shows that it was started as a distinctly western journal, intended to supplement the work of eastern botanical publications.

The first volume included notes by the editor, and Thomas C. Porter, Samuel Lockwood, G. C. Broadhead, M. S. Coulter, Mary E. Pulsifer Ames, J. T. Rothrock, H. C. Beardslee, Coe F. Austin, George Vasey, Alphonso Wood, Isaac Martindale, Elihu Hall, E. A. Rau, and others who have long since diappeared from the botanical field. With the

[9] Neues Jahrb. f. Min., etc., 1866, I, p. 19.

[10] Cf. AMERICAN NATURALIST, June, 1896, p. 478.

[11] Ib., 1896, I, p. 152.

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

[2] Read before the Botanical Seminar of the University of Nebraska. October 10, 1896.

opening of the second volume its name was changed to the *Botanical Gazette*, and the name of M. S. Coulter was added as joint editor. In 1883 by a reorganization of the management, John M. Coulter, Charles R. Barnes and J. C. Arthur became editors, an arrangement which proved to be so satisfactory to the botanists of the country as to become permanent.

The next few years were trying ones for the ambitious editors, but the impetus given to botanical thought by the incoming of modern methods in teaching and study, and perhaps, also, by the organization of the Botanical Club of the American Association for the Advancement of Science, proved helpful in many ways. The Philadelphia (1884) and Ann Arbor (1885) meetings of the Botanical Club created much botanical enthusiasm, the results of which accrued to the benefit of the *Gazette*.

The beginning of its second decade saw it much enlarged, improved in typography and apparently well established in the confidence of American botanists. Year by year it was still further increased in size, better paper was used, and the quality of the matter steadily improved. From the fifty-two pages of short, and mostly local, notes of volume I, we turn to the five hundred and sixty-eight pages of structural, physiological, ecological, systematic and palæontological matter in volume XX. With the opening of the twenty-first volume an additional enlargement was found to be necessary, the numbers averaging sixty-five pages each.

In the earlier volumes there were no plates, the first one occurring in volume VI, illustrating an article by J. C. Arthur on the trichomes of *Echinocystis lobata*. In the twentieth volume there were thirty-seven plates, while for the first half of 1896 the number was twenty-nine.

The last stage in the evolution of this important factor in American botany was reached a few months ago when its financial management was transferred to the University of Chicago. It thus happily becomes an endowed institution, and the editors, relieved from all anxiety as to its business management, are free to develop it along strictly scientific lines. To the three editors whose efforts have given it the foremost place place among botanical journals are hereafter to be added several "associate editors"; at present these are G. F. Atkinson, V. M. Spalding, Roland Thaxter and William Trelease. Under the new regime it promises to be more cosmopolitan than before, and accordingly we are assured that the names of one or more European botanists will soon be added to the corps of editors.

This factor in botanical science has thus been a growth, and it represents to-day much more than so many pages of printed matter. It has grown and developed as the science of botany has grown and developed in this country. When we look over the earlier volumes with surprise at the little notes which fill the pages, we must not forget that American botany had not then generally risen above such contributions. It is true that we had a few masters in the science, with Dr. Gray still in his prime, but these masters wrote little for general reading, and their technically systematic contributions were mostly published in the proceedings of learned societies. The one thing which stands out to-day in sharp contrast with the botany of two decades ago is the very great increase in the number of masters in the science who are making liberal contributions from many different departments. The many-paged *Gazette* of to-day, with its rich variety of matter, differs no more from the four-page *Bulletin* of 1876 than does the botany of the two periods.—CHARLES E. BESSEY.

The North American Species of Physalis and Related Genera.—In a recent number of the Memoirs of the Torrey Botanical Club (Vol. IV, No. 5) Mr. P. A. Rydberg publishes an important contribution to our knowledge of our species of Physalis and related genera. Every one who has attempted to accurately identify any of the native species of Physalis knows well how difficult and discouraging the task has been. Commenting on this Mr. Rydberg says: "The reason is not that the descriptions are so badly drawn, but that only about one half of the actual number of species have, as a rule, been recognized."

After a critical discussion covering fifteen pages the author characterizes the six genera which he includes in his monograph. These are *Margaranthus*, with four southwestern species; *Physalis*, with thirty-nine species; *Quincula*, with one Rocky Mountain species; *Leucophysalis*, with one species of the northern United States and Canada; *Chamæsaracha*, with four species of the southwestern United States; *Oryctes*, with one species from Nevada.

Throughout the paper the nomenclature and synonymy receive full attention, the citations being unusually complete. The descriptions are concise, and apparently drawn with great care. And last, but by no means least, there is a full index of species and synonyms given at the end of the monograph. Altogether it is an unusually good piece of work.—CHARLES E. BESSEY.

Professor Prentiss.—The recent death (August 14th) of Professor Albert Nelson Prentiss of Cornell University calls for more than a mere

brief mention. Born in Cazenovia, N. Y.; May 22, 1836; educated in the Oneida County Seminary, and the Michigan Agricultural College (B. Sc., 1861 and M. Sc., 1864). After short periods of service in the engineering corps of the United States Army, and the public schools of Michigan he became professor of botany in the Michigan Agricultural College (1863 to 1868). After six years of service he was called to the chair of botany in Cornell University (1868), where he remained for twenty-eight years when on account of failing health he was made professor *emeritus* (1896). In these years of work Professor Prentiss was emphatically a *teacher*. The building and equipment of his department, and the training of men who went out to be professors in many colleges, left little time for investigations and the preparation of papers. He chose to impress his thoughts upon men rather than upon paper, and he will be remembered not as a writer, but as a teacher. His life shows how much more effective our work is when we teach men directly by our spoken words rather than through our printed papers.—C. E. B.

The Nomenclature of Mycetozoa.—Professor Mac Bride has been studying the question of nomenclature among these organisms (plants he calls them, and, therefore his results are noticed here) and finds great difficulty in applying the "priority rule" to the solution of the problem. He calls attention to the well-known fact that the earlier botanists did not understand the nature of Mycetozoa and that their descriptions and even their figures in many cases are unintelligible. Rostafinski a little more than twenty years ago gave us the first rational account of the group, and for the first time gave us descriptions by means of which we may know certainly what he had in hand when he applied a particular name. His nomenclature is, therefore, to a large extent the earliest which is authentic. Practically all earlier descriptions are unrecognizable, and therefore, Rostafinski had to take up the work *de novo*. Professor Mac Bride says: "The fact is that when Rostafinski gives credit to his predecessors it is for the most part purely a work of courtesy and grace; there is nothing in the work itself to command such consideration." He therefore concludes that "the man who in his search for priority ascends beyond Rostafinski, does it at the risk of endless confusion and uncertainty in the great majority of cases" and that for these the initial date must be that of his great work, "Sluzowce Monografia" in 1875.—CHARLES E. BESSEY.

The Flora of Wyoming.—Professor Aven Nelson of the University of Wyoming recently issued a valuable "First Report on the Flora of Wyoming," based upon field work in 1892 (by Professor Buf-

fum), 1894 and 1895 by Professor Nelson. The catalogue of plants includes 1118 species of Spermatophytes, 14 Pteriodophytes, 26 Bryophytes, 3 Algæ, 8 Fungi and 7 Lichens, making a total of 1176. The trees of Wyoming are listed as follows: Rocky Mountain Yellow Pine (*Pinus ponderosa scopulorum*), Rocky Mountain White Pine (*P. flexilis*), Lodge-pole Pine (*P. murrayana*), Engelmann's Spruce (*Picea engelmanni*), Blue Spruce (*P. pungens*), Douglas Spruce (*Pseudotsuga douglasii*), Red Cedar (*Juniperus virginiana*), Black Cottonwood (*Populus angustifolia*), Rydberg's Cottonwood (*P. acuminata*), Quaking Aspen (*P. tremuloides*), Sand-bar Willow (*Salix longifolia*), Almond Willow (*S. amygdaloides*), two other species (*S. flavescens, S. lasiandra*), Green Ash (*Fraxinus viridis*), Box Elder (*Negundo aceroides*), Scrub Oak (*Quercus undulata*), Wild Plum (*Prunus americana*), Wild Cherry (*P. demissa*), Choke Cherry (*P. virginiana*), Hawthorn, two species (*Crataegus rivularis* and *C. douglasii*), Service Berry (*Amelanchier alnifalia*), Silver Berry (*Elæagnus argentea*), Buffalo Berry (*Shepherdia argentea*), Black Birch (*Betula occidentalis*), Black Alder (*Alnus incana viresceus*), Sage Brush (*Artemisia tridentata*).

The last species is sometimes so large that "a man on horseback may ride erect underneath the branches."

We notice a curious slip by which *Actinella glabra* Nutt. is listed among the new species, although it was published as a new species fifty-five years ago in the Transactions of the American Philosophical Society, and a year or so later appeared under Nuttall's name in Torrey and Gray s Flora of North America, II, p. 382.—Charles E. Bessey.

The Lichens of Chicago.—Bulletin No. 1, of the Geological and Natural History Survey of the Chicago Academy of Sciences is devoted to an enumeration of the lichens of Chicago and vicinity, by Mr. W. W. Catkins. One hundred and twenty-five species are enumerated and very briefly characterized. The paper is supplemented by a useful but incomplete Bibliography of North American Lichenology.—Charles E. Bessey.

Eastwood's Plants of Southeastern Utah.—In the Proceedings of the California Academy of Sciences (2d series, vol. VI) Miss Alice Eastwood enumerates 162 species collected in 1895 in the valley and on the plateaus of the San Juan River in southeastern Utah, a desert region with curious oases about springs and along cañons. Several new species are enumerated, three of which are figured in the plates which accompany the report.—Charles E. Bessey.

Correction.—On page 748, by a slip of the pen the "popple" of the Colorado Mountains is given as *Populus balsamifera candicans;* it should be *P. tremuloides.*—CHARLES E. BESSEY.

Botanical News.—A suggestive pamphlet on "The Pathology of Plants" by B. T. Galloway comes from the Office of Experiment Stations of the United States Department of Agriculture. Its object is to point out certain lines of work in plant pathology that might be undertaken by botanists in the state experiment stations.—From the Division of Agrostology, (U. S. Dept. Agriculture) we have "Fodder and Forage Plants, exclusive of the Grasses" a pamphlet of fifty-eight pages, by Jared G. Smith. It is a descriptive, illustrated list of these plants, written in semi-popular language. It will be of value not only to stock growers, but to scientific botanists as well—Professor W. J. Beal has recently published a Report of the Botanical Department of the Michigan Agricultural College from which we learn that there are in the herbarium 54,243 specimens, and that the botanic garden, begun in 1877 now contains 1335 species.—The Contributions from the U. S. National Herbarium (Vol. III, No. 9) issued August 5, 1896 contains the following papers: The Flora of Southwestern Kansas, a report on a collection of plants made by C. H. Thompson in 1893, by A. S. Hitchcock; *Crepis accidentalis* and its allies, by F. V. Coville; Plants from the Big-Horn Mountains of Wyoming, by J. N. Rose; Leibergia, a new genus of Umbelliferæ from the Columbia River Region, by J. M. Coulter and J. N. Rose; Roseanthus, a new genus of *Cucurbitaceæ* from Acapulco, Mexico, by Alfred Cogniaux.

ZOOLOGY.

Notes on Turbellaria.—1. ON THE OCCURENCE OF BIPALIUM KEWENSE (MOSELEY) IN THE UNITED STATES.

Since the appearance of Moseley's[1] paper in 1878 the species has been recorded from other parts of Great Britain and Ireland, and from Berlin and Frankfurt, A. M. on the continent. It has also been found at the Cape of Good Hope in Africa, in the colonies of Queensland, New South Wales and Victoria in Australia, at Auckland in New Zea-

[1] Moseley, H. N. Description of a New Species of Land-Planarian from the Hothouses at Kew Garden. Ann. Mag. Nat. Hist., Ser. 5. Vol. I, pp. 237–239 1878.

land, Upolu in Samoa and Joinville in Brazil. The wide distribution of this, the largest of land planarians, has doubtless been brought about through the agency of man, the well-marked genus being indigenous only in Japan, China, India, Ceylon, the Malay Archipelago and the East Indies, but this species, *Bipalium kewense*, has never been found in these countries; its home is unknown.

The purpose of this communication is to record the existence of the species in the United States. It is quite abundant in Cambridge, Mass., and has been found there in two different greenhouses. A methodical search would no doubt reveal it in others of the many greenhouses in the vicinity: The largest of the Cambridge specimens measured 300 mm. in length, with a diameter of 4 mm., shorter individuals measuring from 15 mm. upward with the same diameter of 4 mm. The smallest of the specimens always lack the semilunar head-end, they being without doubt, the products of reproduction by transverse division in which the head-end had not yet regenerated.

In 1892 Sharp[2] published the description of a Bipalium from a greenhouse in Laudsdown, Pennsylvania, which he called *B. manubriatum.* It was suggested by Colin[3] that Sharp's specimen was nothing else than *B. kewense*, for with the exception of the statement that the median stripe is the broadest of the longitudinal markings, the descriptions of *B. manubriatum* agrees in every way with that of *B. kewense.* Variations in the width of the median band in different regions of the same individual of *B. kewense* have been described and figured by Richter[4] and Bergendal,[5] and Dendy[6] has shown the great variability of land planarians within a single species both as regards color and markings. There can be little doubt, therefore, that the single specimen studied by Sharp was the *Bipalium kewense* of Moseley.

The writer would be grateful for any information as to the occurrence of the species in other parts of the United States, and would be glad to have material from other localities.

[2] Sharp, B. On a probable New Species of Bipalium. Proc. Acad. Nat. Sci. Philad., 1891, pp. 120-123, 1892.

[3] Colin, A. Mittheilungen über Würmer. Sitzungsb. Gesell. naturf. Freunde Berlin, Jahrg. 1892, No. 9, pp. 164-166.

[4] Richter, F. *Bipalium kewense* Moseley eine Landplanarie des Palmenhauses zu Frankfurt, A. M. Zool. Garten, Jahrg, XVIII, pp. 231-234, 1887.

[5] Bergendal, D. Studien über Turbellarien. I. Ueber die Vermehrung durch Quertheilung des Bipalium Kewense Moseley. Kongl. Svenska. Vetensk-Akad. Handl., Bd. XXV, No. 4, 42, pp. 1 Pl., 1892.

[6] Dendy, A. Notes on Some New and Little-known Land planarians from Tasmania and South Australia. Proc. Roy. Soc. Victoria, Vol. VI, pp. 178-188, Pl. X, 1893.

2. On the Identity of Procotyla fluviatilis Leidy and Dendrocœlum lacteum Oersted.

Procotyla fluviatilis was first described by Leidy[7] in 1852 under the name of *Dendrocœlum superbum* Girard. In Stimpson's Prodromus[8] (1858) we find for the first time the form under *Procotyla fluviatilis* Leidy M S. with the synonym *Dendrocœlum superbum* Leidy (non Girard). Stimpson's nomenclature evidently being taken from manuscript notes of Leidy, but Leidy himself did not use the name *Procotyla fluviatilis* until 1885.[9] In 1893 Girard[10] in an exhaustive paper on North American Turbellaria makes a new species out of Leidy's first description, which was not his (Girard's) *D. superbum*, calling it Procotyla Leidy (with the synonym *Dendrocœlum superbum* Leidy (non Girard), and also retains *P. fluviatilis* as a second species of Procotyla. In other words, Girard in the same work under two different names gives two different descriptions of the same species. He thus adds greatly to the confusion existing in our knowledge of North American Turbellaria. When our Turbellaria become better known there is reason to believe that the existing large list of species will be much reduced.

A careful study of the structure of *Procotyla fluviatilis* has convinced the writer that this, one of the commonest of our freshwater planarians, is identical with the widely distributed *Dendrocœlum lacteum* Oersted of Europe, and that the genus Procotyla should be abandoned. It was predicted by Hallez[11] that Procotyla would be eliminated when its internal structure should become known. The anatomy and histology of *Dendrocœlum lacteum* has been most carefully worked out by Iijima.[12] His account and figures agree in every way with the American form, as does also the older account of Oscar Schmidt.[13] The variation in

[7] Leidy, J. Corrections and Additions to former Papers on Helminthology. Proc. Acad. Nat. Sci. Philad., Vol. V, pp. 288–289, 1852.

name of *Dendrocœlum superbum* Girard. In Stimpson's[8] Prodromus

[8] Proc. Acad. Nat. Sci. Philad., Vol. IX, pp. 23, 1857.

[9] Leidy, J. Planarians. The Museum, Vol. I, No. 4, p. 5. Philadelphia, 1885.

[10] Girard, Ch. Recherches sur les Planariés et les Némertiens du l'Amérique du Nord. Ann. Sci. Nat., Zool. Tom., XV, pp. 164–166, 1893.

[11] Hallez, P. Catalogue des Turbellariés (Rhabdocœlides, Triclades et Polyclades) du Nord de le France, etc. Revue Biol. du Noru de la France, T. IV No. 11, p. 454, 1892.

[12] Iijima, I. Untersuchungen über den Bau und die Entwickelungeschichte der Süsswasser-Dendrocœlen (Trichleden). Zeitschr. wiss. Zool., Bd. XL, pp. 359-464, Taf. XX-XXIII, 1884.

[13] Schmidt, O. Unterschungen über Turbellarien von Corfu und Cephalonia, Zeitschr. wiss. Zool. Bd. XI, pp. 1–30, Taf. I--IV, 1862.

the number of the eyes in the American form appears to be peculiar, as no mention is made of it in any of the foreign descriptions. In about thirty per cent. of the individuals there are more than the normal number (two) of eyes, the number varying from three to eight, three being the number most frequently occuring.

A detailed account of this and other American Turbellaria, based upon collections made by the Illinois State Natural History Survey and submitted to the writer for study, is in course of preparation.—W. McM. Woodworth.

On the Genus Callisaurus.—Two new species of this genus present lateral fringes of the toes. These are not so well developed as in the species referred to Uma, but they are sufficiently so to show that the latter name must be abandoned, and the species referred to it be placed in Callisaurus. Thus, *Uma notata* Baird, *U. scoparia* Cope, *U. rufopunctata* Cope, etc., must be called *Callisaurus notatus*, etc. The two new species referred to are both from lower California.

Callisaurus crinitus—*Callisaurus dracontoides* Cope, Proceeds. U. S. Natl. Museum, 1889, p. 147. Two series of frontal scales, separated from the rather larger supraoculars by two (or one) rows of small scales. Large supraoculars in four or five longitudinal rows, the inner row largest, the patch bounded by granular scales anteriorly and posteriorly. Interparietal plate longer than wide. Hind leg reaching to front of orbit. Second, third and fourth fingers with well-developed fringes, which are weak on the inner side of the second and third. External side of second, third and fourth toes with well-developed fringes. Femoral pores twenty-three, the scales which they perforate in contact with each other. Color above as in *C. draconoides*. Below a blue patch on each side, with three large oblique black spots and a trace of a fourth. Total length 200 mm., head and body 87 mm., hind leg 72 mm. U. S. N. M., No. 14,895, one specimen.

The differences from *C. draconoides* are the digital fringes, the larger number of femoral pores on adjacent scales, and the three or four black spots of the belly patch; the shorter hind legs, and the longer, interparietal plate. This species has the larger size of the form *C. draconoides ventralis*.

Callisaurus rhodostictus—One row of frontal scales separated by small scales from the rather obscure patch of supraoculars. Interparietal as wide as long. Gular scales subequal. The hind leg extended, reaches to and beyond the end of the muzzle. Well developed fringes on the external sides of the fingers and toes, excepting on the

first and fifth. Femoral pores fifteen and sixteen, in scales which are separated by intervening scales. Coloration above as in *C. draconoides;* below a blue patch on each side which is crossed by three oblique black spots, the third generally followed by a fourth black spot, which does not reach the abdominal border. In front of the blue patch and posterior to the axilla a large rosy spot. A large rosy spot on the gular region. Size smaller, equal the *C. draconoides draconoides.* Numerous specimens from lower California from A. W. Anthony. As this species was accompanied by *Uta parviscutata* V. den B. and *Crotalus ruber* Cope, the locality is not the Cape San Lucas country. It approaches nearer the *C. draconoides* than does the *C. crinitus.* The differences are, the digital fringes, the three or four black abdominal spots, and the rose spots on the sides and throat.—E. D. COPE.

The Food of Birds.—A report upon the food habits of the catbird (*Galeoscoptes carolinensis*) the brown thrasher (*Harporhyncus rufus*) the mocking bird *Mimus polyglottus*) and the house wren (*Troglodytes aëdon*) by S. D. Judd, contains the following information. The wren is exclusively insectivorus, and, therefore highly beneficial to agriculture. Among the pests destroyed by this bird are the snout beetles, of which the plum curculio is a familiar example. Stink bugs and caterpillars, both of which are plant feeders, are also made way with in large numbers. The catbird and thrasher do much less good than the wren because of their mixed diet of animal and vegetable food, the proportion of the former in the thrasher being 63 per cent., that in the catbird 44, for the entire season. The number of mocking birds examined was only 15, so that their character, as friend or foe of the agriculturist, is still undetermined. The stomachs of those examined, however, indicate that the bulk of their food is animal.

Mr. Judd concludes his report by advising farmers to secure the services of the wren by putting up nesting boxes for them, and protecting them from the quarrelsome English sparrows.

A second interesting paper on the food habits of birds records the results of the examination by Mr. F. E. L. Beal of the stomachs of 238 meadow larks, and 113 Baltimore orioles. The meadow lark is a ground feeder and the great bulk of its food is grasshoppers, of which it consumes an enormous number. The other insects eaten are ants, bugs, caterpillars and beetle larvæ.

The oriole feeds largely on caterpillars and wasps, eating so many of the former that it is a highly important beneficial factor in agricultural work.

A summary of the stomach contents for the whole year shows that nearly three-fourths of the food of the meadow lark for the year, including the winter mouths, consists of insects.

The oriole has a similarly good record. The food for the whole season consisted of 83.4 per cent. of animal matter and 16.6 per cent. of vegetable matter.

These statistics show the importance of according these birds the protection they so well deserve. (Year book Dept. Agri. for 1895. Washington, 1896.

Preliminary Description of a New Vole from Labrador.—In the summer of 1895, Mr. C. H. Goldthwaite made a trip to Hamilton Inlet, Labrador, to collect mammals for the Bangs Collection. The material he got is of much interest, but as I am obliged to delay publishing a full account of it for the present, I take this opportunity of making known apparently the only new species he took—a rather remarkable vole.

MICROTUS ENIXUS sp. nov.

Eighty specimens, all taken in the immediate vicinity of Hamilton Inlet.

Type from Hamilton Inlet, Labrador.

No. 3973, ♀, old adult; collection of E. A. and O. Bangs; collected July 15, 1895, by C. H. Goldthwaite. Total length, 210; tail vertebræ, ·67; hind-foot, 22·5.

General characters: Size medium (about that of *M. pennsylvanicus*); tail long; hind-foot large and strong; colors dark with a sooty brown cast to upper parts; skull differing in many minor particulars from that of any eastern vole; molar teeth extremely small and weak, the tooth row very short; incisor teeth long and projecting.

Color: Upper parts a dark burnt umber brown, with many black-tipped hairs intermixed, and a general sooty cast; nose patch the same. Underparts dark gray (some specimens in fresh pelage slightly washed with buffy). Feet and hands dusky. Tail indistinctly bicolored, black above, dark gray beneath.

Cranial characters: Skull rather small (smaller than the skulls of examples of *M. pennsylvanicus*, the external measurements being substantially) the same; rostrum slender and straight; audital bullæ of moderate size, very round; palate without so pronounced a "step" as that of *pennsylvanicus*. Incisor teeth, both upper and under, long, slender and projecting outward at a decided angle. Molar teeth very weak and small, the tooth row averaging 1 m. shorter than in skulls of

pennsylvanicus of equal size; posterior loop of last upper molar extremely small, enamel folding otherwise much as in *pennsylvanicus.*

Size: Average measurements of ten old adult topotypes—total length 189·4; tail vertebræ, 60·4; hind-foot, 22·4.—OUTRAM BANGS.

Zoological News.—CŒLENTERATA.—Mr. Whiteaves records the finding of a second specimen of the branching Alcyonarian coral, *Primnoa reseda,* in the Pacific waters, off the coast of British Columbia. This is the third species of large Alcyonaria now known to occur in this region, viz., *Verrillia blakei* Stearns, *Paragorgia pacifica* Verrill and *Primnoa reseda* Pallas. Fine examples of each of these are in the Museum of the Geological Survey of Canada. (Trans. Roy. Soc. Canada, Vol. I, 1895–'96.)

PISCES.—A new genus (Apogonops) of fishes from Maronba Bay, New South Wales, is described by Mr. J. D. Ogilby. The genus is founded on a single specimen to which has been given the species name, *anomalus.* At first glance this genus appears to belong with the Apogonidæ, but the absence of vomerine teeth and the number of dorsal spines preclude such a classification. (Proceeds. Linn. Soc. New South Wales, 1896.)

REPTILIA.—Dr. Alfredo Dugés has recently published in La Naturaleza, a useful list of the Batrachia and Reptilia of Mexico, with the localities in which they have been found. While a good many species are omitted, the lists of localities are of much value to the student of geographical distribution.

AVES.—From personal observation M. X. Raspail finds that the time occupied by the Magpie (*Pica caudata*) in the incubation of its eggs is from 17 days to 18 days and 13 hours. The young come from the egg entirely bare, without even a trace of down, and are cared for by the parents about 25 or 26 days before they attempt to leave the nest. (Bull. Soc. Zool. de France, Juillet, 1896.)

The birds collected by Dr. A. Donaldson Smith in Somaliland contain a number of species and genera which find their closest allies in the Cape fauna. In a notice of the collection, Dr. Bowdler-Sharpe states that they are more nearly related to the birds of the Cape than to the fauna of Abyssinia or East Africa. (Geol. Journ. Sept., 1896.)

The collection of birds made by Mr. Abbott in Central Asia has been presented to the National Museum. It numbers 210 specimens, representing 97 known species, and one new to science. The collection has been catalogued by Mr. C. W. Richmond, who embodies in his paper a number of interesting notes on many of the species. (Proceeds. U. S. Natl. Mus., Vol. XVIII, 1896.)

MAMMALIA.—Dr. C. H. Merriam has recently revised the Lemmings of the genus Synaptomys, giving descriptions of three new species. He finds that this genus instead of being monotypic, comprises two well marked subgeneric groups—Synaptomys' proper and Mictomys; that the first of these groups inhabits eastern Canada and northeastern United States from Minnesota to New Brunswick, and contains four fairly well defined forms; that Mictomys has a transcontinental distribution from Labrador to Alaska, and contains at least four species. (Proceeds. Biol. Sc., Washington, Vol. X, 1896.)

ENTOMOLOGY.[1]

A New Era in the Study of Diptera.—The work done on the classification of North American Diptera falls naturally into three periods. The first ended with the publication of the "Catalogue of North American Diptera," by Osten Sacken, in 1859. The descriptive work of most value previous to this time was by Wiedmann and Say, and a little by Loew toward the last. Harris, Macquart and Walker had also published numerous species; but there had been little coöperation, and it was nearly impossible to determine from the descriptions the synonyms that had been created. Osten Sacken recognized this condition, and did not attempt to solve such problems in his catalogue.

The following nineteen years to the second edition of the catalogue in 1878 comprise the second period, characterized by the singular fact that the vast amount of work accomplished was almost wholly by Europeans. Walsh published some twenty species, Riley eight, and several others from one to four each—scarce forty in all—while Loew had in the same time performed the monumental work of describing at least 1300 North American species, Osten Sacken had added several hundred, and Schiner and Thomson a considerable number. Moreover, the new edition of the catalogue was enriched with a vast fund of information gathered by the author in the study of American types in all the principal European collections, revising the synonymy and correcting the generic references as would have been impossible in any other way. About the time of the issuance of the catalogue, the collections of Loew and Osten Sacken were deposited in the Museum of Comparative Zoology, at Cambridge, Mass. This marked the conclusion of what may well be called the Loew-Osten Sacken period. Loew died, and Osten Sacken retiring from the diplomatic service, resumed his residence in Germany. His dipterological writings since

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

1878, while very important, include only one work which describes new North American species—Vol. I of the Diptera in Biologia Centrali-Americana.

In 1879 appeared the first paper of S. W. Williston, inaugurating a new American period, which has continued to the present time. After a few years D. W. Coquillet began to publish, followed by C. H. Tyler Townsend, and he by others, until the number of those who publish occasional papers is now ten or more.

The recent appearance of Dr. Williston's Manual of North American Diptera[2] gives reason to hope that the immediate future will greatly increase the number of workers in this order, so that we will be justified in counting a new era from 1896. It is now possible to determine the genera of nearly all the flies of North America, including the West Indies, with no other work of reference than this volume. More than Cresson's Synopsis does for the Hymenoptera, or Leconte and Harris' volume does for the Coleoptera, this book does for the Diptera, because it includes the territory southward to the Isthmus of Panama. Only the Tachinidæ and Dexiidæ are not tabulated and analytically reduced to genera, and in this confused mass a bibliographical generic list is given, extending to 272 numbers.

While the book purports to be a second edition of the small one published by the same author in 1888, it is practically a new work, having been entirely rewritten, greatly enlarged, and extended to include the entire order with the exception noted. The bibliography since 1878 is given, and all genera not found in Osten Sacken's catalogue have references (in the index) to their descriptions.

The external anatomy of Diptera is very fully treated. Dr. J. B. Smith's interpretation of the mouth parts is given in addition to the usual one, the author not assuming to decide between them. Professor J. H. Comstock's system of wing nomenclature, as used in his manual, is given a place for comparison, but is not used in the work "for two reasons: First, that it has not yet been fully crystallized into permanent shape; second, because nearly all the existing literature has the nomenclature here employed, and to use a new one would largely defeat the object of the work in the hands of the beginner." Baron Osten Sacken's system of bristle-naming or chætotaxy is quite fully set forth. Each family table is preceded by a full exposition of the family characters and a description of the larva, its mode of life, food, etc. (where known).

[2] Manual of North American Diptera. By Samuel W. Williston, M. D., Ph. D. Pp. LIV, 167. James T. Hathaway, New Haven, Conn., 297 Crown St. Paper, $2.00; cloth, $2.25.

The family known heretofore as Blepharoceridæ appears as Liponeuridæ. This change of name was made by Osten Sacken several years ago. He has more recently abandoned the change in a published paper, and there seems no reason why the old name should be displaced.

The families Xylophagidæ and Cœnomyidæ are united with Leptidæ, thus simplifying the family and generic diagnoses. This seems a rather surprising arrangement, yet may be logically defended.

The family Lonchæidæ is united with the Sapromyzidæ. Aside from these changes there are no important differences in the higher categories between the last catalogue and the present work.

While the printing and binding are excellent, there are a number of typographical errors especially in the spelling of generic names, as for instance in Subulomyia, p. 43, the list of lepidopterous genera on p. 146 (five mistakes) and the list of Tachinid and Dexiid genera, p. 147 (four mistakes). But few of these, however, are more than the interchanging or omission of a letter.

This book is Dr. Williston's most important single contribution to dipterology thus far, and it worthily exhibits the industry, experience and ability of the author, which have secured for him world-wide recognition as a dipterist of the highest rank.—J. M. ALDRICH, Moscow, Idaho.

Color Variation of a Beetle.—Mr. W. Baterson gives an account of his statistical examination of the color variations of the beetle *Gonioctena variabilis*, which appears to be abundant in hilly places in the south of Spain. He finds that we have here to do with a species whose members exhibit variation in several different respects, and that the variations occur in such a way that the individuals must be conceived as grouped round several special typical forms. There is thus not one normal for the species but several, though all live in the same localities under the same conditions, and though they breed freely all together these various forms are commoner than the intermediates between them. Some time since, when calling attention to the excessive variability of the color of *Coccinella decempunctata* and the no less striking constancy of *C. septempunctata* which lives with it, Mr. Bateson remarked that to ask us to believe that the color of the one is constant, because it matters to the animal, and that the other is variable because it does not matter, is to ask us to abrogate reason. Mr. Wallace, it seems, is of this very opinion; but he does not explain how it is that the color of one is so important, and the color of the other unimportant to the beetle. (Journal Royal Microscopical Society.)

American Nematinæ.—The third of the technical series of bulletins from the U. S. Division of Entomology is entitled "Revision of the Nematinæ of North America, a Subfamily of Leaf-feeding Hymenoptera of the Family Tenthredinidæ." It is by Mr. C. L. Marlatt, and extends over 135 pages, with one excellent plate and several illustrations in the text. We quote from the introduction as follows:

"The subfamily *Nematinæ* of Thompson or *Nematina* of Cameron (Konow's subtribe *Nematides*) comprises a very large group of closely allied species, distributed in the classification adopted by the author among nearly a score of genera. They range from very small insects to medium sized, but include no very large species, or in length from 2 to 12 mm. They are for the most part smooth, shining, and rather soft bodied, and are variously colored, but yet presenting frequently a confusing similarity in general form, and particularly in coloration, rendering their generic and specific references in some cases difficult. In point of number of species and abundance of individuals this subfamily far exceeds any other of the corresponding groups in the family Tenthredinidæ, and in variation and peculiarities in larval habits and in economic importance many of the species belonging to it have a very great interest.

"*Geographical Distribution.*—The Nematinæ are distinctly northern in their range, reaching their greatest development in abundance of species and specimens in the transition and boreal zones, and extend northward into the circumpolar regions—species occurring abundantly in Greenland, Iceland, and Spitzbergen. Southward they become less and less numerous, and are particularly wanting in tropical countries. This is illustrated very forcibly in Europe by the occurrence of over 70 species of the old genus *Nematus* in Scotland (Cameron) and 95 in Sweden (Thompson) as against 12 about Naples, Italy (Costa); and the same discrepancy exists between the temperate and subarctic region of America and the Southern States and Mexico.

"*Food-plants.*—Their food-plants cover a wide range, some species affecting grasses, one or two very destructive to the grains, others various deciduous trees and shrubs, and still others conifers. The majority of the species occur, however, on plants of the families Salicaceæ, Betulaceæ, Rosaceæ, and Coniferæ, in the order given.

"*Life history and habits.*—The Nematines are among the first sawflies to appear in spring, occurring abundantly on trees on the first appearance of the leaves. They do not often frequent flowers, except, at least, those of the plants upon which their larvæ feed. Many willow species, for example, occur abundantly on the earliest spring bloom of the

willow. In common with other sawflies, however, they rarely leave their larval food-plants, and to be collected successfully a knowledge of their habits in this respect is very desirable.

"In number of broods great diversity is found, and the normal rule of most Tenthredinidæ, of a single yearly brood, is frequently deviated from. Some species are known to be limited in number of broods only by the length of the season, as, for example, *Pteronus ventralis* Say, the common willow species. Two annual generations are common, but many species are single brooded, the larvæ entering the soil or other material or remaining in their galls at the completion of growth and continuing in dormant condition until the following spring, when shortly before they emerge as perfect insects the change to the pupal condition takes place. The males normally appear a few days before the females, and the duration of the life of the adults of both sexes is short, not often exceeding a week or ten days. Of a large percentage of the species no males are known, and in the case of many species careful and repeated breeding records indicate that males are very rarely produced.

"In some species parthenogenesis is complete; that is, the eggs from unimpregnated females produce other females. In other instances of parthenogenesis, however, either males only are developed from unfertilized ova or females very rarely.

"The union of the sexes takes place very shortly after the appearance of the females, and egg deposition closely follows. The eggs are inserted either singly or a number together in the young twigs, larger veins, petioles, in the surface parenchyma, or in the edges of the leaves, the single exception being the case of the gooseberry sawfly (*Pteronus ribesii*), which merely glues its eggs to the leaf without making any incision whatever.

"Most of the species are external feeders on the foliage of plants, but the species of two genera, *Euura* and *Pontania*, so far as their habits have been studied, are gall makers, and pass their early life in the interior of the plants, either in the stems without causing abnormal growths or in the excrescences or galls on the stems and leaves. At least one American species develops in the rolled or folded edges of the leaf. The larvæ are 20-footed, some solitary, others gregarious—the latter usually more brightly colored and possessing means of protection in glands secreting a noxious fluid. Most of the solitary ones are green and not readily observed. They usually feed from the underside of the leaves, eating from the edge or cutting circular holes in the general surface, and in some cases taking everything but the stronger veins. Many species rest quietly during the day, feeding only at night.

Some have the habit of throwing the posterior segments violently upward to frighten away parasites or enemies; others adhere to the leaves or twigs by the thoracic feet only, coiling the posterior segments under the middle ones."

Entomological Notes.—Prof. F. L. Harvey monographs in an elaborate manner the Currant Fly, *Epochra canadensis*, in the report of the Maine Experiment Station.

The North American species of Nemobius are monographed by Mr. S. H. Scudder (Journ. N. Y. Ent. Soc., Sept., 1896). Several new species are described.

Mr. Alex. D. MacGillivray has recently monographed the American species of Isotoma in the Canadian Entomologist

In the check-list of the Coccidæ published by Prof. T. D. A. Cockerell, in the Bulletin of the Illinois State Laboratory of Natural History (vol. IV, pp. 318–339) 773 species are listed.

A number of new species of Scarabeidæ are described by Martin L. Linell in the Proceedings U. S. National Museum (vol. XVIII, pp. 721–731.

Prof. J. B. Smith discusses again the San José Scale (*Aspidiotus perniciosus*) in Bulletin 116 of the New Jersey Station.

"The Principal Household Insects of the United Stated" is the title of the extremely valuable and interesting Bulletin No. 4 of the Division of Entomology, U. S. Dept. of Agriculture. It was prepared by Messrs. Howard, Marlatt and Chittenden.

The Lamiinæ of North America are monographed by Messrs. C. W. Leng and John Hamilton, in the Transactions of the American Entomological Society (vol. XXIII, No. 2). In the same issue Mr. William H. Ashmead describes a large number of new parasitic Hymenoptera.

Mr. F. M. Webster discusses the Chench Bug in Bulletin 69 of the Ohio Experiment Station, and several destructive insects in Bulletin 68.

The Phylogeny of the Hymenoptera has recently been discussed by Mr. Ashmead in an interesting and authoritative paper in the Proceedings of the Entomological Society of Washington (vol. III, No. 5).

EMBRYOLOGY.[1]

Movements of Blastomeres.—In a copiously illustrated and extensive paper on the cleavage of *Ascaris megalocephala* Otto zur Strassen[2] lays special emphasis upon certain movements of the cells of the embryo.

In the living egg most remarkable rearrangements of the material are easily seen when the first four cells glide over one another. In later stages changes in form are traced to movements of the cells that must have taken place though not actually seen but inferred from a very detailed study of preserved material. The author confined his attention chiefly to the ectodermal layer of cells and knowing the pedigree of a very large number of them was able to affirm that the changes in shape that the embryo exhibits are due, in part at least, to an actual migration or rearrangement of cells. Cell division and surface tension are not the only factors concerned in this change of position of the cells; there must be some individual movement of certain cells.

This movement of the cells is regarded as being of the same nature as that observed by Roux in the isolated cells of the frog's egg and is, therefore, designated *Cytotropism*.

The production of form in the development of the Ascaris embryo has then this important factor—a power of cells to move towards one another and thus change the shape of the entire mass. This movement is in addition to any purely mechanical movements due to surface tension and is due either to attraction between cells or to repulsion between cells. In either case it is assumed that chemical influences are at work: that this movement arises from *chemotactic* strains.

The movements are much restricted in that a cell travels its own length at most and is never free from its sister-cell. In fact the two cells that arise from the division of one remain connected and are not to be separated by any intrusion of migrating cells and the author thinks that the movements are probably even more restricted in being merely the rearrangements of two groups of such sister-cells both derived from one parent, being merely readjustments of four grandchildren of one cell! The entire ectoderm may then be regarded as a mosaic of such sets of families of four, each having its own internal readjustments.

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts reviews and preliminary notes may be sent.

[2] Archiv f. Entwickelungsmechanik, 3, 1896, pps. 27–101, 133–188, Pls. V–IX.

Moving pigment in Eggs.—In a carefully illustrated account[3] of the cleavage of the Planarian, *Polychœrus caudatus* Mark, Dr. E. G. Gardiner describes most remarkable changes in position of peculiar, algalike, pigment bodies which color the eggs orange-red for a certain period. These bodies appear in certain cells and then others, they lie along the lines where cleavage is to take place.

They move up from the centre of the egg to the surface and move from place to place.

Fertilization.—By the use of nitric acid Kostanecki and Wierzejski find that the so-called achromatic substance may be demonstrated with remarkable clearness in the eggs of the pond snail *Physa fontinalis.* In a detailed description[4] of radiations, or stars, of this substance seen during the process of maturation of the egg and during fertilization, illustrated by many remarkable figures of reconstructed sections, the authors give the facts that lead them toward the following hypothetical conception of the true nature of the process of fertilization.

The object of fertilization is the union of the nuclei; but the necessary condition to make this of avail is that the egg be able to continue to divide, to undergo cleavage. This power is brought to it by the new nuclear part of the sperm.

Each sexual cell needs to be supplemented by what the other has and it itself is deficient in. This lack is in the protoplasm.

The egg has large amounts of nutritive material while the sperm has none. The former has thus relatively too little protoplasm to continue dividing by itself. During maturation, by dividing twice to form polar bodies, the egg uses up its remaining power of division and must have this added to it again if it is to cleave at all.

What the sperm brings in to replace the exhausted cleaving powers of the egg is the connecting piece of the sperm, the portion near the head or nucleus, that contains achromatic material centered on the centrosome or speck next the head. This material is the remnant from the achromatic figure of the last cell division in the formation of sperms.

This material is conceived of as concentrated and not, as yet, recognized till it gets into the egg; then it swells up and extend in radii as an umbrella unfolds. As the sperm revolves through 180° after entering the egg the middle piece preceeds the sperm head or male nucleus in its journey towards the female nucleus. The middle piece appears as a star centered about the centrosome and rapidly grows in all direction

[3] Journal of Morphology, XI, pp. 155–171.

[4] Archiv. f. Mik. Anat., 47, 2, Apl, 1896, pps. 309--379, Pls. 18--20.

by "assimilating" the net-work of the egg. Thus the star, so remarkably distinct in these snail eggs, about the centrosome of the sperm is to be regarded as at first of male origin and then as gradually getting control of the net-work protoplasm, or the archoplasm, of the egg so that it is eventually the centre of an entire rearrangement of this egg material focussed about the male centrosome.

The centrosome next the famale nucleus disappears and the star about it is "assimilated" by the star that arose about the male nucleus. Sooner or later the male star and centrosome divide to furnish the two-centered system concerned with division of the cleavage nucleus. The male and female nuclei unite to make the cleavage nucleus and the two protoplasmic stars do all that remain to be done in the subsequent cleavage.

The substitution of the new male system for the effete female system of radiate protoplasm is regarded as so complete that the chromosomes in the female nucleus become subjected to the domination of the male system by the growing male radii attaching themselves to these chromo somes by a process of "assimilation" of the old connections, that the author believes to exist between the female chromosomes and the female centrosomes. It is assumed that this male system is all along connected with the chromosomes of the sperm head and that the contractoin of the radii draw the sperm head toward the female nucleus.

Along with the reduction of the chromosomes in both egg and sperm there is probably a reduction in the mass of so-called achromatic substance so that in fertilization there may well be restitution of the normal amount by a mutual supplying of the deficiency.

It will be seen that this conception of the process of fertilization is that of Boveri except that the centrosome is regarded as of no importance and the surrounding, radiated protoplasm becomes the essential factor for cell division. The authors follow Heidenhain in regarding the centrosome as merely the point of insertion of that active, contractile part of the cell that radiates out from this centre.

PSYCHOLOGY.[1]

The Effects of Loss of Sleep.—Prof. Patrick and Dr. Gilbert, of the University of Iowa, have reported in the *Psychological Review* some experiments on this problem. Three normal subjects were kept awake for a period of ninety hours, without resort to stimulants or other

[1] Edited by H. C. Warren, Princeton University, Princeton, N. J.

physiological means. During the four days and three nights of the test they were engaged, as far as possible, in their usual occupations; their meals were of the customary kind, and were taken at the ordinary times, with the addition of a light lunch at about midnight. At intervals of six hours a series of tests was made on each subject, to determine his mental and physical condition. To eliminate the effects of practice, these tests were begun three days before the experiment. The test of the first day of experiment, before any loss of sleep had actually occurred, represent the normal condition of the subject. Tests were also made after the night's sleep that followed the conclusion of the experiment. One of the writers was the first subject. The two other subjects were instructors in the university; the latter were experimented upon at the same time.

Some of the results are of special interest. The reaction time (for sound) showed a gradual increase for two of the subjects, which was masked in the third case by increase of practice; at one period (different in the three cases) the time was considerably greater than earlier or later in the experiment; the mean variation was somewhat above the normal, but not remarkably great. The acuteness of vision, measured by the distance at which a page of print could be distinguished and read, actually increased during the progress of the experiment, and fell off again after the ensuing sleep. The memory test of the two last subjects consisted in committing random series of figures; the time required for this memorizing fluctuated considerably, with a marked lengthening towards the close of the experiment. One of the subjects was unable to memorize the figures at all at two of the last day's tests; he found it impossible to hold the attention upon the task long enough to complete it. The time consumed in adding sets of figures was fairly constant, with two or three exceptions; it was apparently independent of the memory conditions. "Voluntary motor ability," tested by the number of taps that could be made with the finger in five seconds, showed no marked alterations; neither did the susceptibility to fatigue, as tested by continuing this tapping for sixty seconds. The strength of grip, measured on the squeeze dynamometer, fell off from 20 to 30 per cent. at the end of the second day, but afterwards recovered—in two cases fully, in the other partially. The weight of the men remained fairly constant, showing a slight increase towards the close of the period, and the variation of the pulse was within the normal range of daily fluctuations.

The first subject suffered from marked visual hallucinations after the second night. "The subject complained that the floor was covered with

a greasy-looking, molecular layer of rapidly moving or oscillating particles. Often this layer was a foot above the floor and parallel with it, and caused the subject trouble in walking, as he would try to step up on it. Later the air was full of these dancing particles, which developed into swarms of little bodies like gnats, but colored red, purple or black. The subject would climb upon a chair to brush them from about the gas jet, or stealthily try to touch an imaginary fly on the table with his finger. These phenomena did not move with movements of the eye and appeared to be true hallucinations, centrally caused, but due no doubt to the long and unusual strain put upon the eyes. Meanwhile the subject's sharpness of vision was not impaired. At no other time has he had hallucinations of sight, and they entirely disappeared after sleep." Neither of the other subjects experienced these hallucinations.

At the close of the experiment the subjects were allowed to sleep as long as they desired. Tests were made upon the first subject, however, at hourly intervals during the first night, to determine the depth of his sleep. He awoke naturally after ten and a half hours, and remained awake during the rest of the day, but slept two hours more than his normal amount the second night. Of the other subjects, one awoke of his own accord after eleven, the other after fourteen hours' sleep; both felt quite refreshed; they required no extra sleep the next night, and felt no ill effects from the experiment.

It will be noticed that the sleep made up was but a small proportion of the amount lost, viz., 16, 25 and 35 per cent. in the three cases respectively. Two possible explanations for this are offered: either a greater depth of sleep may make up for a lesser duration; or sleep is a relative phenomenon, and the subjects, while apparently awake, were in reality partially asleep at times during the experiment. The authors believe that both of these facts are true, and that they operated together in the present instance. While the subjects were not allowed to go to sleep for an instant, and the slightest tendency to close the eyes was met by active measures, still there were indications of the presence of dreams, in lapses of memory and occasional irrelevant remarks. "It must be understood," say the writers, "that these dreams were instantaneous and the subject as wide awake as he could be kept; but these facts reveal a cerebral condition related to sleep. This hypothesis alone, however, would not seen to account fully for the small proportion of sleep made up. And, indeed, a study of our special tests shows that restoration took place chiefly during the profound sleep following the sleep fast, and took place rapidly. That this sleep was actually more profound, and that the profound part of it was longer than usual, was shown by our experiments in depth of sleep," on one of the subjects.

The authors think it would have been possible to prolong the experiment beyond the ninety hours without danger, except in one of the three cases. These results contrast favorably with those obtained by M. de Manacéine upon young dogs. The animals were kept from sleeping and died at the end of the fourth or fifth day.—H. C. WARREN.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

American Philosophical Society.—November 6, 1896—The following communications were made: "Recent Archæological Explorations on the Shell Keys and Gulf Coast of Florida," by Frank Hamilton Cushing, followed by Dr. D. G. Brinton and Prof. F. W. Putnam.

November 20, 1896.—Prof. H. V. Hilprecht addressed the Society on his recent archæological discoveries at Nippur, and exhibited a collection of tablets with Summerian inscriptions. A paper on "A New Physical Property of the X-Ray," by Charles L. Leonard, M. D., was read.

University of Pennsylvania, BIOLOGICAL CLUB.—November 2, 1896.—The following demonstrations were made; Descriptive Exhibitive of Streptocarpus and Ephedra by Dr. J. M. McFarlane and of Botrychium by H. C. Porter. The following communication was made; School Museums, by Mrs. L. L. W. Wilson.

H. C. PORTER, *Secretary.*

The Biological Society of Washington.—The following communications were made; Theodore Gill, "The Category of Family or Order in Biology;" C. Hart Merriam, "Notes on the Fauna of Oregon;" E. A. DeSchweinitz, "Some Methods of Generating Formaldehyde, and its Use as a Disinfectant;" C. Hart Merriam, "Supplementary Notes on Tropical American Shrews.

November 21st.—The following communications were made: G. H. Hicks, "The 'Mildews' (Erysipheæ) of Michigan;" Frederick V. Coville, "The In florescence of the Juncaceæ;" Theodor Holm, "The Alpine Flora of Pikes Peak and Grays Peak in Colorado;" C. L. Pollard, "Some Further Remarks on Briton and Brown's Illustrated Flora."

FREDERIC A. LUCAS, *Secretary.*

National Academy of Sciences.—A scientific session of the Academy was held in New York, at the Columbia University, beginning November 17, 1896, at 11 o'clock, A. M.

The following papers were read: "On Certain Positive Negative Laws in their Relation to Organic Chemistry," A. Michael; "The Jurassic Formation on the Atlantic Coast," O. C. Marsh; "The Hydrolysis of Acid Amides," Ira Remsen; "The Isomeric Chlorides of Paranitroorthosulphobenzoic Acid," Ira Remsen; "The Equations of the Forces Acting in the Flotation of Disks and Rings of Metal, with Experiments Showing the Floating of Loaded Disks and Rings of Metal on Water and on Other Liquids," Alfred M. Mayer; "On the Geographical Distribution of Batrachia and Reptilia in the Medicolumbian Region," E. D. Cope; "On the Solar Motion as a Gauge of Stellar Distances," S. Newcome; "Memoir of F. B. Meek," C. A. White; "The Evolution and Pylogeny of Gastropod Mollusca," A. E. Verrill; "On Flicker Photometers," O. N. Rood; "A New Type of Telescope Free from Secondary Color," C. S. Hastings; "A Graphical Method of Logic," C. Pearce; "On Mathematical Infinity," C. Pearce.

A reception was given to the Academy by Mrs. Henry Draper, on the evening of Wednesday, November 18.

Boston Society of Natural History.—November 4th.—The following paper was read: Prof. George Lincoln Goodale, "The Reclaiming of Deserts."

November 18th.—The following papers were read: Prof. George H. Barton, "Observations upon the Inland Ice and the Glaciers Proceeding from it in the Umanak District, Greenland;" Prof. Alfred E. Burton, "The Topographical Features of the Umanak District, Greenland. Other members of the Greenland Expedition were present, and took part in the discussion.—SAMUEL HENSHAW, *Secretary*.

The Academy of Science of St. Louis.—At the meeting of November 2, 1896, Mr. Colton Russell spoke of "What an Entomologist Can Find of Interest About St. Louis," illustrating his remarks by numerous pinned specimens of insects, giving particular attention to the butterflies, and speaking at some length of the phenomena of periodicity, migration, polymorphism, etc., as illustrated by these insects, his paper embodying the result of a large amount of field work performed during the last ten years. Resolutions were adopted opposing the passage of the antivivisection bill now before the United States Senate. Three persons were elected to active membership.

At the meeting on the evening of November 16, 1896, Dr. Charles R. Keyes, the State Geologist of Missouri, read a paper entitled, "How Shall We Subdivide the Carboniferous?" and Professor J. H. Kinealy exhibited a chart for determining the number of square feet of low-pressure steam-heating surface required to keep a room at 70° F., and gave a description of the method of making the chart. Two active members and one life-member of the Academy were elected.

WILLIAM TRELEASE, *Recording Secretary.*

New York Academy of Sciences.—November 9th.—Members of the Columbia University Expedition to Puget Sound made reports on the summer's work.

Mr. N. R. Harrington gave a short narrative of the expedition, including a description of the equipment of the laboratory, dredging, investigation and plankton collection.

In addition, he made a report on the Echinoderms, Crustacea and Annelids. Mention was made of the relation of the asymmetry in *Scutella excentrica* to its habit of burrowing and its vertical position in the sand. Abundant material, both larval and adult, of *Entoconcha* was obtained. This mollusk had been noted by Müller in 1852, and Baur in 1864, in *Synapta digitata* and by Semper in *Holothuria edulis.* The present material was found in an undetermined species of *Holothuria.* About forty species each of Crustacea, Annelids and Echinoderms have been identified.

Mr. Bradney B. Griffin presented the following report on the Platodes, Nemerteans and Mollusks:

The Platodes and Gephyrea are relatively scarce. They are represented solely by two Dendrocoels, and one Phymosoma respectively. The nemertines occur very abundantly, fully fifteen different species were obtained, most of which appear to be undescribed, though some seem to approach more or less closely the European forms rather than those of the east coast of America. The European species are the more numerous.

The Molluscan fauna is very rich and varied, ninety-three species of sixty-nine genera were collected. These include among others the large *Cryptochiton stellerii* which, when alive and expanded measures over 20 cm., besides numerous smaller species of Mopalia, Katherina, Tonicella, etc., that occur in vast numbers on rocks and piles between tides. The Nudibranchs are notable from their bright colors and large size. One species of *Dendronotus* attains a length of over 25 cm. Cases of color variation (*Cardium* and *Acmaea*) and color series (*Littorina*) were to be met with, as well as color harmonization; many

Chitons and Limpets are colored so as to more or less resemble the speckled and barnacled rocks upon which they occur. A complete series of *Pholadidea penita* (the "boring clam") was obtained, which shows the gradual atrophy of the foot and concrescence of the mantle edges as the adult condition is attained. Specimens of *Zirphæa crispata* were collected, a related form in which the foot remains functional throughout life. A series of maturation and fertilization stages of this form was obtained. *Lepton* is not uncommon, a Lamellibranch that lives with commensal attached by its byssus to the abdomen of the Crustacean *Gebia*, and has caused the atrophy of the first pair of abdominal appendages of its host. It has developed a median furrow on each valve in adaptation to the body form of Gebia. An interesting case was observed in which an otherwise nearly smooth *Placuanomia* shell had assumed during its growth the concentric raised lines of a *Saxidomus* valve upon which it was attached.

The insects are not very abundant, they are represented in the collection mainly by a few wood beetles, myriopods (*Julus, Polydesmus*), and a species of *Termes*.

Mr. Calkins reported on the Protozoa and Coelenterates of Puget Sound and of the Alaskan Bays.

The Protozoa and Coelenterates collected during the summer by Mr. Calkins belong chiefly to the group Flagellata for the former, and to the Leptomedusæ for the latter. In addition, there are nine species of hydroids—a large number, considering the very limited representation of this group in the western waters. Twelve or fourteen species of Actinians and about the same number of sponges, and several Scyphomedusae complete the list of Coelenterates.

Mr. Bashford Dean reported on the Chordates and Protochordates of the Collection. The Ascidians are represented by about a dozen species, Fishes by upwards of forty. The most important part of his work had been the collecting of embryos and larvæ of Chimaera (*Hydrolagus colliei*) and a fairly complete series of embryos of Bdellostoma, including upwards of 20 stages from cleavage to hatching. Of Chimaera, upward of eighty egg cases had been dredged in a single day; but in every case these were found to be empty. The eggs were finally obtained at Pacific Grove, California, from the female, and were incubated in submerged cages. It was in this locality that the eggs of Bdellostoma were collected.

C. L. BRISTOL, *Secretary.*

SCIENTIFIC NEWS.

The Hindshaw Natural History Expedition returned to the University of Washington at Seattle, June 15, 1896 from the eastern part of State. The party consisted of Henry H. Hindshaw, Curator of the Museum, Mrs. Hindshaw, and Trevor C. D. Kincaid, Laboratory Assistant in Biology. Transportation was secured for the members of the expedition from the Northern Pacific Railroad, through Surgeon F. H. Coe of Seattle. The party made headquarters at Pasco, where they proceeded to collect a fine lot of plants not found in other parts of the state. In all there were acquired several hundred specimens, covering 150 species. Arrangements were then made for an exploration for Indian relics up the Snake river; and in the meantime Mr. and Mrs. Hindshaw proceeded to the sand hills of Douglas county. These were reached by a trip to Ritzville, thence to Hatton, and from there a drive to the hills of about sixteen miles. Mr. Hindshaw reports some interesting geological facts concerning these sand-hills, or dunes. By an examination of the surrounding blanket or cover of basalt; he concludes that the area covered by these dunes was left uncovered by the general flow of lava making the basalt. This deposit of sand is the layer known as the "John Day" bed. It is the water-bearing rock of Eastern Washington. Farmers come from miles to these sand-hills, where they get plenty of water with little digging. Away from the dunes artesian wells have been sunk. Water is obtained, but sometimes it is necessary to bore through hundreds of basalt to reach the "John Day," or water-bearing, rock. The "John Day" beds carry most interesting fossil bones. Mr. Hindshaw brought home many teeth and bones of the fossil rhinoceros and horse, the latter probably the three-toed ancestor of the present horse. These bones have been worn in the waves of moving sand as badly or worse than is a shell battered by the waves of the ocean. Only the hardest parts of the bones remain, but these are of great interest until further explorations yield more perfect skeletens of these prehistoric mammal remains. After making a thorough search of these sand-hills and procuring all the fossil bones in sight, the party returned to Pasco. Here Mr. D. A. Owen, an enthusiastic collector of Indian curios, had perfected arrangements for a trip up Snake river to Page's ferry and on to the deserted cattle-ranch known as McCoy's. These places were evidently the camping grounds of great bands or villages of Indians before the arrival of the white

man. Many of the old graves are cut out and washed away by the river, but the expedition succeeded in obtaining many valuable specimens, such as stone mortars, pestles, hammers, skin-scrapers, arrow- and spear-points, all of different sizes and shapes. At one place was found a large stone anvil, around which were many fragments of flint and basalt and half-formed arrow-points, showing the remains of a genuine Indian weapon factory. A number of Indian skulls were also obtained. Mingled with these remains and old stone implements, were old brass buttons, blue beads, and an old iron adz, showing that the time of the making of the graves and caches was about the time of the Hudson Bay Company's occupation of the territory in the early part of this century.

Mr. Kincaid secured about 3,000 specimens of insects, comprising about 300 species, which will make a valuable addition to the University's entomological collections.

Thus far the expenses of these collecting expeditions have been borne by individuals, though the University gets the full benefit of the work, and the entire collections.

Other expeditions are planned for the summer months in the various fields of natural science.